# Tie-Breaking Power in Committees*

Dura-Georg Granić◇ Alexander K. Wagner▲

October 2017

**Abstract**

We investigate the impact of asymmetric tie-breaking power on voting in a committee with divergent preferences. Theoretically, the formal right to break ties does not confer influence over outcomes under rational strategic voting. Testing equilibrium predictions in two experiments, we find a large effect in favor of the agent holding tie-breaking power. The common supposition that tie-breaking power is irrelevant for committee decisions, because ties occur rarely, is refuted by our findings. Outcomes to the benefit of the agent with tie-breaking power, or with small additional power more generally, are realized as soon as voting deviates from full strategic sophistication.

*JEL-classification:* C91, C92, D71, D72

*Keywords:* Committee, tie-breaking rule, asymmetric power, strategic sophistication

* *First version: May, 2015.* We are grateful to Carlos Alós-Ferrer, Steven J. Brams, John Duffy, Holger Herz, Dennis C. Mueller, Karine Van der Straeten, Juha Tolvanen, James Tremewan, and Jean-Robert Tyran for helpful comments. We also thank seminar participants at the University of Cologne, Erasmus University Rotterdam, University of Vienna, as well as conference participants at the 9th AEW in Barcelona, the 2nd IMEBESS in Toulouse, the FPRWS Workshop in Vienna, the NOeG-SEA Conference in Bratislava, the GAMES 2016 in Maastricht, and the 11th NCBEE in Oslo for useful comments. The first draft was written while Granić was visiting New York University, whose hospitality is gratefully acknowledged. Granić also gratefully acknowledges financial support from the German Research Foundation (DFG) through research project AL-1169/2-1 and from the Netherlands Organisation for Scientific Research through VIDI project 452-13-013. Wagner gratefully acknowledges financial support from the German Research Foundation (DFG) through research fellowships WA3559/1-1 and WA3559/2-1.

◇ Department of Applied Economics, Erasmus School of Economics, Erasmus University Rotterdam, PO Box 1739, 3000 DR Rotterdam, The Netherlands. Email: granic@ese.eur.nl

▲ Vienna Center for Experimental Economics and Department of Economics, University of Vienna, Oskar-Morgenstern-Platz 1, A-1090 Vienna, Austria. Email: alexander.k.wagner@univie.ac.at

# 1 Introduction

The power to control outcomes in a committee is rarely distributed equally among its members.[1] Despite its ubiquity in real-world committees, the consequences of small voting rule asymmetries on voting behavior in political committees, or boards of directors in cooperations, have so far received little attention in the literature. In this article, we offer the first investigation of the behavioral effects of asymmetric tie-breaking power on decision making in committees.

In the parsimonious voting model we consider, committee members have divergent preferences and decide by simple plurality voting which of the three possible alternatives to implement. Each committee member holds a regular vote and the regular vote of one member, called the chair, serves in addition as the casting vote in case of a deadlock. The type of tie-breaking rule we consider is used in many small-sized committees, including various Constitutional Courts in Europe (e.g. France, Italy, Spain), the International Court of Justice of the United Nations in The Hague, committees of the Swiss parliament (National Council), among others.[2]

Results from two controlled laboratory experiments demonstrate that the common wisdom that tie-breaking power is irrelevant for committee decision making, because ties are rare events, is not correct. We show theoretically and confirm experimentally that the chair benefits from the asymmetric tie-breaking rule, which is at odds with equilibrium predictions under full rationality. From the perspective of institutional design, our results suggest that it

[1] There exist a number of definitions of power, or authority, in the social sciences (e.g. de la Boétie, 1975; Russel, 1938; Weber, 1978; Simon, 1951; Bartlett, 1989; Aghion and Tirole, 1997). In the economics literature, voting power has been studied in diverse settings, ranging from conditions for *a priori* voting power in cooperative frameworks (e.g. power indices of Penrose, 1946; Shapley and Shubik, 1954; Banzhaf, 1965; Karos and Peters, forthcoming) to the analysis of strategic consequences in non-cooperative frameworks, a prominent example of which is the literature on agenda-setting power (Plott and Levine, 1978; Eckel and Holt, 1989; Bernheim et al., 2006; Apesteguia et al., 2014). Further examples of research in which power differences arise in non-cooperative voting frameworks, more or less explicitly, are studies on bargaining in legislatures (Baron and Ferejohn, 1989; Fréchette et al., 2003, 2005a; Snyder et al., 2005; Ali et al., 2014; Tremewan and Vanberg, 2016; de Groot Ruiz et al., 2016), weighted voting in legislatures (Snyder et al., 2005; Vespa, 2016), veto power (Winter, 1996; Kagel et al., 2010; Bouton et al., forthcoming), committee enlargment (Brams and Affuso, 1976; Montero et al., 2008; Drouvelis et al., 2010), and vote trading (Casella and Turban, 2014; Casella et al., 2012, 2014).

[2] It is innocuous in our setting whether the casting vote comes in form of an additional vote or implements the choice of the regular vote as long as the standard assumption of admissibility is satisfied (see Section 2.1).

is not warranted to assign tie-breaking rules based on arbitrary criteria. The reason is that small rule asymmetries can lead to systematic distortions in decisions reached by committees if its members deviate from fully rational and strategic behavior.

In the analysis of the committee model, we distinguish between formal power as the decision right to break ties and real power as the effective control over the outcome in the committee. The voting model delivers clear-cut predictions for when formal and real power coincide or differ. In the benchmark case of rational strategic voting, formal power to break ties confers no real power and implements the worst alternative of the chair (cf. Farquharson, 1969). Real power of the chair however increases in the degree of limited strategic sophistication of voting (as formalized in models of level-$k$ and in cognitive hierarchy theory, see Stahl and Wilson, 1995; Nagel, 1995; Camerer et al., 2004; Alaoui and Penta, 2016).

Moving from a symmetric random tie-breaking rule to an asymmetric one, the chair benefits from this change in the mapping from votes into outcomes for two different reasons, but both depend on limited strategic sophistication of voters. One is the *direct effect* of breaking ties which becomes decisive only in case of a deadlock. The other is the *indirect effect* of holding tie-breaking power, which refers to all events where the chair's preferred alternative is implemented without wielding tie-breaking power. Although a priori equal for all members, the indirect effect turns out to systematically benefit the chair if the assumption of full strategic sophistication of committee members is relaxed. Experimental voting behavior shows that small tie-breaking asymmetries lead to a significant advantage in terms of payoffs for the chair of the committee. The chair benefits to a much larger extent from the indirect effect of holding tie-breaking power than from breaking ties directly.

In Experiment 1, we vary the size of the indirect effect by manipulating experimentally the chair's legitimacy of holding tie-breaking power. Results show that legitimate formal power changes the indirect effect, as intended, and lures members into voting for the chair's preferred alternative, even against strong monetary incentives.[3] Robustness tests in Experiment 2 find

[3] Silverman et al. (2014), for instance, show that experimental behavior of group members in a public goods game with punishment depends strongly on the perceived legitimacy of the chair. Our findings in a committee with divergent preferences also adds to previous results showing that formal power or authority can trigger obedient, compliant, and conformist behavior in others (e.g. Kelman, 1958; Milgram, 1963; Tyler, 2006).

that the size of the indirect effect cannot be explained by the focality of the chair's preferred alternative. Overall, our level-$k$ interpretation of the data accommodates frequently observed non-equilibrium strategy profiles favorable to the chair. These profiles realize because regular members exhibit low levels of strategic reasoning, which is supported by an exogenously elicited measure of strategic sophistication of committee members.

The most-closely related work, and the only one we are aware of that accounts explicitly for asymmetric tie-breaking power, is Blinder and Morgan (2005, 2008). These papers investigate experimentally how leadership of the chair in monetary policy committees affects outcomes in a common-interest setting, whereas we study a situation of conflict in the committee. The authors find no evidence for a leadership effect of the chair and conclude that "while giving the leader the tie-breaking vote allowed him or her to influence the group's decisions *in principle*, it may not have done so in *practice* – where tie votes were very rare" (Blinder and Morgan, 2008, p. 228, emphasis in the original). Our main contribution is to show that the advantage from holding formal tie-breaking power in the committee lies not in the direct but the indirect effect if members deviate from fully rational voting.

The advantageous indirect effect of tie-breaking power is not restricted to our particular tie-breaking rule but exists for small asymmetries in voting rules more generally. Our insights are for example applicable to weighted voting models. There, the tie-breaking rule of the chair is theoretically equivalent to a voting game with small asymmetric voting weights.[4] Our approach also adds micro-foundations to an increasing empirical literature studying the effects of often subtle asymmetries in power, authority, or leadership in committees (e.g. Berry and Fowler, 2015, 2016).[5] Lastly, our findings also highlight the need to design institutions robust to boundedly-rational voting behavior. Our behavioral analysis is a first step in this

[4] Nominal asymmetries in voting weights have been addressed for example in the sequential-move Baron-Ferejohn model of legislative bargaining, see Ansolabehere et al. (2005) and Snyder et al. (2005). Fréchette et al. (2005b) find no effect of purely formal differences in bargaining power in an experimental study of the Baron-Ferejohn model while Maaser et al. (2016) do.

[5] In addition, the work of Blinder (2004, 2007) and Blinder and Morgan (2005, 2008) triggered a series of important empirical work estimating the impact of institutional rules on committee decision making in Monetary Policy Committees (see, e.g. Riboni and Ruge-Murcia, 2010; Hansen et al., 2014). Chappell et al. (2005, 2014) identify differences in power of different chairs in the Federal Reserve's Federal Open Market Committee based on historical meeting records, and respectively between Monetary Policy Committees in the United Kingdom and Sweden.

direction. We provide robust predictions about committee decision making in the presence of voting rule asymmetries which depend on the level of strategic sophistication, or iterated reasoning, of committee members.[6]

## 2 Tie-breaking power and committee voting

We present the setup of the committee voting model and derive its main behavioral predictions. Different assumptions on committee members' level of strategic sophistication allow us to characterize conditions under which formal tie-breaking power implies real power over committee outcomes. We then summarize the main treatment manipulations and hypotheses regarding voting behavior under limited strategic sophistication.

### 2.1 Behavior under (limited) strategic voting

Following the setup of Farquharson (1969), we consider a committee of three members that decides to implement one of the three available alternatives ($A$, $B$, or $C$). Committee members' preferences over alternatives, summarized in Table 1, are publicly known: $A \succ B \succ C$ for the chair, $C \succ A \succ B$ for player 2, and $B \succ C \succ A$ for player 3, with $\succ$ denoting the strict preference relation. Players vote simultaneously and independently for one of the alternatives. The winner is determined by plurality voting with one important qualification: in case of a tie among alternatives, the tie is broken according to the alternative the chair has voted for with her regular vote. The chair's real power is measured by the likelihood with which the committee implements the chair's most-preferred alternative.[7] The structure of the voting model allows us to parsimoniously relate the real power of holding the tie-breaking rule to the level of strategic sophistication of committee members. We show next why the chair's

[6] The two most prominent models of limited strategic sophistication in voting behavior build on players' iterated reasoning processes (Stahl and Wilson, 1995; Ho et al., 1998; Costa-Gomes et al., 2001) or on players making mistakes in best responding to strategies of others as formalized in quantal response equilibrium (McKelvey and Palfrey, 1995, 1998; Goeree et al., 2016). Based on the latter, Tyszler and Schram (2016) for example analyze behavior in a voting model with a similar preference structure as in our study.

[7] While we confine our analysis to tie-breaking power, it is worth noting that the model setup is formally equivalent to alternative conceptualizations of small rule asymmetries. One example is weighted voting that endows the chair with an additional, arbitrarily small, voting weight of $0 < \epsilon < 1$ on top of the normalized weight of 1 of regular committee members.

real influence is highest under sincere voting and lowest under full strategic sophistication in the committee.

*Sincere voting.* Under sincere voting, committee members neglect strategic motives and simply vote for their most-preferred alternative. Formally, let $s = (s_{chair}, s_2, s_3)$ denote a pure strategy profile with $s_i \in S_i = \{A, B, C\}$ for each player $i \in \{chair, 2, 3\}$. Under sincere voting, $s = (A, C, B)$, the committee arrives at a three-way tie and $A$ is implemented through the chair's tie-breaking power. Holding power is highly beneficial under sincere voting and the committee implements the chair's most-preferred alternative with probability 1.

*Strategic voting.* It is common in the voting literature to apply refinements to the Nash equilibrium solution concept as issues with multiple equilibria are bound to ensue. With $N \geq 3$ voters and plurality voting, all unanimous voting profiles constitute Nash equilibria of the game, irrespective of the tie-breaking rule. We follow two requirements based on admissibility, namely elimination of weakly dominated strategies (WDS) and iterated elimination of weakly dominated strategies (IEWDS) (e.g. Moulin, 1979; Kohlberg and Mertens, 1986; Dhillon and Lockwood, 2004). The first step of sophistication deletes all weakly dominated strategies. In the game we consider, alternative $B$ (respectively $A$) is weakly dominated for player 2 (respectively player 3). For the chair, $A$ is the only weakly undominated alternative. This eliminates all unanimous Nash equilibria. The only two pure-strategy equilibria that survive elimination of WDS are $(A, A, B)$ and $(A, C, C)$. In $(A, A, B)$, player 2 abandons her most-preferred alternative to follow the chair and the committee implements the chair's most-preferred alternative. In $(A, C, C)$, the committee implements the chair's least-preferred alternative. The second step of sophistication eliminates strategies $A$ and $B$ for player 2 and 3, respectively. The only Nash equilibrium which survives IEWDS is $(A, C, C)$. This result is known as the 'paradox of the chairman's vote' (Farquharson, 1969, p.51) as it leads to the chair's least-preferred alternative being implemented by the committee.

*Limited strategic reasoning.* Closely related to the IEWDS process are behavioral models of strategic reasoning (Nagel, 1995; Stahl and Wilson, 1995; Costa-Gomes et al., 2001; Camerer et al., 2004). We follow the standard level-$k$ model and assume that non-strategic $L0$ players

Table 1: Preference profile and theoretical predictions in the game.

| | | Nash equilibria | | | | | | Level-$k$ predictions | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Preference profile | Unanimous | | | WDS | | IEWDS | $L0$ | $L1$ | $L2$ |
| Chair | $A \succ B \succ C$ | $A$ | $B$ | $C$ | $A$ | $A$ | $A$ | $A$ | $A$ | $A$ |
| Player 2 | $C \succ A \succ B$ | $A$ | $B$ | $C$ | $A$ | $C$ | $C$ | $C$ | $\{A, C\}$ | $C$ |
| Player 3 | $B \succ C \succ A$ | $A$ | $B$ | $C$ | $B$ | $C$ | $C$ | $B$ | $C$ | $C$ |

vote sincerely.[8] A chair of type $L1$ believes that the two other voters will vote for $C$ and $B$ respectively, hence her unique best response is $A$. Similarly, a $L1$ player 3 believes that the chair votes for $A$ and that player 2 chooses $C$ and so her unique best response is $C$. Player 2 of type $L1$ is indifferent between action $A$ or $C$ and mixes between the two. Depending on the realization of the properly mixed strategy of player 2, $L1$ behavior hence leads either to $(A, C, C)$ or to the non-equilibrium profile $(A, A, C)$. It is instructive to observe that the $L1$ prediction for player 2 coincides with the one under trembling-hand perfection (Selten, 1975), where player 2 'trembles' on $A$ with some small probability instead of choosing $C$. For a level of $k \geq 2$, the unique strategy predicted by the level-$k$ model is the equilibrium strategy profile $(A, C, C)$. Put differently, limited strategic sophistication is a necessary condition for the chair's formal tie-breaking power to have real consequences in the committee.

## 2.2 Research question and empirical strategy

Our main research question concerns the impact of the chair's formal tie-breaking power on committee decisions, allowing for varying degrees of limited strategic sophistication of committee members. In our basic assessment of the chair's tie-breaking power (and its direct and indirect effect) we compare the outcomes implemented by the experimental committees

[8] We adopt the level-$k$ model of Nagel (1995) and Costa-Gomes and Crawford (2006) in which a player of level $k$ believes that all other players are of level $(k-1)$. Behavior is defined by a simple process of iterative best-responses: $L1$ players are best responding to $L0$ behavior, ..., and $Lk$ are best responding to $L(k-1)$. We assume that $L0$ players act strategically naive which translates to assuming sincere voting (e.g. Bassi, 2015). The level-$k$ results hold under weak assumptions regarding $L0$ behavior. Following the standard approach in the literature on normal-form games by letting $L0$ players randomize uniformly over the set of pure strategies would neither change its prediction nor interpretation. It would only shift the level-$k$ strategies upwards by one level such that the new $L1$ profile coincides with our $L0$ profile, and so on (see Table 1).

to the theoretical benchmark predictions obtained from the same game with a symmetric random tie-breaking rule (in which each alternative wins with an expected probability of 1/3). First, this is the correct counterfactual to our asymmetric tie-breaking rule. Second, this approach is much more stringent than testing against the predictions from the game with asymmetric tie-breaking and fully strategic voting (in which the expected probability of winning is 0 for alternative A).

Note that we chose not to compare behavior in the game with asymmetric tie-breaking to experimental behavior from a treatment with random tie-breaking. The set of equilibria in the game with random tie-breaking differs from the one with asymmetric tie-breaking (see Section 2.1). In the former there is an additional pure strategy Nash equilibrium $(B, C, B)$, surviving deletion of WDS, which can lead to Condorcet cycles. Furthermore, committee members' risk preferences can influence behavior because ties lead to lotteries over which alternative is implemented. These confounding factors make it unfeasible to relate potential differences in behavior between the two games to differences in strategic sophistication – the key component for the indirect effect of tie-breaking power to occur.

We provide causal evidence on the underlying determinants of the chair's real power through a number of treatment comparisons. In Experiment 1, we vary exogenously the size of the indirect effect of tie-breaking power. To this end, we assign tie-breaking power randomly in one treatment and based on performance in an unrelated real-effort task (cf. Erkal et al., 2011) in another treatment. In the performance treatment, the chair position thus comes with a natural notion of legitimacy, or authority. Legitimacy is a vital element of how people react to formal power and relevant in many real-world committees (e.g. Blinder and Morgan, 2005, 2008; Silverman et al., 2014). Assuming that performance in the real-effort task 'legitimizes' the chair to hold the formal right to break ties, our hypothesis is that regular members are swayed into voting more often for the chair's preferred alternative $A$ in the performance treatment. Put differently, the manipulation should lead to a larger indirect effect of holding tie-breaking power. Because the performance-based allocation takes places only once, and before the start of the first voting game, we do not expect that the impact on indirect effect of tie-breaking power is equally strong over all periods.

In Experiment 2, we establish the general robustness of the results of Experiment 1 and control explicitly for the possibility that real power of the chair may be partly due to the salience of the chair label (used in Experiment 1). In the chair-label treatment the player holding tie-breaking power is referred to as the 'chair', while she is referred to as another regular 'member' in the neutral-label treatment. Finally, we test our explanation of limited strategic sophistication as the source of the chair's advantage by relating behavior of committee members to a measure of strategic reasoning elicited in an independent $p$-beauty contest.

# 3 Experiment 1

In Experiment 1, we investigate the influence of asymmetric tie-breaking power of the chair on decision-making in committees. In addition, the main treatment manipulates experimentally the (revealed) level of strategic sophistication of committee members, providing evidence that the real power of the chair depends on the level of strategic voting of committee members.

## 3.1 Design

The experiment consisted of two parts. Participants engaged in a real-effort task and then made decisions in a committee by voting. Upon arrival, they were randomly allocated to isolated working stations and printed instructions (see Appendix B) explained all procedures and parts of the experiment. The experiment started after all control questions were answered correctly. The course of the experiment is summarized below.

*Real-effort task.* We employed the word encoding paradigm of Erkal et al. (2011). Participant's were presented onscreen with words (e.g. fast, hyper,...) and asked to replace letters with numbers from a cipher table for 7 minutes. The encoding table bijectively maps the alphabet's letters into the numbers 1 to 26 (in random order). The ex-ante probability of becoming chair increased with performance, defined as the number of correctly encoded words. The tournament design elicited participants' willingness to become the committee's chair without introducing income effects. It also allowed us to introduce legitimate tie-breaking power as a treatment variable by conditioning the assignment of the chair role on performance. After the

Table 2: Summary of treatments.

| | Chair | | |
|---|---|---|---|
| | assignment | tournament | label |
| Experiment 1 | *performance* | yes | yes |
| | *random* | yes | yes |
| Experiment 2 | random | no | *yes* |
| | random | no | *no* |

task, participants indicated their willingness to become chair in the committee, referred to as WTP1, on a 10-point Likert scale. We introduced this question to augment the real-effort task as the task does not control for participants' opportunity cost of exerting effort. Finally, we elicited participants' beliefs about the likelihood that an election would result in a tie, which reflects the belief about the decisiveness of tie-breaking power.[9]

*Treatments.* Using a between-subject design, we varied the allocation mechanism for the chair role. In the *random* treatment, participants were randomly distributed to player roles. In the *performance* treatment, the chair role was assigned according to performance in the real effort task. Specifically, the top 1/3 performers within this treatment group were assigned the chair role while the remaining player roles were distributed randomly. The ex-ante probability of being allocated to either treatment was identical for participants in a session. Half of the participants in a session were assigned to the random treatment while the remaining half was assigned to the performance treatment (see Dal Bó et al., 2010, for a similar design). The treatment was revealed only after completion of the effort task. Participants then received feedback about own performance and were assigned their player roles, which remained fixed throughout the experiment. The design allows us to control for the level of effort exerted and thus ensures full comparability of behavior between treatments. Although the chair allocation involved an element of competition, the instructions reminded participants to engage in the effort task only if they wanted to become chair. Table 2 provides a summary of the treatments.

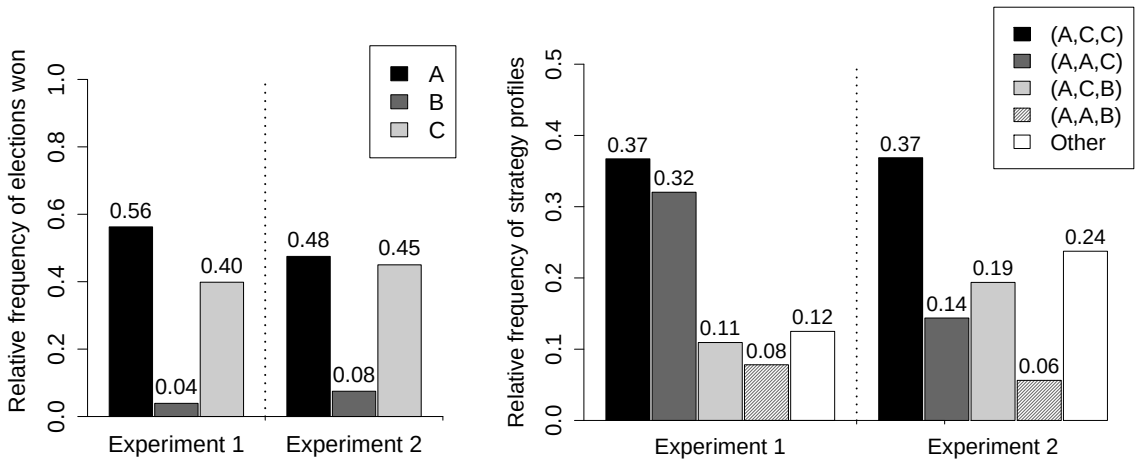
[9] Our main focus is on behavioral data and an incentivized belief elicitation mechanism would have increased the complexity, weakened monetary incentives in the game, and may have created hedging possibilities between experimental parts (Blanco et al., 2010). For a general discussion of the respective advantages of incentivized and non-incentivized belief elicitation, see Schlag et al. (2014) or Trautmann and van de Kuilen (2015).

*Committee game.* Participants played the voting game described in Section 2.1 for 4 periods under perfect-stranger matching. Alternatives were labeled neutrally ($A$, $B$, or $C$) and shuffled at the matching group level to minimize labeling effects. The three committee members voted simultaneously and independently. The winning alternative was determined according to plurality voting and, in case of a tie, by the chair's regular vote. Preferences over alternatives in the elections were induced by monetary incentives. We used four different sets of payments which induced strict preferences over the set of alternatives depicted in Table 1. Underlying payoff schedules shared the same ordinal payoff structure: $(17 \succ 12 \succ 7)$, $(16 \succ 11 \succ 6)$, $(14 \succ 9 \succ 4)$, and $(13 \succ 8 \succ 3)$ where numbers denote the EURO payoff a player received in case her most-preferred, second most-preferred, or least-preferred alternative won the election. Each payoff schedule was used in exactly one period and the order of presentation was randomized. At the end of a period, participants received feedback about the election outcome. Participants were also informed that only one randomly selected election is used for payment. All these measures were taken to preserve the one-shot character of the game.

*Procedural details.* The experiment was conducted at the experimental economics laboratory (Lakelab) of the University of Konstanz. We recruited 96 participants (48 females, average age 23) from a student pool using the online recruitment system ORSEE. All parts of the experiment were run with the software z-Tree (Fischbacher, 2007). Each of the 4 sessions we run was comprised of 24 subjects distributed in 2 independent matching groups, one per treatment. We thus collected data on 4 independent matching groups per treatment in total. A session lasted approximately 70 minutes, including payment. The average earnings from the game were about €12. Participants were paid a show-up fee of €2 on top of their earnings from one randomly selected election at the end of the experiment.

### 3.2 Results

Recall that alternative $A$ is the chair's preferred alternative, $B$ and $C$ are the preferred alternatives of player 3 and player 2, respectively. The left part in Figure 1(a) shows the relative frequency of elections won for each of the three alternatives pooled over treatments in Experiment 1. In stark contrast to the theoretical predictions under rational strategic voting,




(a) Outcomes

(b) Strategy profiles

Figure 1: Election outcomes and strategy profiles over all periods (pooled over treatments).

which see alternative $C$ win 100% of the elections, alternative $A$ was the most frequently implemented outcome. In grand total, $A$ won 56% of all elections in Experiment 1 whereas $C$ won 40%.

The winning frequency of $A$ was 72% in the first period, and then dropped to a constant level of around 50% for the remaining periods (50%, 47%, and 56% in period 2 to 4). The data showed an analogous trend for $C$'s winning frequencies, which increased from 25% in period 1 to 47%, 44%, and 44% in the remaining periods. Alternative $A$ was thus also the most frequently implemented alternative within each single period of the experiment.[10]

Two exact binomial tests run at the level of independent matching groups cofirm that the winning frequency of $A$ was above the benchmark of 1/3 which one would expect if ties were broken randomly. Over all periods, we counted 8 out of 8 matching groups that saw $A$ win more than 1/3 of the elections, and 7 out 8 of such groups in the last period only. Assuming a winning frequency for $A$ of 1/3 under the null, the probability to observe our data or more extreme ones is lower than 0.003 in both cases. In line with our theoretical

[10] Differences between the first-period data and the remaining periods of the experiment are mainly driven by the treatment effect. Player 2 showed a higher inclination to vote for $A$ in the first period of the *performance* treatment, a result we will discuss more thoroughly in our analysis on player 2's behavior (see Result 3).

Table 3: Relative frequency of observed strategy profiles over all periods.

| | $(A,C,C)$ | $(A,A,C)$ | $(A,C,B)$ | $(A,A,B)$ | $(A,C,A)$ | $(B,C,C)$ | Other |
|---|---|---|---|---|---|---|---|
| *Experiment 1* | | | | | | | |
| Performance | 0.328 | 0.359 | 0.078 | 0.109 | 0.031 | 0.016 | 0.079 |
| Random | 0.406 | 0.281 | 0141 | 0.047 | 0.047 | 0.047 | 0.031 |
| Pooled | 0.367 | 0.320 | 0.109 | 0.078 | 0.039 | 0.031 | 0.056 |
| *Experiment 2* | | | | | | | |
| Chair-label | 0.388 | 0.138 | 0.200 | 0.038 | 0.075 | 0.062 | 0.099 |
| Neutral-label | 0.350 | 0.150 | 0.188 | 0.075 | 0.025 | 0.062 | 0.150 |
| Pooled | 0.369 | 0.144 | 0.194 | 0.056 | 0.050 | 0.062 | 0.125 |

considerations, $B$ was empirically irrelevant and won a mere 4% of the elections. The high frequency of $A$ outcomes also translated into the chair's payoff from the game. Average earnings of the chair were with €10.82 substantially higher than the expected €5 under asymmetric tie-breaking power and fully strategic voting, and weakly significantly higher than the expected €10 under random tie-breaking (two-sided Wilcoxon signed-rank (WSR) test, $N = 32$, $z = 1.89$, $p = 0.069$).

The left part in Figure 1(b) shows the most-frequently observed strategy profiles in Experiment 1, again pooled over treatments. Only a small number of implementations of $A$ were due to the *direct effect* of wielding tie-breaking power. The chair broke a three-way tie in favor of $A$ in only 12% of the elections, all of which but one occurrence stemming from the profile $(A,C,B)$. The majority of cases in which committees implemented $A$ were linked to *indirect effects* of holding tie-breaking power. $A$ was implemented mostly through profiles in which player 2 voted for the chair's most-preferred alternative $A$. These profiles were $(A,A,C)$ and $(A,A,B)$ which occurred in 32% and 8% of all elections, respectively. In either case, player 2 would have been weakly better off by voting for her most-preferred alternative, $C$. Even in the last period of the experiment, a vast majority of 77% of implementations underlying $A$ were the consequence of *indirect effects* of holding tie-breaking power. Finally, $(A,C,C)$ occurred in 37% of the elections and was the only non-negligible strategy profile in which player 2's preferred alternative $C$ won. Table 3 breaks these figures down to the treatment level showing all strategy profiles with an empirical frequency of at least 5% in either experiment. Table A.2 in the Appendix shows the evolution of these figures over the course of the experiment,

Table 4: Relative frequency of individual behavior by treatment over all periods.

| | | *Experiment 1* | | | *Experiment 2* | | |
|---|---|---|---|---|---|---|---|
| | | Performance | Random | Pooled | Chair-label | Neutral-label | Pooled |
| Chair | $A$ | 0.938 | 0.922 | 0.930 | 0.888 | 0.800 | 0.844 |
| | $B$ | 0.062 | 0.078 | 0.070 | 0.100 | 0.175 | 0.138 |
| | $C$ | 0.000 | 0.000 | 0.000 | 0.012 | 0.025 | 0.019 |
| Player 2 | $A$ | 0.500 | 0.359 | 0.430 | 0.225 | 0.325 | 0.275 |
| | $B$ | 0.031 | 0.000 | 0.016 | 0.025 | 0.000 | 0.012 |
| | $C$ | 0.469 | 0.641 | 0.555 | 0.750 | 0.675 | 0.712 |
| Player 3 | $A$ | 0.062 | 0.047 | 0.055 | 0.100 | 0.062 | 0.081 |
| | $B$ | 0.219 | 0.188 | 0.203 | 0.275 | 0.300 | 0.288 |
| | $C$ | 0.719 | 0.766 | 0.742 | 0.625 | 0.638 | 0.631 |

demonstrating that our results are robust with regard to learning effects.

**Result 1.** *The alternative preferred by the chair is the most-frequent outcome in the committee. Observed strategy profiles reveal that the chair's advantage is driven to a larger extent by the indirect effect of holding tie-breaking power than by the direct effect of exercising it.*

For an assessment of the real value of tie-breaking power one needs to consider both *direct* and *indirect effects* of formal tie-breaking power. To this end, we analyze how well different concepts of strategic reasoning fare in organizing behavior. Most notably, only 45% of all observed strategy profiles constitute a Nash equilibrium in the game (combined frequency of $(A, C, C)$ and $(A, A, B)$; unanimous profiles did not occur; the figure is equal to 44% in the last period). Equilibrium analysis, however, fails to account for the prominent profile $(A, A, C)$. The level-$k$ model in Section 2.1 explains these deviations from equilibrium play. The three distinct strategy profiles predicted by this model, $(A, C, B)$, $(A, A, C)$, and $(A, C, C)$, were the three most-frequently observed strategy profiles in Experiment 1. Together, they account for 80% of the data (81% in the last period). Voting behavior is highly compatible with limited strategic reasoning as only $L1$-types of player 2 choose alternative $A$ (a necessary condition for the profile $(A, A, C)$).

Individual voting frequencies in Table 4 support our interpretation of the data. The

number of weakly dominated strategies (WDS) played by committee members was negligible and of an order of magnitude with which errors are typically encountered in experiments (7% for the chair, 2% for player 2, and 6% for player 3). At the same time, iterated elimination of weakly dominated strategies (IEWDS) can only partially accommodate observed behavior. In line with IEWDS, the chair voted for her most-preferred option $A$ in 93% of the elections. Behavior of player 3 and player 2 was consistent with IEWDS in 74% and 56% of the elections, respectively. On average, player 3 was 1.32 times more likely to implement the strategy prescribed by IEWDS than player 2 (first period: 1.83, last period: 1.10). This difference was weakly significant according to an exact binomial test. The relative frequency to play $C$ for player 3 exceeded the one for player 2 in 7 out 8 matching groups. Assuming under the null that the relative frequency of playing $C$ is the same for player 2 and player 3, the probability to observe our data or more extreme outcomes is 0.07. Differences in IEWDS between the two players cannot be attributed to exogenous differences in cognitive skills between participants. Our level-$k$ model sheds light on this observation. To unequivocally arrive at the IEWDS prediction requires one step of level-$k$ reasoning for player 3, but two steps for player 2. Table A.3 in the Appendix reports the period-averaged data for the individual vote frequencies and establishes that the grand averages reported here are representative of the behavior in the last period of the experiment.

**Result 2.** *Behavior of regular committee members is characterized by limited strategic sophistication (level-k reasoning). Player 2's failure to iteratively delete all weakly dominated strategies is the root of the chair's advantage in the committee.*

Examining behavior of player 2 in light of potential treatment differences below further elucidates the driving forces of the indirect effects of formal tie-breaking power. Our hypothesis (see Section 2.2) was that player 2 would vote more often for the chair's preferred alternative $A$ in the performance treatment than in the random treatment. For player 3, this argument has no bite as alternative $A$ is her least-preferred alternative and a WDS strategy. Indeed, player 3 rarly voted for $A$ in any of the treatments.

A number of panel probit regressions on player 2's propensity to vote for $A$ (the chair's most-preferred alternative) establish the impact of the performance treatment. Model (1) and

Table 5: Treatment effect on player 2's behavior over all periods.

| | Experiment 1 | | Experiment 2 | |
|---|---|---|---|---|
| *DV Vote for A* | (1) | (2) | (3) | (4) |
| Performance | 1.704* | 1.879** | | |
| | (0.873) | (0.842) | | |
| Neutral-label | | | -0.101 | -0.101 |
| | | | (0.904) | (0.740) |
| Period | -0.143 | -0.141 | -0.608** | -0.608** |
| | (0.113) | (0.109) | (0.271) | (0.278) |
| Performance × Period | -0.425** | -0.430** | | |
| | (0.200) | (0.202) | | |
| Neutral-label × Period | | | 0.276 | 0.260 |
| | | | (0.326) | (0.335) |
| Words encoded | | -0.061*** | | |
| | | (0.016) | | |
| Decisiveness belief | | 0.010 | | -0.001 |
| | | (0.011) | | (0.007) |
| WTP1 chair role | | 0.172 | | 0.018 |
| | | (0.160) | | (0.098) |
| WTP1 player-2 role | | | | -0.254** |
| | | | | (0.115) |
| WTP1 player-3 role | | | | 0.322*** |
| | | | | (0.088) |
| $L1$-type (in $p$-beauty) | | | 0.707*** | 1.216*** |
| | | | (0.269) | (0.432) |
| Constant | -0.265 | -0.359 | 0.276 | 0.141 |
| | (0.634) | (1.368) | (0.644) | (0.642) |
| # of observations | 128 | 128 | 160 | 160 |
| Log pseudolikelihood | -72.04 | -69.53 | -80.64 | -74.75 |
| Wald $\chi^2$ | 14.42 | 140.31 | 16.16 | 100.04 |
| Degrees of freedom | 3 | 6 | 4 | 8 |
| Prob $> \chi^2$ | 0.002 | <0.001 | 0.003 | <0.001 |

*Notes: Reported numbers are coefficients of panel probit regressions with random-effects at the subject level. Dependent variable in all models is the dummy* Vote for *A. Standard errors in parentheses are clustered at matching-group level. Data includes player 2's behavior from all periods.* *** *significant at the 1% level,* ** *significant at the 5% level,* * *significant at the 10% level.*

(2) in Table 5 confirm our hypothesis that tie-breaking power in the performance treatment had a significant effect on the propensity to side with the chair for player 2. The coefficient for the Performance dummy is positive and significant in both models.

We also added additional controls to account for treatment-specific learning effects. While the Period variable is insignificant, the interaction effect between Performance and Period is significant and negative. This points towards a decline in the treatment effect on the propensity to side with the chair over time. Post-estimation tests revealed that the total effect of the Performance treatment (coefficient + interaction effect) was positive and significant for period 1 ($ps < 0.049$), but insignificant for later periods. This pattern is also well reflected in our data as Player 2 voted almost twice as often for $A$ in the performance treatment (81% of the elections) than in the random treatment (44%) in the first period. In the last period the corresponding figures were 25% and 38%.[11] This treatment effect was also the main driver behind the previously reported differences between the first period in the experiment and all later periods. Player 2's inclination to follow the chair in period 1 elevated the number elections won by alternative $A$ and, consequently, led to a higher discrepancy in IEWDS behavior between player 2 and 3 in the same period.

Finally, controlling for the number of correctly encoded words in the encoding task yields a significant and negative coefficient. Regarding the interpretation of this effect, our conjecture is that performance was largely driven by effort, as intended by the real-effort task (see Erkal et al., 2011). Those who exerted a high level of effort, but were not allocated to the chair role, may have voted against the alternative preferred by the chair due to spite. All other variables did not have a significant impact on player 2's propensity to vote for $A$.

**Result 3.** *If tie-breaking power is assigned based on performance, then player 2 chooses the chair's preferred alternative significantly more often in the first period. This in turn increases the indirect effect of the chair's tie-breaking power.*

[11] The strong effect of the performance treatment on player 2's propensity to side with the chair in period 1 was also corroborated by a one-sided Fisher's exact-Boschloo test ($N = 32$, $p = 0.033$). To account for potential differences in the level of effort provision between treatments we excluded top-performing player 2s from the random treatment (no player-2 participant was a top performer in the performance treatment by definition). The corresponding one-sided Fisher's exact-Boschloo test, now comparing non top-performers between treatments, is again significant ($N = 24$, $p = 0.042$).

The treatment comparison provided causal evidence that holding tie-breaking power based on performance (in an unrelated task) alters regular members' decisions. We take this as evidence that player 2 is being lured, against her monetary incentives, by legitimate formal power to side with the chair and thereby turns formal tie-breaking power into real power in the committee. In support of our interpretation, we provide additional information on how participants perceived the attractiveness of the chair's role before making decisions in the committee. If participants were strategically sophisticated, and expected others to act similarly, they should have avoided the chair's role. In line with behavioral findings, questionnaire responses however reveal that the majority of participants did not expect the theoretically detrimental consequences of formal tie-breaking power, but perceived the chair position as overwhelmingly positive. Participants exerted a high level of effort in the real-effort task (mean $m = 36.3$, $SD = 7.3$) and indicated a high ex-ante willingness to become chair prior to the voting game (WTP1: $m = 8.2$, $SD = 1.7$). Lastly, participants overestimated the *direct effect* of tie-breaking power ex-ante: they expected 55% of the elections to result in a three-way, whereas a tie occurred in only 14%.

# 4 Experiment 2

We established in Experiment 1 that the formal power to break ties is beneficial for the chair due to limited strategic sophistication of committee members. In Experiment 2, we investigate the general robustness of the chair's real advantage and provide additional insights into how the underlying perception of formal tie-breaking power changes with members' payoff experiences in the game.

Throughout Experiment 1, we followed the convention to refer to the member holding tie-breaking power as the 'chair' of the committee. The labeling might have increased the salience of the role and thus contributed possibly to the *level* of the chair's advantage. The tournament character in the encoding task in Experiment 1 might have also added to this effect. In Experiment 2, we eliminate any distortions regarding behavior of committee members not related to the incentive structure of the game. This was done by assigning player roles randomly and by introducing a control treatment with neutral chair labels (as a regular

member). Except for these changes, the design was identical to the one of Experiment 1.

### 4.1 Design

*Treatments and committee game.* Participants were allocated randomly to one of two treatments which differed only in the labeling of the chair role. In the *neutral-label* treatment, we used 'voter $i$' to refer to player roles, with $i \in \{X, Y, Z\}$. In the *chair-label* treatment, we used the same label as in Experiment 1 ('chair') to refer to the player holding tie-breaking power. Table 2 summarizes treatment differences between experiments. The design allowed us to assess whether or not role labels, independent of the formal tie-breaking power, change behavior of committee members. We employed the same voting game as in Experiment 1. Moreover, participants stated the attractiveness of each player role in the committee before (WTP1) and after (WTP2) the voting game. The post-voting questionnaire included questions on social status of committee members as well as an incentivized $p-$beauty contest in which the participant with the best guess in a session earned an additional €10. The instructions can be found in Appendix B.

*Procedural details.* The experiment was conducted at the Vienna Center for Experimental Economics (VCEE) of the University of Vienna. In total, 120 participants (60 females, average age 25) were recruited from a student pool using the online recruitment system ORSEE. All parts of the experiment were run with the software z-Tree. Each of the 5 sessions we run was comprised of 24 subjects distributed in 2 independent matching groups, one per treatment. We thus collected data on 5 independent matching groups per treatment in total. A session lasted 70 min and average total earnings were about €16.

### 4.2 Results

Figure 1(a) and 1(b) present the outcome and strategy profile distribution for Experiment 2, pooled over treatments. It can be seen that the distributions of outcomes and of strategy profiles were highly comparable between experiments. Consistent with Experiment 1, $A$ was the most-frequently implemented alternative winning 48% of the elections, followed by $C$ with 45% and $B$ with 8%. The chair's advantage was sizable, with $A$'s winning frequency

being significantly higher than the benchmark prediction of 1/3 under random tie-breaking.[12] Conditioning on outcome $A$, 42% resulted from the chair exercising tie-breaking power directly and 58% stemmed from *indirect effects* of holding tie-breaking power. Restricting data to the last period, direct and indirect effects were equally likely.

Tables 3 and 4 further reveal that differences in behavior observed between experiments were mainly driven by indirect effects in the performance treatment in Experiment 1, which we eliminated in Experiment 2. Using the random treatment of Experiment 1 as the baseline for comparison, differences between experiments further diminish. From this we conclude that the salience of the chair role, introduced by the encoding task, had no significant effect on the level of the chair's advantage in Experiment 1.

Regarding the revealed level of strategic sophistication, the predictive power of the level-$k$ model is again higher than the one of equilibrium analysis. In terms of strategy profiles, 71% are consistent with our level-$k$ model and 46% with Nash (last period 70% and 48%, respectively). To lend further support to the level-$k$ model, we calculated the biserial correlation between an exogenous measure of strategic sophistication elicited in the $p$-beauty game and player 2's behavior in the voting game derived from the level-$k$ model (e.g. Carpenter et al., 2013; Gill and Prowse, 2016). Remember that only player 2 of type $L1$ vote for $A$ according to the level-$k$ model, while all other levels of player 2 vote for $C$. Motivated by this theoretical prediction, we classified player 2 as being of level $L1$ in the $p$-beauty game if her guess was between the optimal point predictions for $L1$ and $L2$ behavior (assuming $L0$ behavior is to randomize uniformly). We found a positive and significant biserial correlation of 0.252 ($p < 0.001$) between the number of times player 2 has voted for $A$ and player 2 being classified as $L1$ in the $p$-beauty game. A player 2 of type $L1$ in the $p$-beauty game, thus, voted more often for alternative $A$. Table A.1 in the Appendix provides details regarding the per-period differences between experiments.

**Result 4.** *Experiment 2 confirms all main behavioral findings of Experiment 1 (cf. Result 1 and Result 2). The exogenously elicited measure of strategic reasoning from a p-beauty contest*

[12] Over all periods, we counted 9 out of 10 matching groups that saw $A$ win more than 1/3 of the elections, and 7 out 10 of such groups in the last period only. Assuming a winning frequency for $A$ of 1/3 under the null, the probability to observe our data or more extreme ones is lower than 0.02 in both cases.

*is predictive of player 2's behavior in the committee.*

Next, we explicitly test whether or not the chair label contributed to the marked real advantage of the one holding formal tie-breaking power by comparing behavior of committee members across treatments (neutral and chair label). We will focus our analysis on player 2's behavior because, first, label effects make the sharpest predictions for player 2 and, second, it ensures comparability between experiments.

Consider the behavior of player 2. If the chair label increased the salience of the alternative preferred by the chair, we would expect player 2 to choose the chair's preferred alternative $A$ more often in the chair-label than the neutral-label treatment. We run panel probit estimations on player 2's propensity to side with the chair to investigate this hypothesis. The results, reported in Table 5 in Models (3) and (4), did not reveal any treatment effect on player 2's propensity to vote for $A$. Player 2 chose $A$ with a frequency of 22% in the *chair-label* treatment and with 33% in the *neutral-label* treatment. The propensity to side with the chair was, however, declining over time as shown by the coefficient of the Period variable.

We included further controls in the regression analysis to reconfirm our correlation analysis reported before. Indeed, the exogenous measure of strategic sophistication was a highly significant predictor of player 2's behavior in the voting game. Those player-2 participants who choose a strategy compatible with level-1 behavior in the $p$-beauty game (*L1-type* dummy) had a higher propensity to choose a voting strategy compatible with $L1$ in the voting game.

We also controlled for the ex-ante stated attractiveness for each of the three player roles, as well as for the stated decisiveness belief of the tie-breaking rule. The WTP1 coefficients for player 2 and player 3 were significant, the former negative, the latter positive. One interpretation is that those player-2 participants who evaluated their own role as more positive were participants capable of solving the game through IEWDS and hence foresaw the negative consequences of holding formal tie-breaking power. Participants who evaluated the player-3 role as relatively positive did not seem to be aware of the solution of the voting game proposed by IEWDS. From this we conclude that the driving factor behind committee behavior was the misperception of the strategic incentives caused by the asymmetry of formal tie-breaking

power and not the labeling of the chair position.[13]

**Result 5.** *The labeling of the chair role has no significant effect on behavior in the committee.*

The focus of our analysis was to establish the real power of the chair relative to the theoretical benchmark of rational strategic voting. Before the voting stage, participants overestimated the decisiveness of tie-breaking power; their mean stated belief about the chair exercising her tie-breaking power of 50% exceeded the actual frequency of 24%. They also ascribed the highest attractiveness to the chair's position (mean WTP1=8.85), followed by the role of player 2 (WTP1=5.21), and player 3 (WTP1=4.26). The high attractiveness of the chair role is however inconsistent with relative earnings in the experiment. Integrating experience from all periods, the chair's average earnings of €10.13 fell short of the €11.88 player 2 earned, which were both larger than the average earnings of €8.00 for player 3.[14] The inconsistency between chair and player-2 role rankings in terms of stated attractiveness WTP1 and payoff experiences give rise to an interesting set of predictions regarding the post-voting attractiveness measure WTP2. If WTP2 statements were guided by relative earnings, we should expect a post-voting decrease in WTP2 for the chair role and an increase in the same for the player-2 role.

Figure 2 plots the differences between post-voting and pre-voting attractiveness assessed by all players, for each player role. In line with the decision process being driven by relative earnings, both player 2 and player 3 down-shaded their stated attractiveness for the chair role and increased the one for the role of player 2. The chair's re-evaluations followed a different

[13] Moreover, analogous regression analyses (unreported) did also not detect any treatment differences in individual behavior for the chair or for player 3. The chair's frequency of voting for $A$ was 89% in the chair-label treatment and 80% in the neutral-label treatment. For player 3, the chair label does not provide an unambiguous cue regarding behavior. The only reasonable way to influence the outcome of the committee for player 3 is to choose $C$ in an effort to gang up against the chair. Player 3 chose $C$ with a frequency of 63% in the chair-label treatment and 64%. Note that focal point theory (Schelling, 1960) makes no prediction regarding behavior in the chair-label treatment. The reason is that the chair label does not provide a *unique* solution to which equilibrium should be played in the three-player game (see also Bacharach and Bernasconi, 1997; Bardsley et al., 2010; Alós-Ferrer and Kuzmics, 2013).

[14] Pairwise two-sided Wilcoxon-signed-rank (WSR) and Wilcoxon-rank-sum (WRS) tests show that the reported differences in elicited WTP1s and differences in earnings across player roles were statistically significant ($N = 120$ for the former and $N = 80$ for the latter, all adjusted $p < 0.001$). All p-values are Holm-Bonferroni corrected to account for multiple hypothesis testing.

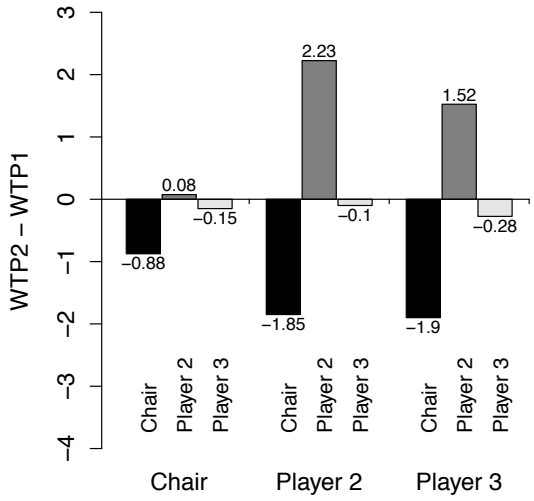



Figure 2: Change in elicited role attractiveness.

pattern altogether, as summarized below.[15]

**Result 6.** *There is a pronounced difference in the role attractiveness elicited from regular members and the chair. While regular members adjust the attractiveness of each role according to its instrumental value in the committee, the chair is reluctant to revise her own-role attractiveness and does not adjust for the relative attractiveness of player 2 ex-post.*

Interestingly, the heterogeneous integration of payoff experiences into post-voting attractiveness measures is consistent with an emerging body of work on the non-instrumental value of holding decision rights. According to this literature, decision makers tend to value decision rights per se (Fehr et al., 2013; Bartling et al., 2014) as they are motivated to retain control over own payoffs (Owens et al., 2014) and cherish non-interference by others (Neri and Rommeswinkel, 2016). Our last questionnaire measure, post-voting elicited social status, is also consistent with this interpretation. Irrespective of relative average earnings in the committee, the chair attributed a higher social status to herself than to any other member.[16]

[15] Two-sided Cuzick's trend tests confirm the significance of these differences in revaluation patterns $WTP2 - WTP1$ across chair, player 2, and player 3 participants ($N = 120$, all adjusted $p < 0.069$). Ex-post, regular members evaluate the chair role and the player-2 role as equally attractive (two-sided WSR, $N = 40$, all adjusted $p > 0.484$). The chair, however, maintained a higher post-voting attractiveness WTP2 for her own role in comparison to the player 2 role (two-sided WSR, $N = 40$, adjusted $p = 0.003$).

[16] In our context, social status could either arise from relative earnings in the committee or by holding the formal power to break ties. The ex-post elicited social status perfectly reflects the differences across player

# 5 Discussion

The effects of formal tie-breaking power on committee decisions were derived from a stylized committee voting model and its predictions confirmed in two independent experiments. We address a number of questions regarding the robustness of the results below.

Our insights are applicable beyond the committee model we present in Section 2.1 if rule asymmetries remain small (e.g. no dictators) and preferences diverge (e.g. no absolute majority of members with identical preferences). For instance, we provided participants with complete information about the preference profile in the committee in order to encourage strategically sophisticated voting. In accordance with the predictions under limited strategic sophisticated in behavior, and building on established findings in the literature (see Tyszler and Schram, 2016; Granić, 2017), it is reasonable to expect that limited information about preferences in the committee induces less strategic voting. Theoretically, this leads to a higher level of real power of the chair through an increase in both the direct and the indirect effect of holding tie-breaking power.

Our theoretical results also remain valid for large committees as long as the distribution of preferences (e.g. voting weights of each preference type or voting block) in the committee remains constant. For a complete assessment of the effects of group size on committee decisions, our results also suggest that one needs to take into account both the direct effect and the indirect effect. Obviously, the direct effect of exercising tie-breaking power decreases in the total number of committee members. Indirect effects of holding (not wielding) tie-breaking power can however increase in the number of committee members under boundedly-rational behavior, as the probability of decision mistakes increases in the size of the committee. Assuming that each regular member in a voting block is making mistakes with some small probability, the real power of the chair increases as long as the increase in the indirect effect dominates the decrease in the direct one. For the empirically observed rates of 'trembles' of each player type (see Table 4), the total effect of an increase in committee size clearly favors the chair.

The simple one-shot voting game allowed us to parsimoniously relate behavior of partic-

roles found in the WTP2 measure, again with the chair being reluctant to recognize the strategically decisive role of player 2. A detailed account of both measures is found in Appendix A.2.

ipants to the level of strategic sophistication (WDS, IEWDS, and level-$k$). This is possible because *every* outcome of the game induces the same distribution of (in)equity in the committee, and thus results cannot be explained by social or distributional preferences (cf. Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). Another important reason for focussing on behavior in ad-hoc committees is that the study of standing committees, in which decisions are made repeatedly by the same members, would change the interpretation of the committee setting. First, by virtue of the tie-breaking rule it is impossible that the committee votes on the *same* issue more than once. Second, even if the committee would meet regularly on *different* issues, it is reasonable to assume that preferences of members vary between issues that are decided upon. In such an environment, learning effects are severely limited even if the same members partake in the committee repeatedly.

## 6 Conclusion

There exists a remarkable variety in how the formal decision right to break ties is allocated in committees. Sometimes it is based on position, experience, seniority, or emerges implicitly within an organization. But does the variation in assignment procedures imply that small asymmetries in *formal* power bear no *real* consequences on committee decisions?

In this article, we studied the impact of formal tie-breaking power on committee decision making in a situation of conflict. The committee was set up such that formal tie-breaking power has in equilibrium no influence over outcomes if committee members vote fully rational and strategic. Results from two independent laboratory experiments show however that purely formal tie-breaking power has a large impact on decisions in ad-hoc committees. Simple plurality voting leads to more outcomes preferred by the chair under the asymmetric tie-breaking rule than predicted theoretically.

Our theoretical and experimental analysis highlights important drivers behind the real power of the chair that deserve emphasis. Regarding the institutional setting, we show that the common intuition that formal tie-breaking power is irrelevant for committee decisions, because ties are low probability events, is at best incomplete. We demonstrate that the real advantage of the chair is only partially due to exercising tie-breaking power directly. It is

rather the consequence of (mostly off-equilibrium) outcome profiles favorable to the chair that are reached as soon as voting deviates from full strategic sophistication. To illustrate the underlying channel, we manipulated the size of this indirect effect through changing the legitimacy of holding tie-breaking power. The treatment variation provides causal evidence that formal tie-breaking power, if allocated according to performance, entices regular members into siding with the chair more frequently and hence increases the indirect effect of tie-breaking power. Behavior of this kind can have many different sources. For example, committee member may be prone to making small mistakes, have limited strategic reasoning, hold a belief about limited strategic sophistication of others, or may simply hold incomplete information about the preference distribution in committee.

More broadly speaking, formal tie-breaking power in our setting is a crisp way of formalizing small asymmetries in voting weights, bargaining power, authority, or leadership which exist in many committees but are often difficult to identify for external observers. Our findings imply that even purely formal power should not be allocated randomly in a voting board as it can lead to real consequences under reasonable conditions. Designing institutions and mechanisms robust to small deviations from fully-rational behavior is an important challenge for advancing our understanding of decision making in committees.

## References


Aghion, Philippe and Jean Tirole (1997), "Formal and real authority in organizations." *Journal of Political Economy*, 105, 1–29.

Alaoui, Larbi and Antonio Penta (2016), "Endogenous depth of reasoning." *Review of Economic Studies*, 83, 1297–1333.

Ali, S. Nageeb, B. Douglas Bernheim, and Xiaochen Fan (2014), "Predictability and power in legislative bargaining." Working Paper.

Alós-Ferrer, Carlos and Christoph Kuzmics (2013), "Hidden symmetries and focal points." *Journal of Economic Theory*, 148, 226–258.

Ansolabehere, Stephen, James M. Snyder, Aaron B. Strauss, and Michael M. Ting (2005), "Voting weights and formateur advantages in the formation of coalition governments." *American Journal of Political Science*, 49, 550–563.

Apesteguia, Jose, Miguel A. Ballester, and Yusufcan Masatlioglu (2014), "A foundation for strategic agenda voting." *Games and Economic Behavior*, 87, 91–99.

Bacharach, Michael and Michele Bernasconi (1997), "The variable frame theory of focal points: An experimental study." *Games and Economic Behavior*, 19, 1–45.

Banzhaf, John F. (1965), "Weighted voting doesn't work: a mathematical analysis." *Rutgers Law Review*, 19, 317–343.

Bardsley, Nicholas, Judith Mehta, Chris Starmer, and Robert Sugden (2010), "Explaining focal points: Cognitive hierarchy theory versus team reasoning." *Economic Journal*, 120, 40–79.

Baron, David P. and John A. Ferejohn (1989), "Bargaining in legislatures." *American Political Science Review*, 83, 1181–1206.

Bartlett, Randall (1989), *Economics and Power: An Inquiry into Human Relations and Markets*. Cambridge University Press.

Bartling, Björn, Ernst Fehr, and Holger Herz (2014), "The intrinsic value of decision rights." *Econometrica*, 82, 2005–2039.

Bassi, Anna (2015), "Voting systems and strategic manipulation: An experimental study." *Journal of Theoretical Politics*, 27, 58–85.

Bernheim, B. Douglas, Antonio Rangel, and Luis Rayo (2006), "The power of the last word in legislative policy making." *Econometrica*, 74, 1161–1190.

Berry, Christopher R. and Anthony Fowler (2015), "Cardinals or clerics? congressional committees and the distribution of pork." *American Journal of Political Science*, 60, 692–708.

Berry, Christopher R. and Anthony Fowler (2016), "Committee chairs and the concentration of power in congress." Working Paper.

Blanco, Mariana, Dirk Engelmann, Alexander K. Koch, and Hans-Theo Normann (2010), "Belief elicitation in experiments: Is there a hedging problem?" *Experimental Economics*, 13, 412–438.

Blinder, Alan S. (2004), *The Quiet Revolution: Central Banking Goes Modern*. Yale University Press.

Blinder, Alan S. (2007), "Monetary policy by committee: Why and how?" *European Journal of Political Economy*, 23, 106–123.

Blinder, Alan S. and John Morgan (2005), "Are two heads better than one? monetary policy by committee." *Journal of Money, Credit, and Banking*, 37, 798–811.

Blinder, Alan S. and John Morgan (2008), "Do monetary policy committees need leaders? a report on an experiment." *American Economic Review, Papers & Proceedings*, 98, 224–229.

Bolton, Gary E. and Axel Ockenfels (2000), "Erc—a theory of equity, reciprocity, and competition." *American Economic Review*, 10, 166–193.

Bouton, Laurent, Aniol Llorente-Saguer, and Frédéric Malherbe (forthcoming), "Get rid of unanimity: The superiority of majority rule with veto power." *Journal of Political Economy*.

Brams, Steven J. and Paul J. Affuso (1976), "Power and size: A new paradox." *Theory and Decision*, 7, 29–56.

Camerer, Colin F., Teck-Hua Ho, and Juin-Kuan Chong (2004), "A cognitive hierarchy model of games." *Quarterly Journal of Economics*, 119, 861–898.

Carpenter, Jeffrey, Michael Graham, and Jesse Wolf (2013), "Cognitive ability and strategic sophistication." *Games and Economic Behavior*, 80, 115–130.

Casella, Alessandra, Aniol Llorente-Saguer, and Thomas R. Palfrey (2012), "Competitive equilibrium in markets for votes." *Journal of Political Economy*, 120, 593–658.

Casella, Alessandra, Thomas Palfrey, and Sébastien Turban (2014), "Vote trading with and without party leaders." *Journal of Public Economics*, 112, 115–128.

Casella, Alessandra and Sébastien Turban (2014), "Democracy undone. systematic minority advantage in competitive vote markets." *Games and Economic Behavior*, 88, 47–70.

Chappell, Henry W., Rob Roy McGregor, and Todd A. Vermilyea (2005), *Committee Decisions on Monetary Policy: Evidence from Historical Records of the Federal Open Market Committee*. MIT Press.

Chappell, Henry W., Rob Roy McGregor, and Todd A. Vermilyea (2014), "Power-sharing in monetary policy committees: Evidence from the united kingdom and sweden." *Journal of Money, Credit and Banking*, 46, 665–692.

Costa-Gomes, Miguel A. and Vincent P. Crawford (2006), "Cognition and behavior in two-person guessing games: An experimental study." *American Economic Review*, 96, 1737–1768.

Costa-Gomes, Miguel A., Vincent P. Crawford, and Bruno Broseta (2001), "Cognition and behavior in normal-form games: An experimental study." *Econometrica*, 69, 1193–1235.

Dal Bó, Pedro, Andrew Foster, and Louis Putterman (2010), "Institutions and behavior: Experimental evidence on the effects of democracy." *American Economic Review*, 100, 2205–2229.

de Groot Ruiz, Adrian, Roald Ramer, and Arthur Schram (2016), "Formal versus informal legislative bargaining." *Games and Economic Behavior*, 96, 1–17.

de Kwaadsteniet, Erik W. and Eric van Dijk (2010), "Social status as a cue for tacit coordination." *Journal of Experimental Social Psychology*, 46, 515–524.

de la Boétie, Etienne (1975), *The Politics of Obedience: the Discourse of Voluntary Servitude*. Black Rose Books.

Dhillon, Amrita and Ben Lockwood (2004), "When are plurality rule voting games dominance-solvable?" *Games and Economic Behavior*, 46, 55–75.

Drouvelis, Michalis, Maria Montero, and Martin Sefton (2010), "Gaining power through enlargement: Strategic foundations and experimental evidence." *Games and Economic Behavior*, 2, 274–292.

Eckel, Catherine and Charles A. Holt (1989), "Strategic voting in agenda-controlled committee experiments." *American Economic Review*, 79, 763–773.

Erkal, Nisvan, Lata Gangadharan, and Nikos Nikiforakis (2011), "Relative earnings and giving in a real-effort experiment." *American Economic Review*, 101, 3330–3348.

Farquharson, Robin (1969), *Theory of Voting*. Yale University Press.

Fehr, Ernst, Holger Herz, and Tom Wilkening (2013), "The lure of authority: Motivation and incentive effects of power." *American Economic Review*, 103, 257–272.

Fehr, Ernst and Klaus M. Schmidt (1999), "A theory of fairness, competition, and cooperation." *Quarterly Journal of Economics*, 114, 817–868.

Fischbacher, Urs (2007), "z-tree: Zurich toolbox for ready-made economic experiments." *Experimental Economics*, 10, 171–178.

Fréchette, Guillaume, John H. Kagel, and Steven F. Lehrer (2003), "Bargaining in legislatures: An experimental investigation of open versus closed amendment rules." *American Political Science Review*, 97, 221–232.

Fréchette, Guillaume, John H. Kagel, and Massimo Morelli (2005a), "Behavioral identification in coalitional bargaining: An experimental analysis of demand bargaining and alternating offers." *Econometrica*, 73, 1893–1937.

Fréchette, Guillaume, John H. Kagel, and Massimo Morelli (2005b), "Nominal bargaining power, selection protocol, and discounting in legislative bargaining." *Journal of Public Economics*, 89, 1497–1517.

Gill, David and Victoria Prowse (2016), "Cognitive ability, character skills, and learning to play equilibrium: A level-k analysis." *Journal of Political Economy*, 124, 1619–1676.

Goeree, Jacob K., Charles A. Holt, and Thomas R. Palfrey (2016), *Quantal Response Equilibrium: A Stochastic Theory of Games*. Princeton University Press.

Granić, Dura-Georg (2017), "The problem of the divided majority: Preference aggregation and uncertainty." *Journal of Economic Behavior & Organization*, 133, 21–38.

Hansen, Stephen, Michael McMahon, and Carlos Velasco Rivera (2014), "Preferences or private assessments on a monetary policy committee?" *Journal of Monetary Economics*, 67, 16–32.

Ho, Teck-Hua, Colin Camerer, and Keith Weigelt (1998), "Iterated dominance and iterated best response in experimental 'p-beauty contests'." *American Economic Review*, 88, 947–969.

Kagel, John H., Hankyoung Sung, and Eyal Winter (2010), "Veto power in committees: An experimental study." *Experimental Economics*, 13, 167–188.

Karos, Dominik and Hans Peters (forthcoming), "Effectivity and power." *Games and Economic Behavior*.

Kelman, Herbert C. (1958), "Compliance, identification, and internalization: Three processes of attitude change." *Journal of Conflict Resolution*, 2, 51–60.

Kohlberg, Elon and Jean-Francois Mertens (1986), "On the strategic stability of equilibria." *Econometrica*, 54, 1003–1037.

Maaser, Nicola, Fabian Paetzel, and Stefan Traub (2016), "Power illusion in coalitional bargaining: An experimental analysis." Working Paper.

McKelvey, Richard D. and Thomas R. Palfrey (1995), "Quantal response equilibria for normal form games." *Games and Economic Behavior*, 10, 6–38.

McKelvey, Richard D. and Thomas R. Palfrey (1998), "Quantal response equilibria for extensive form games." *Experimental Economics*, 1, 9–41.

Milgram, Stanley (1963), "Behavioral study of obedience." *Journal of Abnormal and Social Psychology*, 67, 371–378.

Montero, Maria, Martin Sefton, and Ping Zhang (2008), "Enlargement and the balance of power: An experimental study." *Social Choice and Welfare*, 30, 69–87.

Moulin, Hervé (1979), "Dominance solvable voting schemes." *Econometrica*, 47, 1137–1151.

Nagel, Rosemarie (1995), "Unravelling in guessing games: An experimental study." *American Economic Review*, 85, 1313–1326.

Neri, Claudia and Hendrik Rommeswinkel (2016), "Decision rights: Freedom, power and interference." Working Paper.

Owens, David, Zachary Grossman, and Ryan Fackler (2014), "The control premium: A preference for payoff autonomy." *American Economic Journal: Microeconomics*, 6, 138–161.

Penrose, Lionel S. (1946), "The elementary statistics of majority voting." *Journal of the Royal Statistical Society*, 109, 53–57.

Plott, Charles R. and Michael E. Levine (1978), "A model of agenda influence on committee decisions." *American Economic Review*, 68, 146–160.

Riboni, Alessandro and Francisco J. Ruge-Murcia (2010), "Monetary policy by committee: Consensus, chairman dominance or simple majority?" *Quarterly Journal of Economics*, 125, 363–416.

Russel, Bertrand (1938), *Power: A New Social Analysis*. Allen and Unwin.

Schelling, Thomas (1960), *The Strategy of Conflict*. Harvard University Press.

Schlag, Karl, James Tremewan, and Joël van der Weele (2014), "A penny for your thoughts: A survey of methods for eliciting beliefs." *Experimental Economics*, 18, 475–490.

Selten, Reinhard (1975), "Reexamination of the perfectness concept for equilibrium points in extensive games." *International Journal of Game Theory*, 4, 25–55.

Shapley, Lloyd S. and Martin Shubik (1954), "A method for evaluating the distribution of power in a committee system." *American Political Science Review*, 48, 787–792.

Silverman, Dan, Joel Slemrod, and Neslihan Uler (2014), "Distinguishing the role of authority "in" and authority "to"." *Journal of Public Economics*, 113, 32–42.

Simon, Herbert A. (1951), "A formal theory of the employment relationship." *Econometrica*, 19, 293–305.

Snyder, James M., Michael M. Ting, and Stephen Ansolabehere (2005), "Legislative bargaining under weighted voting." *American Economic Review*, 95, 981–1004.

Stahl, Dale O. and Paul W. Wilson (1995), "On players' models of other players: Theory and experimental evidence." *Games and Economic Behavior*, 10, 218–254.

Trautmann, Stefan T. and Gijs van de Kuilen (2015), "Belief elicitation: A horse race among truth serums." *Economic Journal*, 125, 2116–2135.

Tremewan, James and Christoph Vanberg (2016), "The dynamics of coalition formation–a multilateral bargaining experiment with free timing of moves." *Journal of Economic Behavior & Organization*, 130, 33–46.

Tyler, Rom R. (2006), *Why People Obey the Law*. Princeton University Press.

Tyszler, Marcelo and Arthur Schram (2016), "Information and strategic voting." *Experimental Economics*, 19, 360–381.

Vespa, Emanuel I. (2016), "Malapportionment and multilateral bargaining: An experiment." *Journal of Public Economics*, 133, 64–74.

Weber, Max (1978), *Economy and Society: An Outline of Interpretive Sociology*. University of California Press.

Winter, Eyal (1996), "Voting and vetoing." *American Political Science Review*, 90, 813–823.

**(FOR ONLINE PUBLICATION)**

**Appendix**

*for*

"Tie-Breaking Power in Committees"

# A Additional results

## A.1 Behavior and outcomes over time

Table A.1 reports the most important variables regarding behavior and outcomes, broken down to the per-period level. We do so for two reasons. First, per-period results demonstrate that the main findings are not driven by our choice of reporting average behavior in the main text, but also obtain if we restrict our data to the first period or last period only. Second, results highlight important similarities/differences, we observe between experiments. We also provide a detailed per-period presentation of individual behavior and strategy profiles in Table A.2 and Table A.3.

As reported in the main text, we observed a pronounced learning effect from the first to the second period. This holds true for both experiments. For example, the percentage of elections won by $A$ is highest in the first period. It then drops to a non-negligible, constant level from period 2 onward. The same trend in the data is observed for indirect effects of holding power in Experiment 2, but not for Experiment 1. Overall, indirect effects play a prominent role and underlie a significant number of implementations of $A$ in all periods. The explanatory power of the level-$k$ model introduced in Section 2.1 also remains stable over time. The level-$k$ model has in both experiments a clearly higher explanatory power than Nash-equilibrium analysis in every single period. This is exemplified by the high relative frequency of the prominent non-Nash strategy profile $(A, A, C)$. Despite level differences between experiments, $(A, A, C)$ constitutes an empirically relevant part of our data.

Where do the main differences between experiments originate from? We identify two main sources. First, the relative frequency with which player 2 vote for $A$ is lower in Experiment 2 than in Experiment 1. Second, the data in Experiment 2 seems to contain more noise than the in Experiment 1, as evidenced by the lower level of weakly dominated strategies played in the latter than in the former. The main driver for this observations is to be found in the behavior of the chair. The chair voted for non-$A$ alternatives in only 7% of the elections in Experiment 1, while she did so in 16% of the elections in Experiment 2. Withstanding these differences, we conclude that both experiments support our interpretation that limited strategic sophistication of regular committee members turned formal tie-breaking power through the indirect effect into real power for the chair.

Table A.1: Main behavioral results in Experiment 1 and 2 by period.

| | Experiment 1 | | | | Experiment 2 | | | |
|---|---|---|---|---|---|---|---|---|
| | P1 | P2 | P3 | P4 | P1 | P2 | P3 | P4 |
| A wins | 72 | 50 | 47 | 56 | 58 | 47 | 40 | 45 |
| Indirect effects | 82 | 75 | 80 | 77 | 78 | 47 | 50 | 50 |
| NE | 38 | 56 | 41 | 44 | 50 | 40 | 45 | 48 |
| Level-$k$ | 78 | 81 | 78 | 81 | 70 | 73 | 70 | 70 |
| $(A, A, C)$ | 44 | 25 | 31 | 28 | 25 | 13 | 13 | 8 |
| Player 2 Voting A | 53 | 38 | 41 | 31 | 50 | 25 | 20 | 15 |
| WDS | 2 | 3 | 7 | 7 | 5 | 10 | 8 | 10 |

*Notes: P1 to P4 represent the four periods in the experiments. All reported numbers are percentages. A wins: percentage of elections won by A. Indirect effects: percentage of implementations underlying A due to indirect effects. NE: percentage of strategy profiles consistent with Nash-equilibrium analysis. Level-k: percentage of strategy profiles consistent with the level-k model discussed in the main text. (A,A,C): percentage of (A,A,C) strategy profiles. Player 2 Voting A: percentage of voting decisions in favor of A by player 2. WDS: percentage of weakly dominated strategies.*

## A.2 Attractiveness measures of chair role

In this section we provide a detailed analysis of various attractiveness measures for player roles elicited in the experiments. Recall that in Experiment 1, each committee member stated her belief about the attractiveness of the chair role only. Experiment 2 allows for a more detailed analysis of these attractiveness measures as participants stated their beliefs about the attractiveness of each player role in the committee.

### A.2.1 Experiment 1

Performance in the encoding task shows a strong motivation of participants to hold tie-breaking power in Experiment 1. On average, participants encoded 36 words, with 75% of the mass of observations lying in the interval $[31, 42]$. The stated ex-ante attractiveness (WTP1) is skewed strongly towards the maximal response of 10. The mode WTP1 was 10 with mean 8.82 and median 10 (53 out 96 participants responded with 10). These numbers suggest that participants assigned a positive value to the chair role. As reported in the main text, participants overestimated the decisiveness (pivotality) of the tie-breaking power. While they expected a three-way tie in 55% of the elections (median 52.5%), three-way ties occurred in only 12.5% of the first-period elections and in 14% of all elections. The overestimation, at least partially, rationalizes the positive ex-ante attitude (WTP1) participants attributed to the chair's tie-breaking power.

### A.2.2 Experiment 2

As in Experiment 1, we also observed a positive ex-ante attitude towards the chair role in Experiment 2. As explained in the main text, WTP1 ratings of participants were highest for

Table A.2: Relative frequency of observed strategy profiles in each period.

| | | $(A,C,C)$ | $(A,A,C)$ | $(A,C,B)$ | $(A,A,B)$ | $(A,C,A)$ | $(B,C,C)$ | Other |
|---|---|---|---|---|---|---|---|---|
| *Experiment 1* | | | | | | | | |
| | P1 | 0.125 | 0.563 | 0.063 | 0.188 | 0.000 | 0.000 | 0.063 |
| | P2 | 0.500 | 0.250 | 0.063 | 0.125 | 0.000 | 0.000 | 0.063 |
| Performance | P3 | 0.313 | 0.438 | 0.000 | 0.063 | 0.000 | 0.063 | 0.125 |
| | P4 | 0.375 | 0.188 | 0.188 | 0.063 | 0.125 | 0.000 | 0.063 |
| | All | 0.328 | 0.359 | 0.078 | 0.109 | 0.031 | 0.016 | 0.078 |
| | P1 | 0.313 | 0.313 | 0.188 | 0.125 | 0.000 | 0.063 | 0.000 |
| | P2 | 0.438 | 0.250 | 0.125 | 0.063 | 0.063 | 0.000 | 0.063 |
| Random | P3 | 0.438 | 0.188 | 0.188 | 0.000 | 0.063 | 0.063 | 0.063 |
| | P4 | 0.438 | 0.375 | 0.063 | 0.000 | 0.063 | 0.063 | 0.000 |
| | All | 0.406 | 0.281 | 0.141 | 0.047 | 0.047 | 0.047 | 0.031 |
| | P1 | 0.219 | 0.438 | 0.125 | 0.156 | 0.000 | 0.031 | 0.031 |
| | P2 | 0.469 | 0.250 | 0.094 | 0.094 | 0.031 | 0.000 | 0.063 |
| Pooled | P3 | 0.375 | 0.313 | 0.094 | 0.031 | 0.031 | 0.063 | 0.094 |
| | P4 | 0.406 | 0.281 | 0.125 | 0.031 | 0.094 | 0.031 | 0.031 |
| | All | 0.367 | 0.320 | 0.109 | 0.078 | 0.039 | 0.031 | 0.055 |
| *Experiment 2* | | | | | | | | |
| | P1 | 0.350 | 0.350 | 0.150 | 0.050 | 0.000 | 0.000 | 0.100 |
| | P2 | 0.350 | 0.100 | 0.300 | 0.050 | 0.050 | 0.000 | 0.150 |
| Chair-Label | P3 | 0.400 | 0.100 | 0.100 | 0.000 | 0.100 | 0.150 | 0.150 |
| | P4 | 0.450 | 0.000 | 0.250 | 0.050 | 0.150 | 0.100 | 0.000 |
| | All | 0.388 | 0.138 | 0.200 | 0.038 | 0.075 | 0.063 | 0.100 |
| | P1 | 0.300 | 0.150 | 0.100 | 0.250 | 0.000 | 0.050 | 0.150 |
| | P2 | 0.350 | 0.150 | 0.200 | 0.000 | 0.050 | 0.050 | 0.200 |
| No-Label | P3 | 0.400 | 0.150 | 0.250 | 0.000 | 0.000 | 0.100 | 0.100 |
| | P4 | 0.350 | 0.150 | 0.200 | 0.050 | 0.050 | 0.050 | 0.150 |
| | All | 0.350 | 0.150 | 0.188 | 0.075 | 0.025 | 0.063 | 0.150 |
| | P1 | 0.325 | 0.250 | 0.125 | 0.150 | 0.000 | 0.025 | 0.125 |
| | P2 | 0.350 | 0.125 | 0.250 | 0.025 | 0.050 | 0.025 | 0.175 |
| Pooled | P3 | 0.400 | 0.125 | 0.175 | 0.000 | 0.050 | 0.125 | 0.125 |
| | P4 | 0.400 | 0.075 | 0.225 | 0.050 | 0.100 | 0.075 | 0.075 |
| | All | 0.369 | 0.144 | 0.194 | 0.056 | 0.050 | 0.063 | 0.125 |

Table A.3: Relative frequency of individual behavior in each period.

| | | Chair | | | Player 2 | | | Player 3 | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Period | A | B | C | A | B | C | A | B | C |
| *Experiment 1* | | | | | | | | | | |
| | P1 | 0.938 | 0.063 | 0.000 | 0.813 | 0.000 | 0.188 | 0.000 | 0.313 | 0.688 |
| | P2 | 1.000 | 0.000 | 0.000 | 0.375 | 0.063 | 0.563 | 0.000 | 0.188 | 0.813 |
| Performance | P3 | 0.813 | 0.188 | 0.000 | 0.563 | 0.000 | 0.438 | 0.063 | 0.125 | 0.813 |
| | P4 | 1.000 | 0.000 | 0.000 | 0.250 | 0.063 | 0.688 | 0.188 | 0.250 | 0.563 |
| | All | 0.938 | 0.063 | 0.000 | 0.500 | 0.031 | 0.469 | 0.063 | 0.219 | 0.719 |
| | P1 | 0.938 | 0.063 | 0.000 | 0.438 | 0.000 | 0.563 | 0.000 | 0.313 | 0.688 |
| | P2 | 0.938 | 0.063 | 0.000 | 0.375 | 0.000 | 0.625 | 0.063 | 0.188 | 0.750 |
| Random | P3 | 0.875 | 0.125 | 0.000 | 0.250 | 0.000 | 0.750 | 0.063 | 0.188 | 0.750 |
| | P4 | 0.938 | 0.063 | 0.000 | 0.375 | 0.000 | 0.625 | 0.063 | 0.063 | 0.875 |
| | All | 0.922 | 0.078 | 0.000 | 0.359 | 0.000 | 0.641 | 0.047 | 0.188 | 0.766 |
| | P1 | 0.938 | 0.063 | 0.000 | 0.625 | 0.000 | 0.375 | 0.000 | 0.313 | 0.688 |
| | P2 | 0.969 | 0.031 | 0.000 | 0.375 | 0.031 | 0.594 | 0.031 | 0.188 | 0.781 |
| Pooled | P3 | 0.844 | 0.156 | 0.000 | 0.406 | 0.000 | 0.594 | 0.063 | 0.156 | 0.781 |
| | P4 | 0.969 | 0.031 | 0.000 | 0.313 | 0.031 | 0.656 | 0.125 | 0.156 | 0.719 |
| | All | 0.930 | 0.070 | 0.000 | 0.430 | 0.016 | 0.555 | 0.055 | 0.203 | 0.742 |
| *Experiment 2* | | | | | | | | | | |
| | P1 | 0.900 | 0.100 | 0.000 | 0.450 | 0.000 | 0.550 | 0.000 | 0.250 | 0.750 |
| | P2 | 0.950 | 0.050 | 0.000 | 0.250 | 0.050 | 0.700 | 0.100 | 0.450 | 0.450 |
| Chair-Label | P3 | 0.800 | 0.150 | 0.050 | 0.150 | 0.050 | 0.800 | 0.150 | 0.100 | 0.750 |
| | P4 | 0.900 | 0.100 | 0.000 | 0.050 | 0.000 | 0.950 | 0.150 | 0.300 | 0.550 |
| | All | 0.888 | 0.100 | 0.013 | 0.225 | 0.025 | 0.750 | 0.100 | 0.275 | 0.625 |
| | P1 | 0.850 | 0.150 | 0.000 | 0.550 | 0.000 | 0.450 | 0.100 | 0.350 | 0.550 |
| | P2 | 0.750 | 0.250 | 0.000 | 0.250 | 0.000 | 0.750 | 0.100 | 0.300 | 0.600 |
| No-Label | P3 | 0.800 | 0.150 | 0.050 | 0.250 | 0.000 | 0.750 | 0.000 | 0.250 | 0.750 |
| | P4 | 0.800 | 0.150 | 0.050 | 0.250 | 0.000 | 0.750 | 0.050 | 0.300 | 0.650 |
| | All | 0.800 | 0.175 | 0.025 | 0.325 | 0.000 | 0.675 | 0.063 | 0.300 | 0.638 |
| | P1 | 0.875 | 0.125 | 0.000 | 0.500 | 0.000 | 0.500 | 0.050 | 0.300 | 0.650 |
| | P2 | 0.850 | 0.150 | 0.000 | 0.250 | 0.025 | 0.725 | 0.100 | 0.375 | 0.525 |
| Pooled | P3 | 0.800 | 0.150 | 0.050 | 0.200 | 0.025 | 0.775 | 0.075 | 0.175 | 0.750 |
| | P4 | 0.850 | 0.125 | 0.025 | 0.150 | 0.000 | 0.850 | 0.100 | 0.300 | 0.600 |
| | All | 0.844 | 0.138 | 0.019 | 0.275 | 0.013 | 0.713 | 0.081 | 0.288 | 0.631 |

Table A.4: Change in elicited role attractiveness and social status in Experiment 2.

| | *WTP2 – WTP1* | | | *Social Status* | | |
|---|---|---|---|---|---|---|
| | Chair | Player 2 | Player 3 | Chair | Player 2 | Player 3 |
| Chair | -0.88 | 0.08 | -0.15 | *n.a.* | 2.10 | 2.42 |
| | (0.262) | (1.000) | (1.000 ) | | (<0.001) | (<0.001) |
| Player 2 | -1.85 | 2.23 | -0.1 | -0.08 | *n.a.* | 2.08 |
| | (0.005) | (0.001) | (1.000) | (0.850) | | (<0.001) |
| Player 3 | -1.90 | 1.52 | -0.28 | -1.15 | -1.77 | *n.a.* |
| | (0.005) | (0.192) | (1.000) | (0.152) | (<0.001) | |

*Notes: Adjusted p-values of Wilcoxon signed-rank (WSR) tests in parentheses.*

the chair role (8.85). The role of player 2 was rated second highest (5.21) and participants expressed the lowest attractiveness towards the player-3 role (4.26). These ratings were in line with the elicited belief that the tie-breaking rule is decisive in 49.5% of the elections, against the realized decisiveness of 17.5% in the first period and 24% over all periods.

Results revealed a heterogeneity in the adjustment of the elicited WTP measures across player roles. Ex-post, chair participants still evaluated the chair role as the most attractive one (WTP2 chair = 8.15 > WTP2 player 2 = 5.25 > WTP2 player 3 = 3.98). Player 2s reversed their ranking and evaluated their own role more favorably than the chair role (WTP2 player 2 = 7.55 > WTP2 chair = 6.85 > WTP2 player 3 = 3.88). Player 3s also evaluated the chair role as the most attractive one ex-post (WTP2 chair = 6.93 > WTP2 player 2 = 6.65 > WTP2 player 3 = 4.40). Table A.4 reports the mean differences between post-voting and pre-voting WTP measures, as well as the results of two-sided WSR tests with Holm-Bonferroni adjusted p-values. Specifically, for each player and the elicited attractiveness measure of each role, we test whether the difference $WTP2 - WTP1$ is significantly different from zero. Most notably, our main observations remain highly significant even after adjusting p-values for 9 different hypotheses. Regular members down-shade the attractiveness of the chair role significantly, but the chair is reluctant to adjust her rating with respect to her own role or the role of regular members.

Finally, we report data from the social status questions elicited after the voting stage of the experiment. Following de Kwaadsteniet and van Dijk (2010), each participant $i$ answered two questions about *each* of the other two committee members $j \neq i$ on a 7-point Likert scale: a) "Do you believe you had a higher status than voter j?" and b) "Do you believe you had a lower status than voter j?" The measure of social status $S_{i,j}$ of player $i$ relative to player $j$ is the rating difference between the two questions. If $S_{i,j} > 0$ ($S_{i,j} < 0$), member $i$ attributes a higher (lower) social status to herself than to the other committee member $j$.

Figure A.1 shows the social status measures ($S_{i,j}$) and Table A.4 reports the corresponding

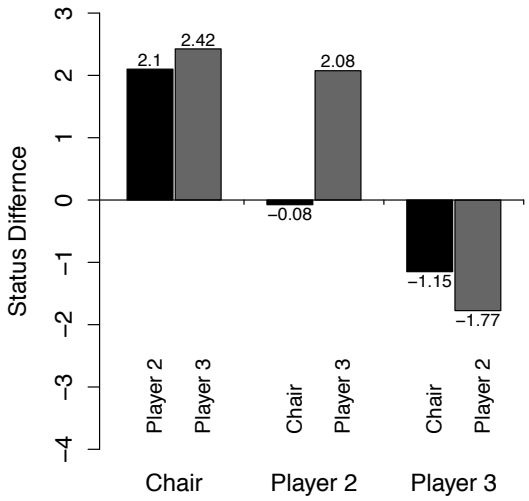



Figure A.1: Difference in social status of own role relative to other roles.

WSR tests, again adjusted for multiple comparisons. The chair attributed a much higher status to herself than to player 2 ($S_{ch,p2} = 2.10$) and player 3 ($S_{ch,p3} = 2.42$). Player 2 believed to have a social status similar to the one of the chair ($S_{p2,ch} = -0.08$) but a much higher status than player 3 ($S_{p2,p3} = 2.08$). Player 3 believed to have lower status than both the chair ($S_{p3,ch} = -1.15$) and player 2 ($S_{p3,p2} = -1.775$). Regarding the interpretation of these difference across players in social status assessments, the two channels that can both cause a positive social status in our experiment are relative payoff experiences and holding formal tie-breaking power. Recall that average earnings of player 2 were slightly higher than those of the chair, which in turn were higher than the ones for player 3. Player 2's and player 3's status assessments are compatible with relative earnings which we take as evidence that they were mostly driven by material well-being, i.e. $S_{p2,ch} \approx 0$, $S_{p2,p3} > 0$ and $S_{p3,ch}, S_{p3,p2} < 0$. The chair, however, attributed a much higher status to herself than to regular members, consistent with the lack of adjustment of the chair found already in adjustments of WTP measures.

# B Experimental instructions

*We provide a complete translation of the written instructions used in Experiment 1. The instructions for Experiment 2 were identical except for removing any reference of the effort task and player labels, as described in the main text. The instruction for the p-beauty game, which were only displayed on screen, are also provided. Figure C.2 to C.4 shows the sample screen-shots of the decision screen that accompanied written instructions. Instructions in the original language (German) are available upon request.*

## General Instructions

Welcome! Today's experiment is part of a research project investigating how people make decisions. The expected duration is one hour.

*If you have any questions, now or during the course of the experiment, please raise your hand and remain seated. An experimenter will come to you and answer your question.*

In addition to the **2 Euro** which you receive today for showing up on time, you can earn a considerable amount of money. How much money you will earn will depend on your decisions and the decisions of the participants you are going to interact with today. Therefore, it is important that you carefully read the written instructions as well as the instructions on your computer screen before you make decisions.

At the end of the experiment, you will receive the amount of money you have earned today (in addition to the 2 Euro show-up fee) **in private and in cash**.

**You are not allowed to communicate in any form with other participants.** Non-observance of this rule leads to exclusion from the experiment and you will not receive any payment.

In the experiment, which consists of several parts, you will be asked to make decisions. The experiment will conclude with a questionnaire. Please read the following instructions carefully and then answer the control questions on page 7.

(End of page 1)

## General instructions regarding the experiment

In the experiment you will participate in a number of elections. Elections takes place in a group of three voters. That is, you and two other participants form one group. The three of you decide on the outcome of the election in your group. In the following, we will call the

three voters in one group, Voter 1, Voter 2, and Voter 3.

**Casting a ballot**

In each election, **you have to cast one vote in favor of one of the three available alternatives (A, B, or C)**. So do the other two voters in your group. Voting is anonymous and takes place simultaneously. In other words, you don't know the identity of the other two voters, and you also don't know their decisions when casting your vote. After each voter has submitted her/his vote, you will receive feedback about the outcome of the election. **The alternative receiving the most votes wins the election**.

**Payoff profiles of voters**

The three alternatives can differ in terms of their desirability for each of the three voters. The desirability of an alternative is represented by the EURO amount voters receive if the respective alternative wins the election. The payoffs voters receive depend on the outcome of the election, i.e. on which alternative wins the election.

The table in Screenshot 1 below shows an example of a payoff profile of an election group (the Euro amounts in the example are different from the ones used in the actual experiment). For each election in the experiment, the structure of payoffs is the same as in the example in Screenshot 1. The structure of a payoff profile is such that for one voter, alternative A is the most desirable alternative, alternative B the second most desirable alternative, and C the least desirable alternative. For another voter, B is most desirable, C is second most desirable and A is least desirable alternative. For the third voter, C is the most desirable alternative, A the second most desirable alternative, and B is the least desirable alternative.

The payoff profile in the table reads as follows:

- **If A wins the election**, Voter 1 receives 600 Euro, Voter 2 receives 400 Euro, and Voter 3 receives 200 Euro.
- **If B wins the election**, Voter 1 receives 400 Euro, Voter 2 receives 200 Euro, and Voter 3 receives 600 Euro.
- **If C wins the election**, Voter 1 receives 200 Euro, Voter 2 receives 600 Euro, and Voter 3 receives 400 Euro.

(End of page 2)

Important: Your payoff (in Euro) only depends on which alternative wins the election. In the example above, Voter 1 receives 400 Euro if B wins independently of whether she/he

| | If alternative A wins | If alternative B wins | If alternative C wins |
|---|---|---|---|
| Voter 1 | €600 | €400 | €200 |
| Voter 2 | €400 | €200 | €600 |
| Voter 3 | €200 | €600 | €400 |

Screenshot 1: Example payoff profile.

actually voted for B or not.

**Voting method**

**The alternative which receives the most votes wins the election**. In case of a tie among alternatives, the following rule applies: One Voter assumes the role of the "chairman" in the voting group (see description below). The winner of the election in case of a tie is then the alternative the chairman has voted for.

Example: Suppose one voter votes for A, one voter votes for B and one voter votes for C, that is, each alternative receives exactly one vote. Assume that the chairman has voted for alternative C, then alternative C is the winner of the election.

**Course of the experiment**

Now that you know the general rules of the elections in the experiment, let's summarize the course of the experiment:

- Before the start of the first election in the experiment, you will be asked to work on a task that influences the probability of you being the "chairman" in the elections (the task and the exact rules on how the chairman role is determined will be explained on the next page in detail).
- After this task, you will receive information whether you are the "chairman" or not. If you are assigned to the chairman role, you will assume the chairman role in every election.
- Then, you participate in a series of four elections. In each of the four elections, you will meet two other participants (details regarding the elections are provided on page 6).

*If you have any questions regarding the instructions or the course of the experiment, please raise your hand and remain seated. An experimenter will come to you and answer your question.*

(End of page 3)

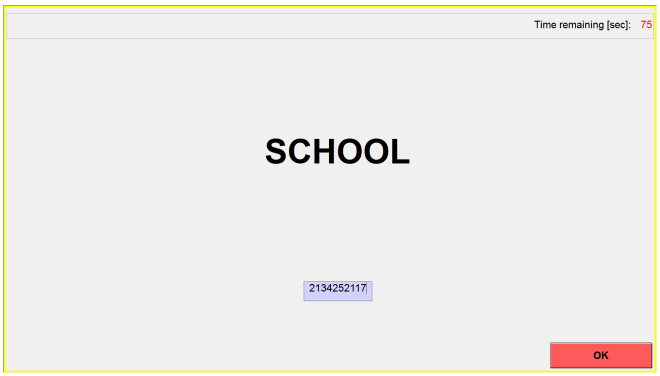
## Explanation: How the chairman role is assigned

Whether you will be chairman in all elections or not depends either:

- on your performance in the encoding task, or
- is determined randomly.

### Encoding task

Before the first election, you will participate in a so-called encoding task. Your performance in this task influences the probability of you becoming "chairman" or not: the more words you encode correctly, in comparison to the other participants, the higher is the likelihood of you being assigned to the chairman role. The encoding task is the same for all participants. Different words are presented to you on screen, one after another. Your task is to replace each letter in the presented word with a number. The encoding table (see Table 1 on page 8) shows for each letter the corresponding number with which each letter has to be replaced in a given word.




Screenshot 2: Example encoding task.

Example: Screenshot 2 displays the word SCHOOL. From the encoding table (see page 8), you can see that the letters of the word SCHOOL need to be replaced by the following numbers: S=13, C=14, H=22, O=21, O=21, L=3. To correctly encode this word, enter the number 13 14 22 21 21 3 into the corresponding text field displayed on screen and then click OK.

When you encoded a word correctly, the next word will be displayed on screen. In total, you have **7 minutes** for the task. All participants are presented with the same words in the same order. The computer will record how many words you encode correctly in the task.
(end of page 4)

**Random draw**

After the encoding task the computer draws randomly how the role of the chairman is assigned. The role will either be assigned **according to performance in the encoding task (with probability 50%)** or will be assigned **randomly (with probability 50%)**:

- **If the role of the chairman is assigned according to performance in the encoding task**, it means according to the number of words correctly encoded. If your number of correctly encoded words belongs to the top 33% of all participants in the encoding task you will be assigned the role of the chairman. (In case that two or more top performing participants encoded the same number of words correctly, each top performer has the same chance of becoming the chairman). If your number of words encoded does not belong to the top 33% of all performances, you will not be assigned the role of the chairman in the experiment.

- **If the role of the chairman is assigned randomly**, it is assigned independently of the number of words you encoded correctly. A participant is then randomly assigned to the role of the chairman for all elections with a probability of 33% (since only one voter in a group will be chairman).

After completing the encoding task, every participant will be informed about how many words she/he encoded. **Every participant will be informed about whether the chairman role is assigned randomly by the computer or according to performance in the encoding task**. Please notice that the outcome of the random draw is the same for every participant you interact with today (i.e. for each participant you interact with today the role will be assigned either by a random draw or according to the performance).

Furthermore, each participant will be privately informed whether she/he is chairman or not. **The role of the chairman is assigned before the first election and remains fixed for all elections**. In other words, if you are chairman, you are chairman in each election. If you are not the chairman, you will be not be chairman in any of the elections. The table displaying the payoff profile will also indicate which of the three voters in your group takes the chairman role in the election.

**Note: The more words you encode in the encoding task within 7 minutes, the higher are your chances of becoming chairman in the elections**. That is, if you would like to be chairman, the best you can do is to encode as many words as possible. If you don't want to be chairman, the best you can do is to encode as few words as possible.

(end of page 5)

## Elections and payoffs

You will participate in a **total of four** elections. In each election, you will interact with **two other participants with which you haven't interacted before**. That is, you will never interact with a participant more than once.

At the beginning of each election, you are told whether you are Voter 1, Voter 2, or Voter 3 and informed about who of the three voters assumes the chairman role in your voting group. You will also see this information in the table summarizing the payoff profile of the election. As already mentioned, your task in an election is to vote for one of the three available alternatives, A, B, or C. So do the other two voters in your group. The outcome of the election depends on your decision and the decisions of the other two voters. Therefore, it is important to pay attention to the table displaying the payoff profile of all voters before taking your decision.

Once all voters have made a decision, the number of votes each alternative received will be displayed. The alternative with the most votes wins the election. Only in case of a tie between two or more alternatives, the winner of the election is the alternative the chairman has voted for.

Your payoff from an election only depends on the outcome, i.e. the winning alternative in the election. It is independent of whether you casted a vote for the winning alternative or not. Each voter receives the payoff specified in her/his payoff profile for the winning alternative. Then, another election starts. Please notice that the payoff profile can change with each election. Please recall that the chairman role is assigned before the first election and remains fixed over the course of all four elections.

## How your payoff is determined

After all four elections, the computer will randomly draw one of the four elections. The payoff you received in this randomly drawn election is the Euro amount you will earn (and paid out) in the experiment. This random draw will take place at the end of the experiment, i.e. after you made all your decisions. Of course, the 2 Euro show-up fee will be added to the realized Euro amount from the chosen election.

*Are there any questions? If so, please raise your hand and remain seated. An experimenter will come to you and answer your question.*

(end of page 6)

## Control questions

Please answer all control questions. If you have any questions, please raise your hand and remain seated. An experimenter will come to you and answer your question.

Question 1: When and how often will you engage in the encoding task? (please mark the correct answer)

O Before each election.

O Only once before the first election.

Question 2: In which case does the chairman's vote decide the winning alternative in an election? (please mark the correct answer)

O Always.

O Only if there is a tie.

Question 3: If I want to be the chairman, the best I can do is to encode as many words as possible, because during the encoding task I don't know whether the role of the chairman is assigned randomly or by performance in the encoding task (for each participant I interact with). (please mark the correct answer)

O True.

O False.

Question 4: If I am assigned to the role of the chairman I maintain this role for every election today. (please mark the correct answer)

O True.

O False.

Question 5: In each of the four elections, I interact with two other participants I haven't interacted before. (please mark the correct answer)

O True.

O False.

Question 6: The payoff I receive for one election only depends on: (please mark the correct answer)

O The winning alternative of the election.

O The alternative I voted for in the election.

Question 7: Consider the payoff profile example shown in screenshot 1 (page 2). If you are voter 2 and alternative B wins the election you receive: (please mark the correct answer)

O 200 Euro.

O 400 Euro.

O 600 Euro.

(end of page 7)

## Encoding table

| Letter | Number |
|---|---|
| A | 8 |
| B | 12 |
| C | 14 |
| D | 10 |
| E | 9 |
| F | 6 |
| G | 24 |
| H | 22 |
| I | 7 |
| J | 5 |
| K | 11 |
| L | 3 |
| M | 18 |
| N | 1 |
| O | 21 |
| P | 16 |
| Q | 23 |
| R | 2 |
| S | 13 |
| T | 19 |
| U | 25 |
| V | 4 |
| W | 26 |
| X | 17 |
| Y | 20 |
| Z | 15 |

Figure A.2: Encoding table.

(end of page 8 and end of written instructions)

## P-beauty game [Experiment 2 only – onscreen]

*All participants in a session played a p-beauty contest after answering the post-voting WTP2 questionnaire, but before feedback about which period was selected for payment in the voting stage was provided.*

**You will now participate in a guessing game.**

In this game, you can win an additional €10 on top of your earnings from the voting part. Independent of the decisions you are about to make in the guessing game you will receive your earnings from the voting part in any case.

Here are the rules of the guessing game: You and the other 23 participants in the session will participate in the guessing game. Your task is to guess a number between 0 and 100. The participant whose guess is closest to the average over all guesses multiplied by two thirds (2/3) will win €10. After all participants submitted their guesses, we will calculate the average over those guesses. We will then multiply the average guess with 2/3 and determine the winner(s) of the game. The €10 will be split equally among all winners in case of a tie.

Please provide your guess here and click on NEXT (numbers up to three decimals are allowed): [INPUT BOX]

# C Screenshots

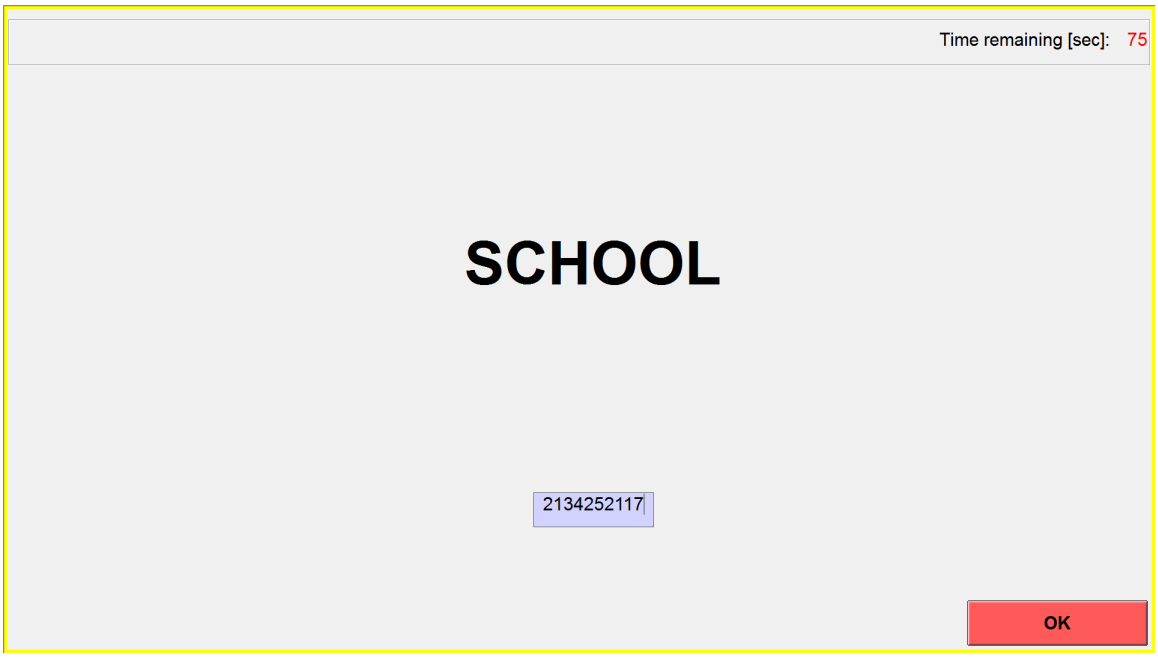



Figure C.1: Screenshot real-effort task (Experiment 1).

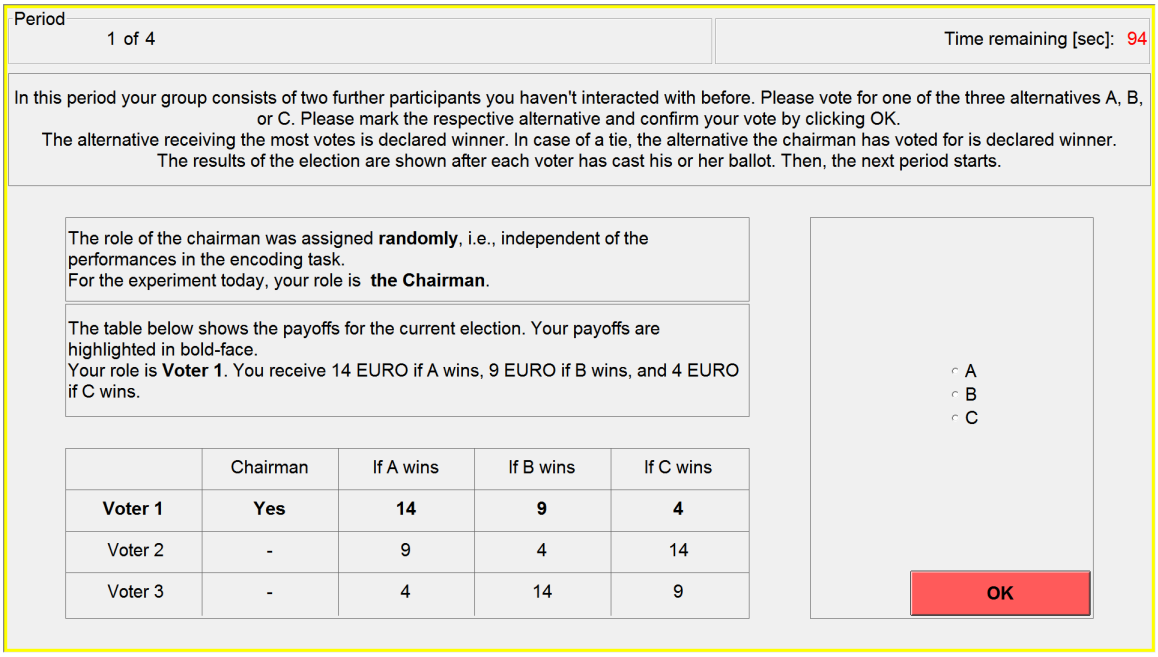



Figure C.2: Screenshot voting stage (Experiment 1).

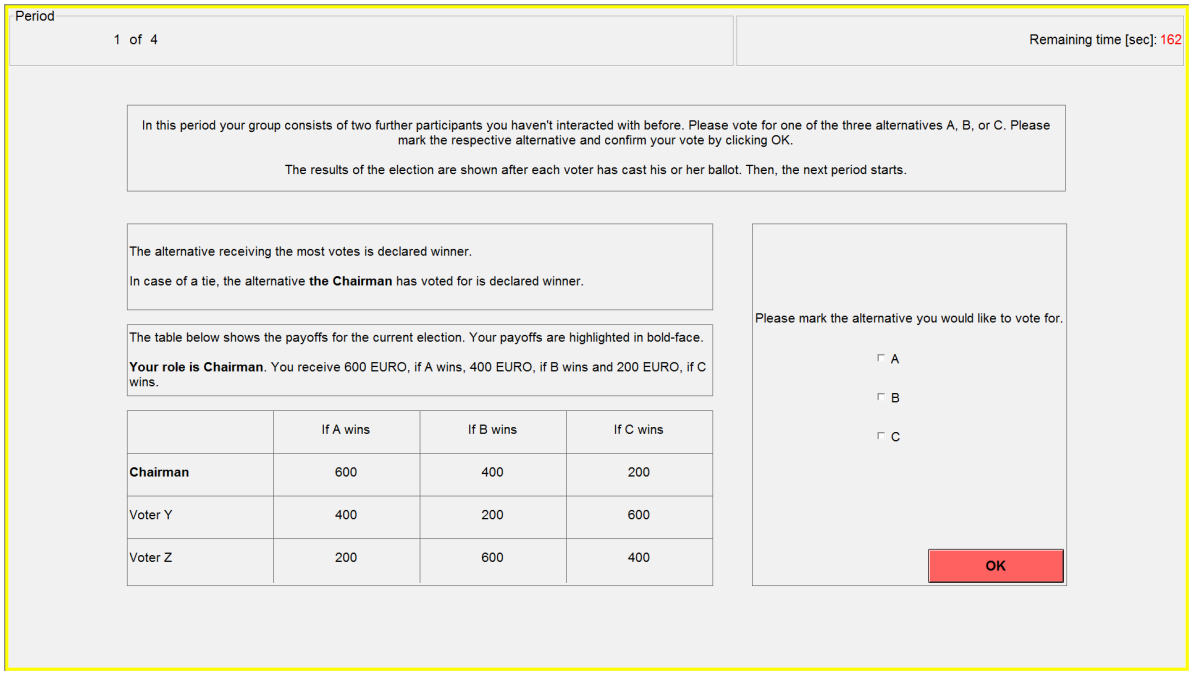



Figure C.3: Screenshot voting stage (chair-label treatment, Experiment 2).

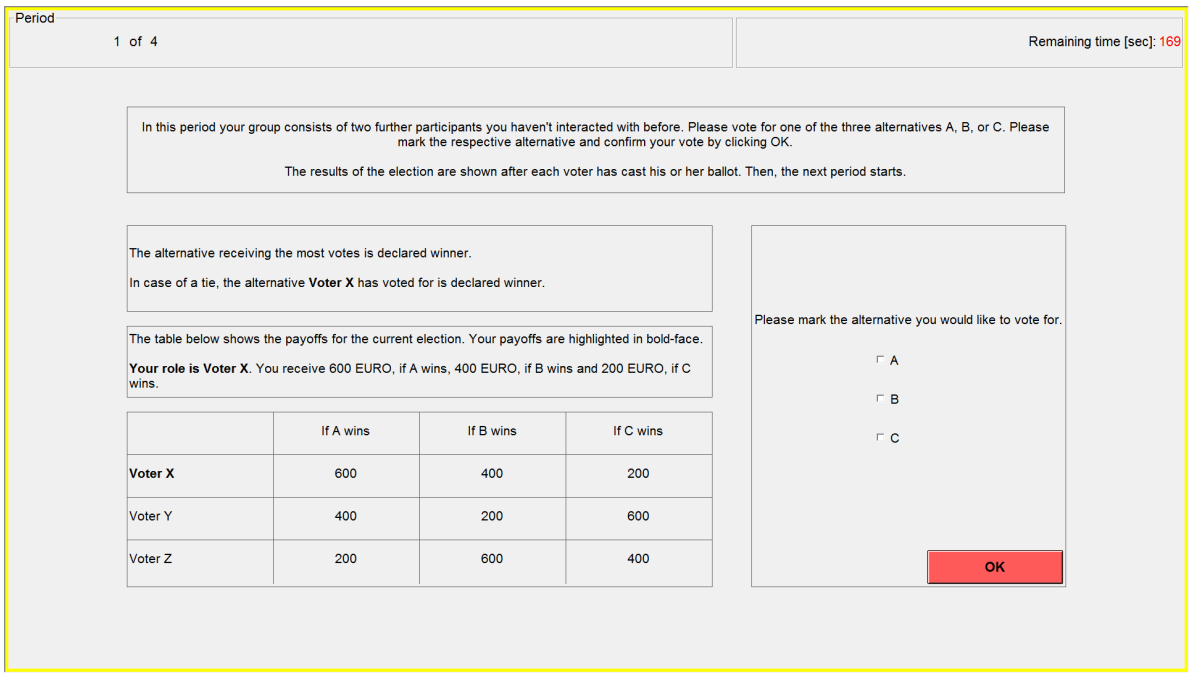



Figure C.4: Screenshot voting stage (neutral-label treatment, Experiment 2).