Tie-Breaking Power in Committees*

Dura-Georg Granić[◇] Alexander K. Wagner[▲]

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Abstract

We investigate the impact of asymmetric tie-breaking 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

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◇ 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 outcome-space distortions induced by the asymmetric tie-breaking rule, which is at odds with equilibrium predictions under full rationality. From the perspective of optimal institutional design, our results suggest that it 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 committee members deviate from fully rational and strategic behavior.

[1] There exist a number of different definitions of power, or authority, in the social sciences (for an overview, see Russel, 1938; Weber, 1978; Bartlett, 1989). 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).

In the analysis of the committee model, we follow the definition proposed in Aghion and Tirole (1997) and 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. 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 vote of 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. Our results reveal that the chair benefits to a much larger extent from the indirect effect of holding tie-breaking power than from breaking ties directly.

We demonstrate in Experiment 1 the underlying mechanism through which the indirect effect operates by introducing a treatment in which committee members can earn the decision right to break ties through performance in an unrelated real-effort task (Erkal et al., 2011). Manipulating the legitimacy of holding tie-breaking power changes the size of the indirect effect and shows that legitimate formal power (e.g. Silverman et al., 2014) lures members into voting for the chair's preferred alternative, even against strong monetary incentives.[3] In Experiment 2, we show that the size of the indirect effect cannot be explained by the focality of the chair's preferred alternative. Overall, our level-k interpretation accommodates frequently observed non-equilibrium strategy profiles favorable to the chair. These profiles realize when regular members exhibit low levels of strategic reasoning, which is supported by

[3] Silverman et al. (2014) show that experimental behavior of group members in a public goods game with punishment depends strongly on the perceived legitimacy of authority of the group leader. 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).

an exogenously elicited measure of strategic sophistication of committee members.

The most-closely related work, and the only one we are aware of that allows explicitly for the possibility of 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.

The advantageous indirect effect tie-breaking power is not restricted to our particular tie-breaking rule but exist 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 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]

[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.

[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.

2 Theoretical background

We present here the setup of the committee voting model and derive behavioral predictions under varying assumptions on committee members' level of strategic sophistication. The latter allows us to characterize conditions under which formal tie-breaking power implies real power over committee outcomes. While we confine our analysis to tie-breaking power, it is worth noting that the logic below is equivalent to one in a weighted-voting model in which the chair holds an additional, arbitrarily small, voting weight of $0 < \epsilon < 1$ on top of the normalized weight of 1 of regular committee members. As in 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 and 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. 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 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

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

	Preference profile	Nash equilibria						Level-k predictions		
		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

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 vote sincerely.[7] 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),

[7] 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 the $L0$ profile, and so on (cf. Table 1).

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.

3 Experiment 1

Experiment 1 investigates the influence of asymmetric tie-breaking power on decision-making in committees. We also provide causal evidence that holding tie-breaking power legitimately changes behavior of regular committee members to the benefit of the one holding formal power to break ties. In Section 2, we have shown that the formal power of the chair does not translate into real power if committee members vote rational and strategically sophisticated. It is however well known that decision makers deviate often from sophisticated behavior or expect others to do so. Our experimental implementation of the game allows to relate the level of the real power of the chair to the strategic sophistication 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 authority (of holding tie-breaking power) as a treatment variable by conditioning the assignment of the chair role on performance. After the 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'

Table 2: Summary of treatments.

	Chair		
	assignment	contest	label
Experiment 1	*performance*	yes	yes
	random	yes	yes
Experiment 2	random	no	*yes*
	random	no	*no*

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.[8]

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. 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. This design, as argued in Dal Bó et al. (2010), 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.

Committee game. Participants played the voting game described in Section 2 for 4 rounds 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

[8] 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).

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 round and the order of presentation was randomized. At the end of a round, 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.

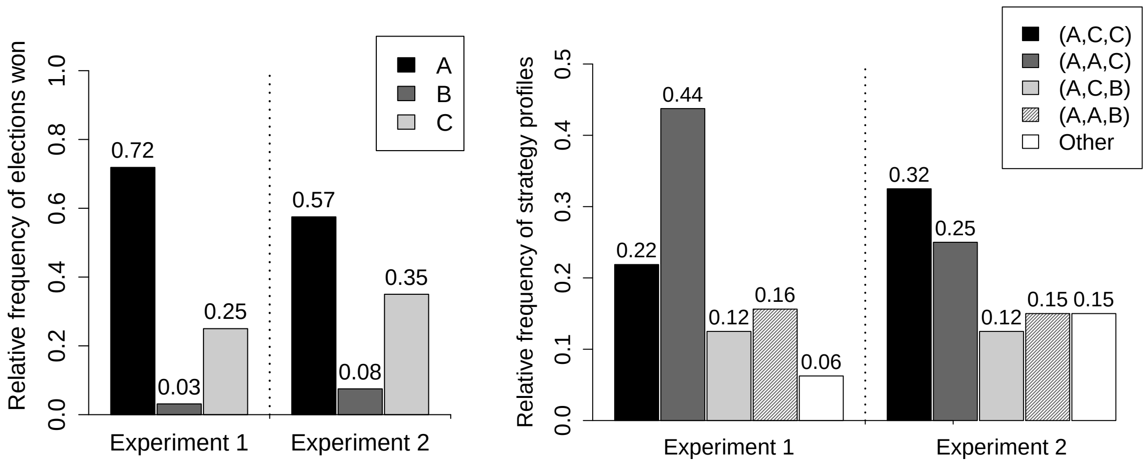
Procedural details. The experiment was conducted at the experimental economics laboratory (Lakelab) of the University of Konstanz. We recruited 96 participants (48 females, average age 23) from a student pool using the online recruitment system ORSEE. All parts of the experiment were run with the software z-Tree (Fischbacher, 2007). Each of the 4 sessions was comprised of 24 subjects and 4 independent matching groups per treatment. 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 represents the chair's most-preferred alternative, B and C represent the most-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. Unless noted otherwise, we report first-round behavior to not confound treatment with learning effects.[9] In stark contrast to the theoretical predictions under strategic voting, which sees alternative C win 100% of the elections, alternative A was by far the most frequently implemented outcome. A won 72% of all elections in the first period whereas C won only 25%. The winning frequency of A was also above the benchmark of one third one expects if ties were broken randomly (two-sided exact binomial test, $N = 32$, wins $A = 23$, $p < 0.001$).[10] The sizable advantage of tie-breaking power is corroborated

[9] Although we focus is on first-period behavior, all reported results are qualitatively robust to incorporating data from all periods into the analysis, see the Discussion (in Section 5) and Appendix A.2 for additional results.

[10] The winning probability of 1/3 is the appropriate benchmark for two reasons. First, note that the correct counterfactual is a situation in which the committee plays the same game but with a symmetric random tie-breaking rule. Under this rule, the expected probability of winning is in equilibrium 1/3 for each alternative. We did not conduct a treatment with the symmetric random tie-breaking device as it changes the equilibrium predictions of the game (in this case, an additional pure strategy Nash equilibrium, (B, C, B), which survives



(a) Outcomes

(b) Strategy profiles

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

by the chair's normalized average earnings which were significantly different from the €10 one would expect if ties were broken randomly (two-sided Wilcoxon signed-rank (WSR) test, $N = 32$, $z = 2.694$ $p = 0.007$).[11] In line with our theoretical considerations, B was empirically irrelevant and won a mere 3% of the elections.

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 only 12.5% of the elections, all of which come from 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 44% and 16% of the elections, respectively. In either case, player 2 would have been weakly

deletion of WDS is added). This in turn renders a direct comparison of the strategic sophistication in behavior between treatments with asymmetric and symmetric tie-breaking infeasible. Second, our choice of testing observed behavior against the symmetric random tie-breaking rule is much more conservative than testing against strategically sophisticated voting, in which the expected probability of winning is 0% for A.

[11] To compare earnings across participants, we normalized realized payoffs to the vector $(15 \succ 10 \succ 5)$ and find that the chair (€12.34) and player 2 (€11.09) earned more than player 3 (€6.56). A two-sided Wilcoxon rank-sum (WRS) test revealed that the chair's payoff is significantly different from the payoff of regular members ($N = 96$, $p < 0.001$). As non-chair participants had an ex-ante equal chance of being assigned to either player-2 or player-3 role, the test corroborates that chair's advantage from holding tie-breaking power.

Table 3: Relative frequency of strategy profiles by treatment.

	(A, C, C)	(A, A, C)	(A, C, B)	(A, A, B)	*Other*
Experiment 1					
Performance	12.50	56.25	06.25	18.75	06.25
Random	31.25	31.25	18.75	12.50	06.25
Pooled	21.88	43.75	12.50	15.63	06.25
Experiment 2					
Chair-label	35.00	35.00	15.00	05.00	10.00
Neutral-label	30.00	15.00	10.00	25.00	20.00
Pooled	32.50	25.00	12.50	15.00	15.00

better off by voting for her most-preferred alternative, C. Finally, (A, C, C) occurred in 22% 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. We summarize our results as follows.

Result 1. *The formal power of the chair to break ties is real. In contrast to the prediction under rational strategic voting, the alternative preferred by the chair was the most-frequently implemented alternative in the committee. Observed strategy profiles reveal that the chair's advantage is largely driven by indirect effects of holding tie-breaking power and not 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 38% of the strategy profiles constitute a Nash equilibrium in the game (combined frequency of (A, C, C) and (A, A, B); unanimous profiles did not occur) but equilibrium analysis fails to account for the modal profile (A, A, C). The level-k model in Section 2 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 78% of the data, see Table 3. Voting behavior is highly compatible with limited strategic reasoning as only $L1$-types of player 2 choose alternative A (a necessary condition for the modal 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 (0% for player 2 and player 3, 6% for the chair). 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 94% of the elections. Player 3 and player 2 behavior is consistent with IEWDS in 69% and 38% of the elections, respectively. Player 3 was 1.8 times more likely to implement the strategy prescribed by IEWDS than player 2. The large 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.

Result 2. *Behavior of regular committee members is characterized by limited strategic sophistication (level-k reasoning). They exclude weakly dominated strategies, but fail to apply this procedure iteratively. This failure is especially pronounced in player 2's behavior, which is the root of the chair's advantage in the committee.*

Examining player 2 behavior in light of potential treatment differences below further elucidates the driving forces of the indirect effects of formal tie-breaking power. In many committees, the chair position entails a natural notion of authority or leadership (for example in monetary policy boards, see Blinder and Morgan, 2005, 2008). In the experiment, we assigned tie-breaking power randomly in one treatment and based on performance in the real-effort task in the other. We did so because perceived legitimacy is known to be a vital element of how people react to formal authority (de la Boétie, 1975; Weber, 1978; Silverman et al., 2014). Assuming that performance in the encoding task legitimized formal tie-breaking power of the chair, our hypothesis was that player 2 would be swayed by legitimate authority in the performance treatment into voting more often for the chair's preferred alternative A. For player 3, this argument has no bite as alternative A is her least-preferred alternative and a WDS strategy. Indeed, player 3 never voted for A in any of the treatments.

Voting behavior of player 2 reveals a strong effect of legitimate authority attached to the chair's position and thereby on the magnitude of the indirect effect of holding tie-breaking power. Player 2 voted almost twice as often for A in the performance treatment (81% of the elections) than in the random treatment (44%). The difference is significant according to 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

Table 4: Relative frequency of individual behavior by treatment.

		Experiment 1			*Experiment 2*		
		Performance	Random	Pooled	Chair-label	Neutral-label	Pooled
Chair	A	93.8	93.8	93.8	90.0	85.0	87.5
	B	06.2	06.3	06.2	10.0	15.0	12.5
	C	00.0	00.0	00.0	00.0	00.0	00.0
Player 2	A	81.3	43.8	62.5	45.0	55.0	50.0
	B	00.0	00.0	00.0	00.0	00.0	00.0
	C	18.8	56.3	37.5	55.0	45.0	50.0
Player 3	A	00.0	00.0	00.0	00.0	10.0	05.0
	B	31.2	31.2	31.2	25.0	35.0	30.0
	C	68.8	68.8	68.8	75.0	55.0	65.0

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 ($N = 24$, $p = 0.042$). A probit regression on the propensity of choosing A by player 2 strongly corroborates our findings, see model 1 in Table 5. Legitimate authority significantly increased player 2's likelihood to side with the chair. The result is robust against the inclusion of various controls as shown in Table A.3.

Result 3. *Player 2 chooses the chair's preferred alternative significantly more often if the chair holds tie-breaking power legitimately than under randomly assigned tie-breaking power.*

The treatment comparison provided causal evidence that legitimate authority 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-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 $m = 36.27$, $SD = 7.3$) and indicated a high ex-ante willingness to become chair prior to the voting game (WTP1: $m = 8.22$, $SD = 1.70$). 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 12.5%.

4 Experiment 2

We established with Experiment 1 that the formal power to break ties is beneficial for the chair and that legitimate authority lures regular members, against strong material self-interest, into siding with the chair. 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 payoff experience.

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 which labeled the chair neutrally (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.

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. In total we ran 5 sessions, each with 24 subjects and 4 independent matching groups per treatment. 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. As in Experiment 1, we report first-period behavior unless noted otherwise. It can be seen that the distributions of outcomes and of strategy profiles are highly comparable between experiments. Consistent with Experiment 1, A was the most successful alternative winning 57% of the elections in the first period, followed by C with 35% and B with 8%. The chair's advantage was sizable, with A's winning frequency significantly different from the benchmark prediction of 1/3 under random tie-breaking (two-sided exact binomial test, $N = 40$, $p = 0.002$).[12]Alternative A was mostly implemented to due *indirect effects* of holding tie-breaking power: profiles (A, A, C) and (A, A, B) account for 25% and 15% of the strategy profiles, respectively, and together make up about 70% of all implementations of A.

Table 3 and 4 reveal that differences in behavior observed between experiments were mainly driven by the effect of legitimate authority 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 as shown in the probit regression in model 3 of Table 5. Pooling data over experiments, there is no difference in the probability of player 2 to vote for A between the random treatment (Experiment 1) and both chair-label and neutral-label treatments (Experiment 2). We conclude that the labeling 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 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, 70% are consistent with level-k and 55% with Nash. Moreover, the exogenous measure of strategic sophistication elicited in a p-beauty game is predictive of player 2's behavior in our voting

[12] The chair (€11.13) and player 2 (€11.38) earned more than player 3 (€7.50). A two-sided WRS test revealed that the chair's payoff is significantly different from the payoff of regular members ($N = 120$, $p = 0.044$). Analogous to our findings from Experiment 1, chair participants benefited from holding power in Experiment 2.

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

	(1) Experiment 1		(2) Experiment 2		(3) Pooled	
DV Vote For A	Coefficient	Marginal	Coefficient	Marginal	Coefficient	Marginal
Performance	1.044**	0.375**			1.044**	0.375**
(Experiment 1)	(0.480)	(0.158)			(0.480)	(0.158)
Chair-label					0.032	0.013
(Experiment 2)					(0.422)	(0.167)
Neutral-label			0.251	0.100	0.283	0.113
(Experiment 2)			(0.397)	(0.157)	(0.422)	(0.167)
Constant	-0.157		-0.126		-0.157	
	(0.315)		(0.281)		(0.315)	
# of observations	32	32	40	40	72	72
Pseudo R^2	0.117		0.007		0.066	

Notes: Reported numbers are coefficients and average marginal effects of panel probit regressions with random-effects. Standard errors in parentheses. Dependent variable in all models is the dummy Vote for *A. Data includes player 2's first-period behavior. Model (1) uses data of Experiment 1 and random treatment as baseline category, (2) uses data of Experiment 2 and chair-label treatment as baseline category, and (3) uses data of Experiment 1 and 2 and random treatment as baseline category.* *** *significant at the 1% level,* ** *significant at the 5% level,* * *significant at the 10% level.*

game (e.g. Carpenter et al., 2013; Gill and Prowse, 2016). Probit regressions in Table A.4 shows that a player 2, if classified as type $L1$ in the p-beauty game, has a significantly higher propensity to vote for alternative A ($p = 0.005$) than other types of iterated reasoning. Taken together, Experiment 2 strongly confirms the robustness of previous results.

Result 4. *Experiment 2 confirms the main behavioral findings of Experiment 1 on all dimensions (cf. Result 1). Limited strategic sophistication of committee members is a necessary condition for turning formal tie-breaking power into real power for the chair (cf. Result 2).*

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). 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. Results show that player 2 chose A with a frequency of 45% in the *chair-label* treatment and with 55% in the *neutral-label* treatment. We cannot reject the null hypothesis of no difference between treatments according to a one-sided Fisher's exact-Boschloo test ($N = 40$, $p = 0.376$). The probit regression on player 2s' likelihood of choosing A, see model 2 in Table 5, confirms the insignificance of the treatment effect. We also do not detect any treatment differences in individual behavior for the chair or for player 3.[13] The driving factor behind committee behavior is the misperception of the strategic incentives caused by the asymmetry of formal tie-breaking power (and not the labeling of the chair).

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

A key ingredient for our analysis was to establish the real power of the chair relative to the theoretical benchmark of fully rational and 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 49.5% exceeded the actual frequency of 17.5% in first-period elections. 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 rounds, 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 for player 3.[14] The inconsistency between chair and player-2 role rankings in terms of stated attractiveness (WTP1) elicited before the voting stage and payoff experiences give rise to an interesting set of predictions with regard to the post-voting attractiveness measure (WTP2). If WTP2 statements were guided by relative earnings, we should expect a post-voting decrease for the chair role and an increase 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. It reveals a heterogeneous pattern in attractiveness adjustments over time. 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 player-2 role. The chair's re-evaluations followed a different pattern. She is more conservative in revising her initial statement regarding her own role and does not

[13] The difference in the chair's frequency of voting for A is not significant between the chair-label (0.90) and neutral-label treatment (0.85) according to a one-sided Fisher's exact-Boschloo test ($N = 40$, $p = 0.500$). 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 0.75 in the chair-label treatment and with 0.55 in the neutral-label treatment, the difference is not significant (one-sided Fisher's exact-Boschloo test, $N = 40$, $p = 0.160$). In accordance with these results, there is no significant difference in outcome distributions between treatments (pooling B and C into one category, one-sided Fisher's exact-Boschloo test, $N = 40$, $p = 0.500$).

[14] Pairwise two-sided WSR and WRS tests show that the reported differences in elicited WTP1s and in average earnings of player roles were statistically highly significant ($N = 120$ for the former and $N = 80$ for the latter, all adjusted $p < 0.001$). Unless otherwise noted, we adjust p-values according to Holm-Bonferroni to account for multiple hypothesis testing.

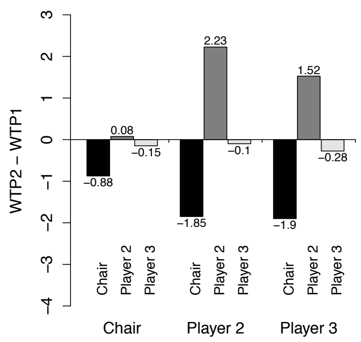


Figure 2: Change in elicited role attractiveness.

adjust for the attractiveness of player 2 ex-post.[15]

Result 6. *There is a pronounced difference in the role attractiveness elicited from regular committee 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 fails to acknowledge the relative attractiveness of player 2.*

While the positive view of the chair regarding her own role is partially consistent with observed boundedly-rational behavior of committee members, it cannot explain the asymmetry in the belief adjustments ($WTP2 - WTP1$) we observe across player roles. The heterogeneous integration of payoff experiences into post-voting attractiveness measures is however 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). Results of ex-post elicited social status is also consistent with this interpretation. Irrespective of relative average earnings in the committee, chair members attributed a higher social status to themselves than to regular members.[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 in terms of WTP2 (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 is caused either by the relative performance in the committee or by holding the formal power to break ties. The ex-post elicited social status perfectly reflects the differences across player 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 can be found in Appendix A.3.

5 Discussion

The effects of the chair's 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 presented in Section 2 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 of limited strategic sophisticated in behavior and related 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 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 larger 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 the direct effect as well as the indirect effect into account. Obviously, the direct effect of exercising tie-breaking power decreases in the total number of committee members only if the distribution of voting weights decreases the likelihood of a three-way tie. 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 in voting, 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' for each player type as shown in Table 4, the total effect in our experiments clearly favors the chair.

In this first step in understanding the consequences of formal tie-breaking power in committees, we aimed for a setting of analytical clarity. The simple one-shot voting game allowed to parsimoniously relate behavior of participants to the level of strategic sophistication (WDS, IEWDS, and level-k). This is possible because *any* possible outcome in 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). An additional reason for restricting the main analysis to first-period behavior is that the four one-shot observations per treatment are not sufficient for a clean separation of treatment and learning effects. All behavioral results however remain qualitatively robust to analyzing average behavior or repeated one-shot behavior, as shown in Appendix A.2. Apart from the observation that the chair's real power is weakly decreasing over time, our setup allows no conjectures regarding long-run effects on committee behavior. Moreover, the identification of the underlying mechanism (i.e. strategic sophistication) would be much more challenging in a repeated game due to the multiplicity of subgame-perfect equilibria.

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 again severely limited even if the same members partake repeatedly in the committee.

Finally, it is instructive to point out that the results in the constant-sum one-shot committee cannot be explained by focal point theory (Schelling, 1960). The reason is that the 'chair' cue does not provide, as required by focal point theory, 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). Treatment results in Experiment 2 corroborate that the real power of the chair is not driven by the salience, or focality, of the chair's preferred alternative. Results go even slightly in the opposite direction, with player 2 choosing A more often in the neutral treatment. In summary, observed behavior in the one-shot committee is fully explained by the level of strategic sophistication of committee members and their reasoning about the limited rationality of others.

6 Conclusion

There exists a remarkable variety in how the formal decision right to break ties is allocated across 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 two 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 not 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 the indirect effect through changing the legitimacy of holding tie-breaking power. Our main treatment variation provides causal evidence that formal tie-breaking power, if perceived as legitimate, entices regular members into siding with the chair more frequently. 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 possess 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 (see, e.g. Blinder and Morgan, 2008). 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 the understanding of decision making in committees.

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(FOR ONLINE PUBLICATION)
Appendix
for
"Tie-breaking Power in Committees"

A Additional results

A.1 Distortion of outcome space

The possible implications of limited strategic behavior on committee decisions can be seen by considering the change in the outcome-space of the game induced by replacing the symmetric random tie-breaking rule with the asymmetric tie-breaking rule of the chair. The outcome-space representation is reminiscent of the approach taken in models of a priori voting power (for an excellent overview, see Laruelle and Valenciano, 2008; Holler and Nurmi, eds, 2013). Neglecting strategic behavior, the introduction of an ex-ante asymmetric tie-breaking power favors the chair in comparison to a random (symmetric) tie-breaking rule, under which each member's most-preferred alternative wins in 9 of the possible 27 voting profiles. Given the tie-breaking power of the chair, the chair's most-preferred alternative wins in 13 out of the 27 possible profiles. Put differently, the chair's vote coincidences with the committee's implemented outcome in 21 out of the 27 distinct voting profiles, the vote of a regular member coincides with the outcome in only 15 profiles. Of the 21 implementations favoring the chair's vote, 6 are *direct effects* of exercising tie-breaking power after a three-way tie. We refer to the remaining implementations as *indirect effects* of holding tie-breaking power, which become more likely the less strategically sophisticated behavior in the committee is (see Section 2).

A.2 Robustness analysis

We provide here a number of robustness results related to main findings in Experiment 1 and Experiment 2. We establish that results do not depend on reporting first-period data but persist if behavior of later periods is taken into account. Tables A.1 and A.2 report observed strategy profiles and individual vote frequencies over all four periods. Taking all four periods into account, alternative A was the strongest alternative winning 56% and 48% of elections in Experiment 1 and Experiment 2, respectively. Alternative C won 40% of elections in Experiment 1 (45% in Experiment 2) while alternative B won only 4% (8%) of elections. Election outcomes are highly comparable across experiments, with realized outcomes distorted towards A and C. A's winning frequency is substantial in comparison to the prediction under strategically sophisticated voting and significantly higher than the benchmark of one third which would obtain under the random symmetric tie-breaking rule.

Table A.1: 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	32.81	35.94	7.81	10.94	3.13	1.56	7.81
Random	40.63	28.13	14.06	4.69	4.69	4.69	3.13
Pooled	36.72	32.03	10.94	7.81	3.91	3.13	5.47
Experiment 2							
Chair-label	38.75	13.75	20.00	3.75	7.50	6.25	10.00
Neutral-label	35.00	15.00	18.75	7.50	2.50	6.25	15.00
Pooled	36.88	14.38	19.38	5.63	5.00	6.25	12.50

Exact binomial tests run at the level of independent matching groups confirm this observation. Assuming that A's winning frequency is equal to one third under the null hypothesis, we would expect A to win 5 or 6 out of 16 elections held within a matching group. We observed 6 out 8 matching groups in Experiment 1 and 9 out of 10 in Experiment 2 with A winning strictly more than 6 elections. Under the null, the probability to observe our data or more extreme outcomes is given by 0.006 and 0.007 for Experiment 1 and Experiment 2, respectively. Similarly, we observe 8 out of 8 matching groups in Experiment 1 and 6 out of 10 groups in Experiment 2 with A winning strictly more than 5 elections. Under the null, the probability to observe our data or more extreme outcomes equals 0.001 in Experiment 1 and 0.025 in Experiment 2, respectively. Applying Holm-Benferroni to correct for family-wise error rates does not alter the significance of the tests. We reject the null hypothesis and conclude that A's winning frequency is systematically higher than one third.

The most frequently observed strategy profile, considering all periods, was (A, C, C) which we observed in 37% of the elections in each experiment, see Table A.1. Implementations of alternative C were almost uniquely associated with this strategy profile. In contrast, only a relatively small number of implementations of A were a direct consequence of the chair's tie-breaking power. A three-way tie in which the chair exercised her power in favor of A occurred in 12% (Experiment 1) and in 20% (Experiment 2) of elections. The vast majority of the remaining implementations of A stemmed from the two strategy profiles in which player 2 cast a vote in favor of A, strategy profiles (A, A, C) and (A, A, B). (A, A, C) occurred in 32% (Experiment 1) and 14% (Experiment 2) of the elections. Strategy profile (A, A, B) occurred in 8% (Experiment 1) and 7% (Experiment 2) of the elections. Committees systematically implemented A and holding (not necessarily exercising) tie-breaking power increased the frequency of the chairman's most-preferred outcome being implemented far beyond the level of chance. The distributions of strategy profiles are comparable in magnitude between experiments. Differences further diminish if we compare Experiment 2 to the random treatment in Experiment 1, see Tables A.1 and A.2.

Regarding limited strategic sophistication in the committee, Nash equilibrium analysis

Table A.2: 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	0.063	0.078	0.070	0.100	0.175	0.138
	C	0.000	0.000	0.000	0.013	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	0.013
	C	0.469	0.641	0.555	0.750	0.675	0.713
Player 3	*A*	0.063	0.047	0.055	0.100	0.063	0.081
	B	0.313	0.188	0.250	0.275	0.300	0.288
	C	0.625	0.766	0.695	0.625	0.638	0.631

explains roughly 45% of the observed strategy profiles, comprising all periods in each experiment. Specifically, it fails to account for the prominent profile (A, A, C). The level-k voting model captures the data very well. The three distinct strategy profiles predicted by the level-k model coincide with the three most-frequently observed strategy profiles in the experiments. In Experiment 1 these three profiles constitute 80% of our data, in Experiment 2 they constitute 71%. Individual voting behavior is compatible with elimination of WDS: chair participants violated WDS and voted for B or C in 7% (Experiment 1) and 16% (Experiment 2), player 2 participants voted for B in 1.6% and 1.3%, and player 3 voted for A in 5.5% and 8.1% of the elections, respectively. Player roles also differed in the extent to which they played according to IEWDS. Chair, player 2 and player 3 participants did so in 93% (84%), 56% (71.3%), and 70% (63%) of the elections in the two experiments, respectively.

Finally, we run a number of panel probit regressions with random effects and clustered standard errors on player 2's propensity to vote for A (the chair's most-preferred alternative). Table A.3 reports the results for Experiment 1 and establishes that the legitimacy of holding tie-breaking power had a significant effect on the propensity to side with the chair for player 2. The dummy variable for the performance treatment is positive and significant in all models which include additional controls. Specifically, we control for treatment specific time-effects through the interaction between the performance dummy and period. This interaction effect is significant and negative. Controlling for the number of correctly encoded words in the encoding task yields a significant and negative coefficient. Regarding the interpretation of this effect, assume that performance in the real-effort task correlates with cognitive ability. This would lead to the conclusion that the level of cognitive ability is negatively related to deviations from strategically sophisticated choices in the voting committee. However, if performance represents cognitive ability of players, one wonders why these participants competed for the chair role in the first place. Our conjecture is that performance is largely

Table A.3: Treatment effect on player 2's behavior over all periods in Experiment 1.

DV Vote For A	(1)	(2)
Performance	1.704*	1.879**
	(0.873)	(0.842)
Period	-0.143	-0.141
	(0.113)	(0.109)
Performance × period	-0.425**	-0.430**
	(0.200)	(0.202)
Words encoded		-0.061***
		(0.016)
WTP1		0.172
		(0.160)
Decisiveness belief		0.010
		(0.011)
Constant	-0.265	-0.359
	(0.634)	(1.368)
# of observations	128	128
Log pseudolikelihood	-72.04	-69.53
Wald χ^2	14.42	140.31
Degrees of freedom	3	6
Prob > χ^2	0.002	<0.001

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

driven by effort and not by innate cognitive ability, as intended by the real-effort task (see Erkal et al., 2011). A more direct explanation of the sign of the coefficient is that 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. Taken together, even though the absolute size of the effect of formal tie-breaking power decreases over time as shown by the interaction effect, the total effect of the performance treatment (coefficient + interaction effect) remains positive and significant ($p = 0.049$) when all periods are included.

Table A.4 reports the regression results for Experiment 2. Consistent with the analysis of the first period, a treatment effect and time-specific treatment effects are absent. The propensity to side with the chair is however declining as shown by the coefficient of the period variable. Regarding the exogeneous measure of strategic sophistication, being classified as level-1 in the p-beauty game (*L1-type* dummy) is 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 had a higher propensity to choose a voting strategy compatible with level-1 in the voting game. We also control for the ex-ante stated attractiveness for each of the three player roles, as well as for the stated decisiveness belief

Table A.4: Treatment effect on player 2's behavior over all periods in Experiment 2.

DV Vote For A	(1)	(2)
Neutral-label	-0.101	-0.101
	(0.904)	(0.740)
Period	-0.608**	-0.608**
	(0.271)	(0.278)
Neutral-label × period	0.276	0.260
	(0.326)	(0.335)
$L1$-type (in p-beauty)	0.707***	1.216***
	(0.269)	(0.432)
Decisiveness belief		-0.001
		(0.007)
WTP1 chair role		0.018
		(0.098)
WTP1 player-2 role		-0.254**
		(0.115)
WTP1 player-3 role		0.322***
		(0.088)
Constant	0.276	0.141
	(0.644)	(0.642)
# of observations	160	160
Log pseudolikelihood	-80.64	-74.75
Wald χ^2	16.16	100.04
Degrees of freedom	4	8
Prob > χ^2	0.003	<0.001

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

of the tie-breaking rule. The WTP1 coefficients for player 2 and player 3 are significant, the former negative, the latter positive. One interpretation is that those player-2 participants who evaluated their own role as more positive are participants capable of solving the game through IEWDS and hence foresee the negative consequences of holding formal tie-breaking power. For instance, participants who evaluate the player-3 role as relatively positive do also not seem to be aware of the solution of the voting game proposed by IEWDS. In conclusion, we do not observe a treatment effect if we incorporate data from all periods, as in the analysis of first-period behavior.

A.3 Attractiveness measures

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

Table A.5: 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 WSR tests are shown in parentheses.

more detailed analysis of these attractiveness measures as participants stated their beliefs about the attractiveness of each player role in the committee.

A.3.1 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.3.2 Experiment 2

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

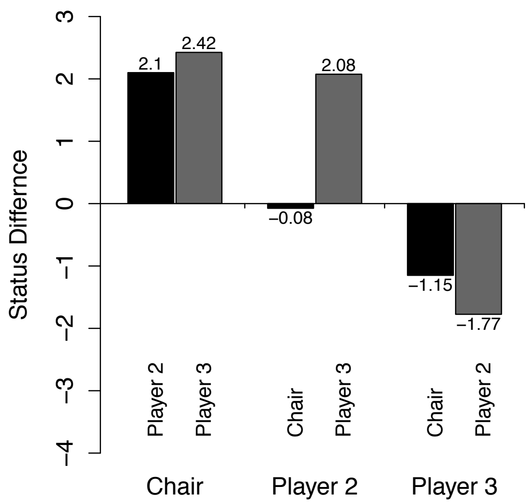


Figure A.1: Social status of own role relative to other roles.

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 3 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.5 reports the mean differences between post-voting and pre-voting WTP measures and 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, although we adjust p-values for 9 different hypotheses, our main observations remain highly significant. Regular members significantly down-shade the attractiveness of the chair role, 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 to herself a higher (lower) social status than to the other committee member j.

Figure A.1 shows the social status measures ($S_{i,j}$) and Table A.5 reports the corresponding WSR tests, 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 a lower status than both the chair ($S_{p3,ch} = -1.15$) and player 2 ($S_{p3,p2} = -1.775$). How can we interpret 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 the material well-being component, i.e. $S_{p2,chair} \approx 0$, $S_{p2,p3} > 0$ and $S_{p3,p2}, S_{p3,p2} < 0$. 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. 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 elections. An election takes place in a group of three voters. That is, you and two other participants build 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.

	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.

(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 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)

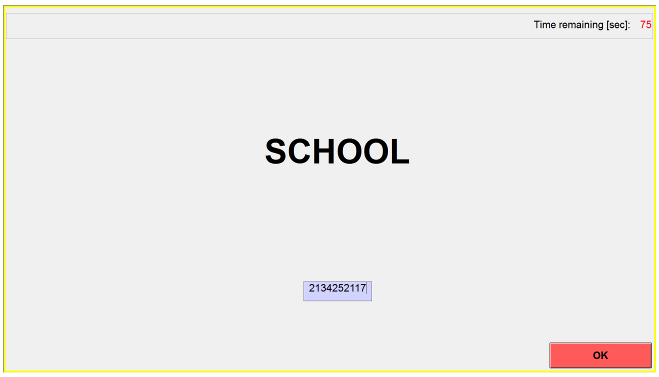
Explanation: How the chairman role is assigned

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)

C Sample screenshots

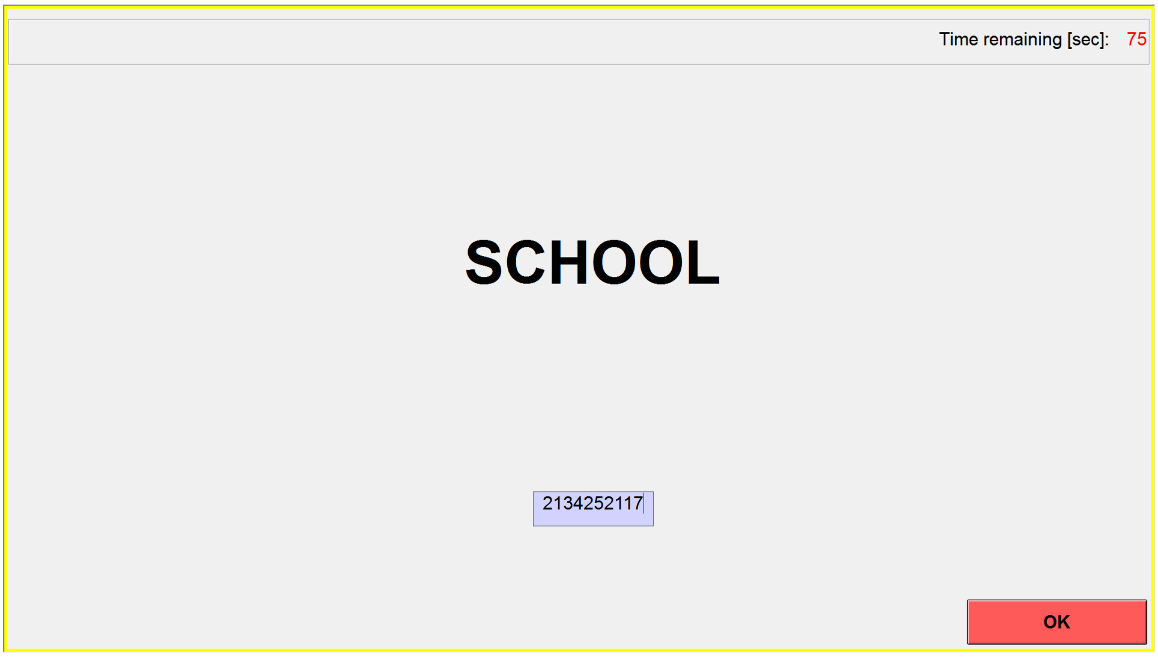


Figure C.1: Screenshot real-effort task (Experiment 1).

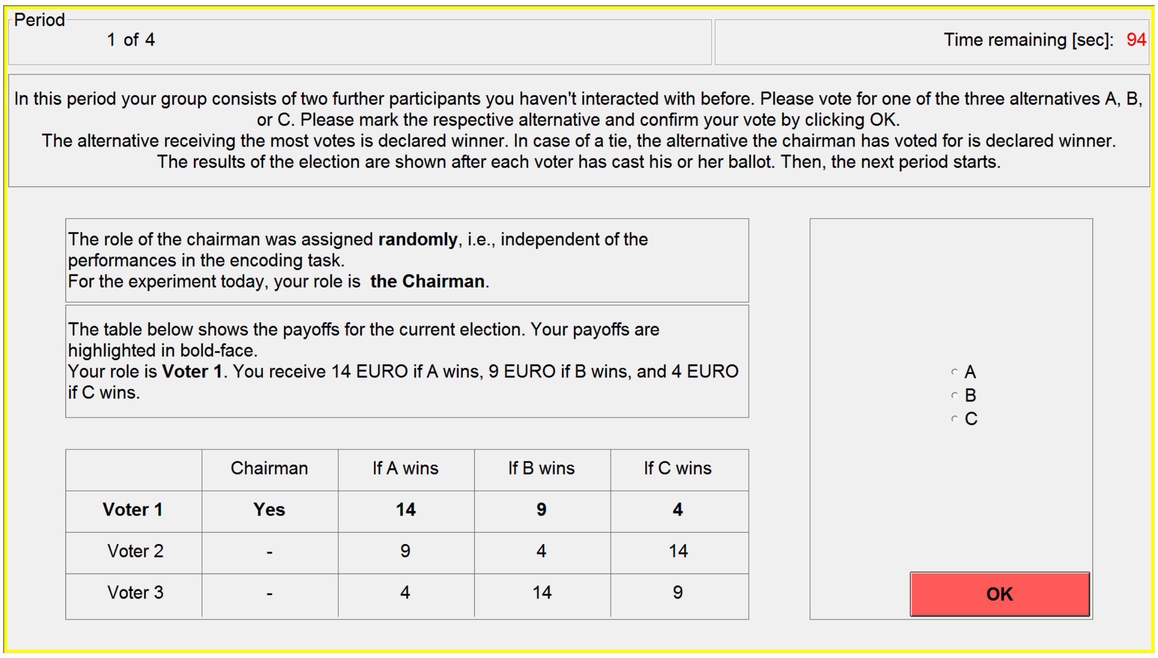


Figure C.2: Screenshot voting stage (Experiment 1).

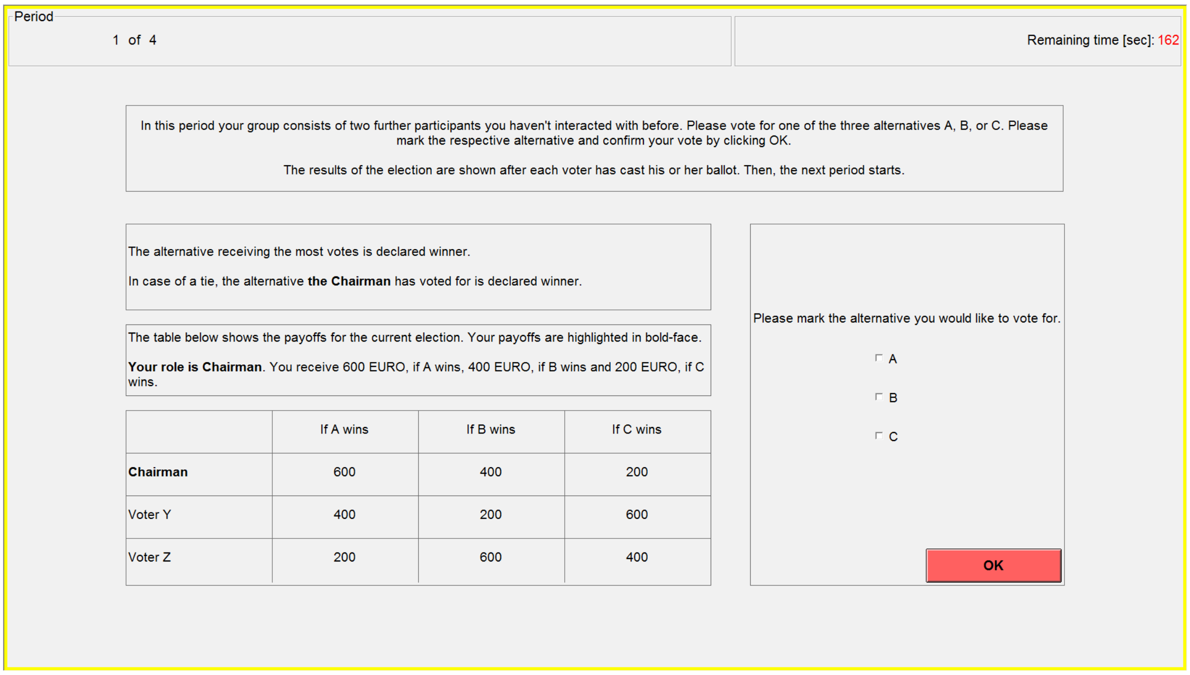


Figure C.3: Screenshot voting stage (chair-label treatment, Experiment 2).

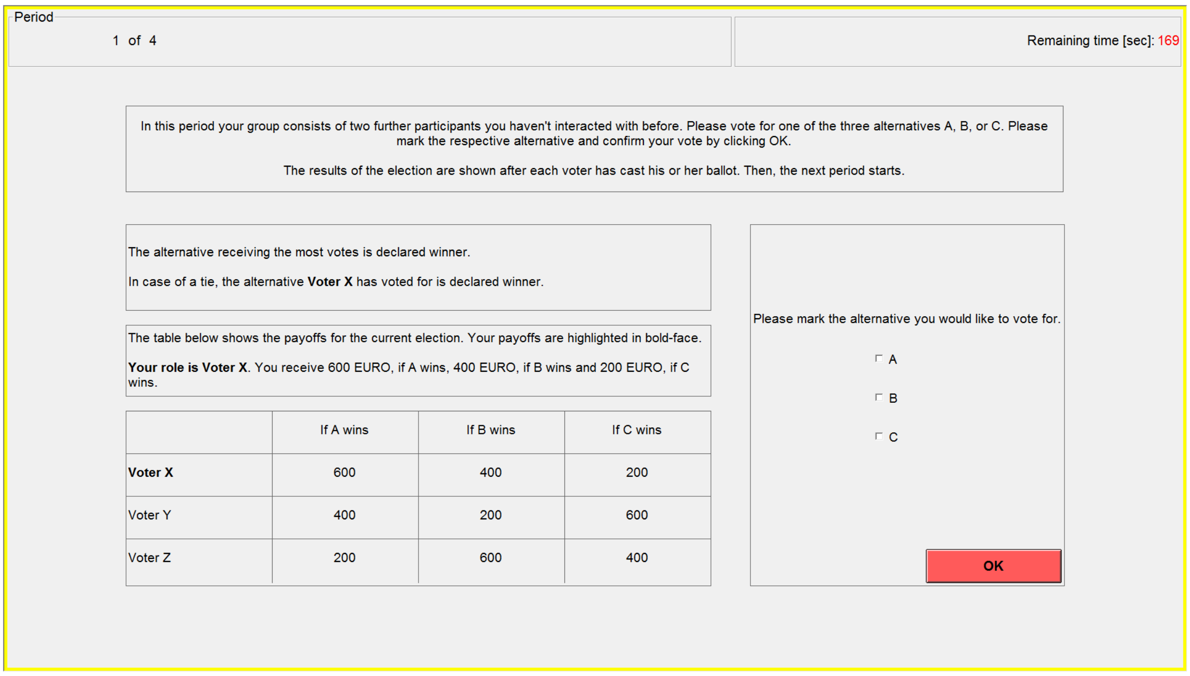


Figure C.4: Screenshot voting stage (neutral-label treatment, Experiment 2).