Where Power Resides in Committees*

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Abstract

The power to control decisions is rarely distributed equally in committees. We study in a voting committee how formal power (the decision right to break ties) translates into real power (effective control over outcomes). Two laboratory experiments provide causal evidence on how legitimacy and salience of formal power affects voting behavior and thus real power. Attitudinal measures regarding the perception of power exhibit a strong asymmetry between regular members and members who hold formal power; the latter seem to attribute a positive intrinsic value to holding the decision right to break ties.

Keywords: Leadership, power, committee, tie-breaking rule, experiment

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All animals are equal, but some animals are more equal than others.

George Orwell, *Animal Farm*

1 Introduction

Casual observation seems to suggest that many hierarchical structures consist of very few leaders, who exercise their decision power relentlessly, and of many low-ranking others, whose job it is to simple carry these decisions out. While this view has certainly bite for some forms of social interactions, it neglects many important guises in which power manifests itself in organizations (Simon, 1951; Arrow, 1974; Flynn et al., 2011; Sturm and Antonakis, 2015).[1] Quite often, those who hold the formal right to decide are *not* the ones who possess effective control over outcomes in organizations. The potential discrepancy between the right to control decisions (formal power) and effective control over outcomes (real power) in strategic situations, as well as covariates which mediate this trade-off, is the main subject of this article.

We consider a committee voting model in which decisions are taken in a group through voting. In this setup, we investigate the effects of asymmetric, formal tie-breaking power on behavior in the committee, as well as on the members' perception of power as an important antecedent of behavior, using controlled laboratory experiments. Members in the committee 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 also as the casting vote in case of a deadlock. This small asymmetry in the decision right to break ties is not only of theoretical interest but is prevalent in many committees, such as, in boards of directors in corporations, or in juries at court.

Our setup allows us to investigate the existence of real power of the chair and to isolate important factors which drive real power through different treatment interventions. Re-

[1] Note that we use the terms power, authority, or leadership interchangeably because the exact differences between these concepts do not matter for the research question we address in this article. Sturm and Antonakis (2015) provide a comprehensive and up-to-date overview of the different defintions and effects of power relevant to organizational behavior. For a historic perspective, see also de la Boétie (1975); Russel (1938); Weber (1978); Bartlett (1989).

sults of two independent experiments establish that the power to break ties of the chair is real, and its impact is larger than predicted by rational choice theory. Our treatment variations in Experiment 1 provide causal evidence that holding tie-breaking power legitimately increases the real power of the chair in the committee in the short run. The treatment effect supports previous findings which have shown that legitimacy is a vital element of how people react to formal power in other contexts (e.g. Kelman, 1958; Milgram, 1963; Suchman, 1995; Tyler, 2006; Silverman et al., 2014). Specifically, legitimate power of the chair sways regular members, against their monetary incentives, into choosing the chair's preferred alternative more often than if tie-breaking power is allocated randomly. The direction of the treatment effect is consistent with behavior of inhibition, i.e. compliant and conformist behavior of regular members (Keltner et al., 2003; Cialdini and Goldstein, 2004; Karakostas and Zizzo, 2016).

In Experiment 2 we study whether salience (labeling) of the chair role has a causal effect on the real power of the chair in the committee and on how tie-breaking power is perceived in the committee. Results show that salience of the chair role, on top of the asymmetric formal power, has neither an effect on voting behavior nor on the stated attractiveness of the chair role in the committee. Elicited attractiveness measures, however, reveal that the inclination to side with the chair is positively associated with the motive to adapt one's behavior to the member that holds the decision right to break ties. Moreover, it is interesting to observe how differently committee members seem to learn about the actual attractiveness of holding tie-breaking power in the committee. Whereas regular members rationally adjust their assessment of real power over time in accordance with their committee decision making experience, chairs only partially take this information in their assessment of their own role into account and also do not revise their view about regular members. In this sense, elcitied measures of attractiveness are consistent with an emerging body of work on the non-instrumental value of decision rights (Fehr et al., 2013; Bartling et al., 2014).

Our study is one of the first to provide insights into the internal workings of decision making in committees when small power asymmetries among members exist. We contribute to the existing literature in more than one way. Methodologically, we contribute to a rigorous causal analysis of power in committees and some of its important determinants,

using controlled laboratory experiments. Our stylized setup allows for a precise definition and identification of power. It thereby also circumvents possible experimenter demand effects related to power manipulations that employ priming interventions (cf. Zizzo, 2010).

We also add to the emerging literature on evidence-based assessments of power in committees , and of behavior in committees, more generally. While there is much anecdotal evidence regarding the power of chairs in committees, rigorous causal evidence has been scarce in the literature. One reason for this lies in the difficulty to establish reliable causal evidence of chair power using observational data from real-world committees because of issues of endogeneity, e.g. selection bias (Antonakis et al., 2010; Zehnder et al., 2017). A notable exception in this respect is the work by Berry and Fowler (2015, 2018), who provide strong empirical evidence on the influential position of chairs using observational data from congressional committees in the United States.[2] With regards to experimental evidence, it has been unclear how tie-breaking power influences committee decision making. The only work we are aware of is Blinder and Morgan (2005, 2008), who study how leadership in monetary policy boards affects decision making. They find no evidence for leadership effect in general, and no effect of tie-breaking power in particular.[3]

Finally, note that differences between formal power (authority, or leadership) and real power have been studied in the context of delegating decision rights in organizational economics both theoretically (Simon, 1951; Grossman and Hart, 1986; Hart and Moore, 1990; Aghion and Tirole, 1997) and experimentally (Fehr et al., 2013; Bartling et al., 2014). Aghion and Tirole (1997), for instance, show that the degree of asymmetric information between parties in a contracting environment determines whether the formal right to decide and the effective control over decisions coincide or not. Although this distinction plays also a key role for the effects of asymmetric tie-breaking power in committee voting, we will show below that the underlying reason for the difference between formal and real power is

[2] Berry and Fowler (2018) show empirically that the position of the chair is clearly desirable in legislative committees and provide an overview of the empirical literature.

[3] In their study, committees perform about the same with or without a leader. Note however that the preferences structure of common interest is fundamentally different from our setting, in which members have strictly conflicting preferences over the best alternative to implement. To ensure success in either of these two situations, leaders will require very different strategies. Kosfeld (2018) describes what kind of leader strategies behavioral economics has to offer in cooperative environments.

quite different in our committee setting.[4] In the remainder, we present the setup and and the results of the two independent experiments in detail and then discuss the implications of our findings for future research in the conclusion.

2 Experiment 1

In Experiment 1, we investigate the influence of asymmetric tie-breaking power on decision-making in committees. Before we describe the results in detail, we summarize the committee voting game participants engaged in, present its benchmark predictions, and the hypothesized treatment effect when the chair in the committee holds tie-breaking power legitimately.

2.1 Tie-breaking in the committee and main treatment

The following committee voting model serves in both experiments as the strategic environment in which committee members interacted with each other. The committee, originally due to Farquharson (1969), consists of three members which we refer to as the chair, player 2 and player 3. The committee decides to implement one of three available alternatives, say A, B, or C. Members' preferences over alternatives, summarized in Table 1, are publicly known and represent a situation of conflict. Each member favors a distinct alternative: $A \succ B \succ C$ 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. Members 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 asymmetry in tie-breaking power seems to give the chair and edge over the other two members, henceforth referred to a *regular members*. We show next why this is not necessarily the case and how strategic incentives can undermine the chair's greater power.

[4] Another fundamental difference between our setting and the assumptions in the delegation literature is that in the latter the owner of the property right typically holds the *de facto* right to transfer effective decision power to the agent, say the managing director, and needs to credibly delegate this power. In our 'democratic' voting committee, in contrast, the majority of board members can overrule the chair, which can render her ex-ante formal right to break ties completely ineffective.

As in Farquharson (1969), we assume that rational committee members use voting strategies that are weakly dominant, i.e. strategies that are never worse and sometimes better than other strategies. Consider the committee structure from the chair's perspective. For her, two different types of situations are relevant. First, the other two members agree and vote for the same alternative. Then the committee decisions is fixed and the chair's vote has no influence on the final outcome. Second, the other two members disagree in their votes and vote for distinct alternatives. Then the chair's vote is decisive and whatever she votes for is implemented (either by creating a three way tie among the alternatives, or a 2 to 1 victory for the alternative the chair voted for). Since she strictly prefers alternative A over both B and C, to vote for A weakly dominates voting for B or C. A chair should therefore always vote for A. This process of reasoning is not alien to regular members, they can foresee the chair's strategic consideration and expect her to vote for A. Given that the chair votes for A, player 2 has only one weakly dominant strategy: to vote for C. This leaves player 3 with the expectation that the chair will vote for A and player 2 will vote for C. In that case, voting for C is the only weakly dominant strategy. Hence, the committee implements the chair's least-preferred alternative C for sure and the formal decision-right to break ties transform into a burden to bear for the chair.[5]

The stylized committee voting model is particularly well suited to investigate our main research questions as theory predicts a large discrepancy between the formal power of the decision-right to break ties and the chair's real power (effective control over outcomes). Comparing committee behavior in the experiment against the game-theoretical benchmark predictions, we first establish that the chair's power is real. We then provide causal evidence on the underlying determinants of the chair's real power through treatment variations in the allocation of the formal tie-breaking decision right. To do so, 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

[5] Note that there are in total 5 strategy profiles which constitute pure-strategy Nash equilibria in the one-shot game; these are (A, A, A), (B, B, B), (C, C, C), (A, A, B), and (A, C, C). The three unanimous NE are ruled out by assuming that players do not play weakly dominated strategies, because the strategy to vote for her worst alternative is weakly dominated for each player. Applying the concept of iterated elimination of weakly dominated strategies (IEWDS), which is commonly assumed in the voting literature, it is immediate to see that the only profile surviving IEWDS in the game is (A, C, C).

Table 1: Preference profile of committee members.

Chair	$A \succ B \succ C$
Player 2	$C \succ A \succ B$
Player 3	$B \succ C \succ A$

position thus comes with a natural notion of legitimacy. 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 than in the random treatment. As the performance-based allocation takes places only once, and before the start of the first voting game, we do not expect that the impact of the performance-based allocation is equally strong over all periods; all members are informed about the preference structure in the committee and receive feedback about the election outcome together creating learning opportunities through experience.

2.2 Design

The experiment consisted of two parts. Participants engaged in a real-effort task and then made decisions in committees by voting. Upon arrival, they were randomly allocated to isolated working stations. 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 commit-

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*

tee's chair without introducing carry-over 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 or chance. 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 itself 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 decision right.

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 each 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). Hence, each session had two matching groups present in which interaction through committee voting took place (i.e. no interaction across matching groups). 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. Al-

though 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 2.1 for 4 periods under perfect-stranger matching, i.e. the same participants interacted with each other only once. 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 was used for actual payment. All these measures were taken to confounding carry-over effects from one period to another (reciprocity, colaition fomration, etc)

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.

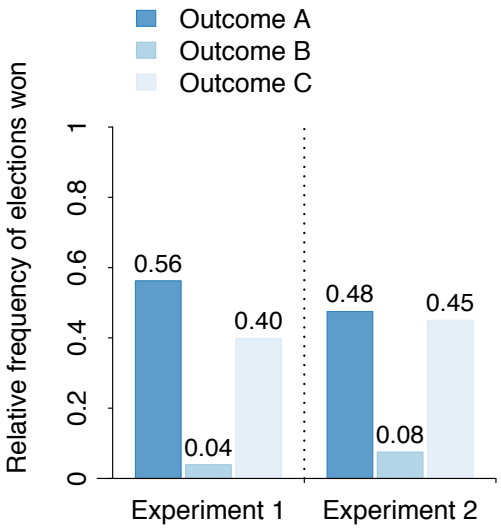
2.3 Results

We start presenting outcomes and behavior in the committee when one member, the chair, holds the decision right to break ties. Results shed light on whether the chair's formal power is advantageous for the chair or whether formal power does not bear favorable consequences for the chair, as predicted theoretically (see Section 2.1). Recall that alternative A, B and C represent the most preferred alternative for the chair, player 3, and player 2, respectively, and that we measure the real power of a player by the relative frequency with which committees implement that players most-preferred outcome.

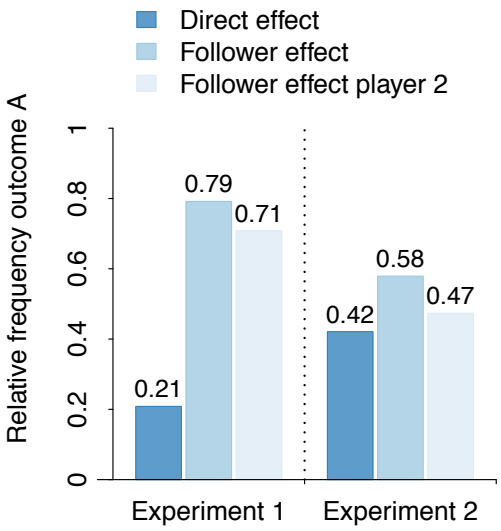
The left-hand part in Figure 1(a) shows the relative frequency of committee decisions won by each of the three alternatives (pooled over treatments). Alternative A was the most frequently implemented outcome and in grand total won 56% of all elections whereas C won 40%. Two two-tailed, exact Wilcoxon-signed-rank (WSR) tests run at the level of independent matching groups corroborate statistically that the chair was more powerful than regular members (all periods: $p < 0.008$, last period: $p < 0.016$, $N = 8$ in each test).[6] The formal tie-breaking decision right transformed into real power for the chair and she was at least as powerful as regular members.

Next, we isolate important factors that underlie the real power of the chair. To do so, we decompose the chair's real power into *direct effects*, the chair exercises her tie-breaking decision right in favor of A, and into *follower effects*, A wins the election and at least one regular member votes for A. From the left-hand part in Figure 1(b) it is evident that follower effects were empirically more prominent than direct effects in Experiment 1. Only 21% of A-outcomes occurred as a consequence of exercising tie-breaking power, whereas 79% of A-outcomes were due to follower effects. A total of 71% of all A-outcomes entailed player 2 voting for A; of these, all outcomes were associated associated with voting profiles that saw the chair vote for alternative A and player 3 vote for either C (57%) or B (14%). Two two-sided exact WSR tests run at the level of independent matching groups corroborate that follower effects occurred more frequently than direct effects (all periods:

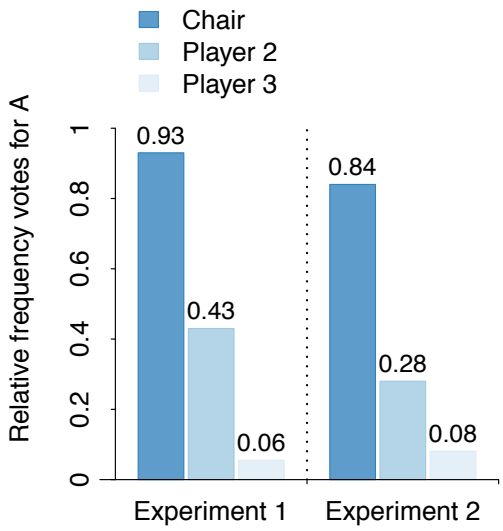
[6] Observe that regular members in the experiment enter a 50/50 lottery for the committee roles of player 2 and player 3. The correct ex-ante counterfactual from the chair's perspective is to compare her power to the average power of player 2 and player 3 combined. Given that our measure of power is represented by relative frequencies (adding up to one), we immediately obtain an effective power threshold of 1/3 for the chair which was used to obtain our null-hypotheses.



(a) Committee outcomes



(b) Determinants of real power



(c) Individual voting behavior

Figure 1: Committee outcomes, determinants of real power, and individual voting behavior over all periods.

$p < 0.023$, last period: $p < 0.054$, $N = 8$ in each test).

Assessing behavior at the individual level, the left-hand part in Figure 1(c) presents the individual vote frequencies in favor of alternative A. We observe stark differences across member roles, which reflect closely the material incentives induced by the committee's preference structure over alternatives. The chair displayed a strong motive to support her favorite option and voted for A in 93% of all elections. On the contrary, player 3 supported A, her least-preferred option, in only 6% of the elections. Committee decisions in favor of A impose the highest costs in terms of foregone payoffs for this committee member. Player 2 showed an intermediate level to vote for A which captures her trade-off between the, possibly psychological, motives to follow the chair and the pursuit of her own material interests.[7]

Taken together, our results at the individual-level behavior demonstrate that follower effects associated with player 2's behavior are the root of the chair's real power. We thus examine player 2 behavior in light of potential treatment differences to further elucidate the driving forces of the follower effect. Recall, that our hypothesis was that performance-based allocation of the tie-breaking decision right would legitimize power, which in turn would sway player 2 to vote more often for A in the performance treatment than in the random treatment. Note that the same argument has little bite for player 3 as alternative A is her least-preferred alternative. Indeed, player 3 rarely voted for A in any of the treatments.

To investigate our main treatment hypothesis, which captures the causal effect of holding tie-breaking power legitimately on voting behavior of regular members, we ran probit regressions on player 2's propensity to vote for A. The dependent variable was the same across models and takes the value of 1 if player 2 voted for A, and 0 otherwise. Both models in Table 3 confirm that 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 itself is insignificant, the

[7] Table A.1 in the appendix shows the evolution of the main dependent variables and demonstrates the robustness of our findings across periods. Table A.2 and Table A.3 present a more detailed summary of committee outcomes and individual vote frequencies.

Table 3: Probit regressions on player 2's behavior, Experiment 1.

DV: vote for A	(1)	(2)
Performance	1.157**	1.369**
	(0.521)	(0.546)
Period	-0.083	-0.090
	(0.067)	(0.069)
Performance × Period	-0.317**	-0.340**
	(0.152)	(0.162)
Words encoded		-0.046***
		(0.009)
Decisiveness belief		0.008
		(0.007)
WTP1 chair		0.147
		(0.108)
Constant	-0.154	-0.419
	(0.369)	(0.949)
Average marginal effect of performance		
at Period 1	0.320**	0.361**
	(0.161)	(0.147)
at Period 2	0.206	0.250*
	(0.153)	(0.141)
at Period 3	0.078	0.123
	(0.157)	(0.147)
at Period 4	-0.039	0.003
	(0.165)	(0.155)
Number of observations	128	128
Number of participants	32	32
Number of clusters	8	8

Notes: Probit regressions with standard errors clustered at the matching-group level in parentheses. Dependent variable is dummy, 1 if player 2 voted for A. We report average marginal effects of performance dummy at different periods separately. Significance codes: *** at 1% level, ** at 5% level, * at 10% level.

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. We have therefore computed average marginal effects for the Performance coefficient for each of the four periods in the experiment separately. In model (2), the treatment effect was positive and significant in the first two periods, but insignificant for later ones. Player 2's probability to vote for A was 36 and 25 percentage points higher in the performance treatment than in the random treatment in the first two periods of the experiment, respectively. In the last period we observed no difference across treatments. These results point towards a short-lived effect of legitimate power on the follower effect within the experiment. The treatment effect was washed away by feedback and learning opportunities in this complete-information setting.

Finally, controlling for the number of correctly encoded words in the real-effort task yielded 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. None of the other variables did have a significant impact on player 2's propensity to vote for A.

2.4 Discussion

Experiment 1 demonstrates that the formal decision-right to break ties confers real influence over committee decisions for the chair, partly through exercising the decision right to break ties in case of a deadlock, but mainly through follower effects. The treatment comparison provided causal evidence that holding tie-breaking power legitimately swayed player 2, against strong monetary incentives, to side with the chair. Observed committee behavior and particular the treatment difference in behavior is consistent with the interpretation that the formal decision right to break ties of the chair induced, through perceived authority or higher status of the chair, a tendency for compliant or conformist behavior in regular members (e.g. Kelman, 1958; Milgram, 1963; Tyler, 2006; de Kwaadsteniet and van Dijk, 2010). Our interpretation of the underlying motives of regular members is also

in line with well-established findings in social psychology that a lack of power leads to emotional states and behavior of inhibition (Keltner et al., 2003).

Pre-voting elicited perception of the attractiveness of the chair's role lends further credence to our interpretation. The majority of participants perceived the chair position as overwhelmingly positive as evidenced by the high level of effort exerted in the real-effort task (correctly encoded words, mean $M = 36.3$, $SD = 7.3$) and the high ex-ante stated willingness to become chair (WTP1: $M = 8.2$, $SD = 1.7$, on 10-point Likert scale). Participants also overestimated the *direct effect* of tie-breaking power: they expected 55% of all elections to result in a three-way tie, whereas a tie occurred in only 14%.

Our results bear important implications for committee decision in the presence of a well-defined asymmetry in formal power among committee members. They suggest that the perception of power can lead to strong follower effects even if formal power, the decision right to break ties, does not necessarily lead to influence over outcomes (real power). To gain further insights into the underlying reasons for the observed follower effects in Experiment 1, we conducted a second experiment to assess rigorously the perception related to the decision power of the chair before and after the committee elections.

3 Experiment 2

Our motivation for the follow-up Experiment 2 is two-fold. First, we investigate the general robustness of our Experiment 1. Second, we provide rigorous insights on the perception of formal and real power as well as the underlying motives of follower effects.

Throughout Experiment 1, we followed the convention to refer to the member holding the tie-breaking decision right as the 'chair' of the committee. The connotations of this particular labeling might have increased the salience of the role and thus contributed possibly to the *level*-effect of the chair's power. The tournament character in the encoding task in Experiment 1 might have also added to this effect. In Experiment 2, we therefore assigned player roles randomly and introduced a control treatment with neutral chair labels (same as a regular member). We have also expanded upon on our pre-voting and post-voting questionnaire used in Experiment 1, which we detail below in the experimental design. Except for these changes, the design was identical to the one of Experiment 1.

3.1 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 decision right. Table 2 summarizes treatment differences between experiments. The design allowed us to assess whether or not role labels, independent of the formal decision right, influenced behavior of committee members. We employed the same voting game as in Experiment 1. Moreover, participants stated the attractiveness of each committee member role before (WTP1) and after (WTP2) the committee voting stage on 10-point Likert scales. The post-voting questionnaire also included questions on social status of committee members (taken from de Kwaadsteniet and van Dijk, 2010). In particular, each participant answered two questions about *each* of the other two committee members' status on 7-point Likert scales: 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 member i relative to member j was taken as 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. Participants also answered two questions about about their adaption motives with regard to the behavior of each of the other two members via 7-point Likert scales: a) "To which extent did you feel that you had to adapt your decisions to the decisions of voter j?" and b) "To which extent did you feel that voter j had to adapt his or her decisions to your decisions?" The measure of adaption motive $M_{i,j}$ of member i with regard to member j was taken as the rating difference between the two questions. If $M_{i,j} > 0$ ($M_{i,j} < 0$), member i expressed a motive to adapt her behavior to the behavior of member j (an expectation that member j had to adapt her decision to member i's decision).

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.

3.2 Results

The right-hand side in Figure 1(a) to Figure 1(c) presents the outcome distribution, the decomposition of the chair's power into direct effects and follower effects, and the relative frequencies of votes cast in favor of A across member types for Experiment 2. Observe however that, as in Experiment 1, the chair's power was real and to a large extent driven by follower effects associated with player 2 voting for A. We ran the same set of hypothesis tests in Experiment 2 as we did for Experiment 1. Two-sided WSR tests corroborate that the chair was more powerful than regular members (all periods: $p < 0.004$, last period: $p < 0.086$, $N = 10$ in each test) and that follower effects were as prominent as direct effects (all periods: $p > 0.388$, last period: $p > 0.718$, $N = 10$ in each test).

We ran a series of probit regressions to investigate the factors influencing player 2's decision to follow the chair and vote for A. In all models presented, the sample is restricted to player 2 participants and the dependent variable takes the value 1 if a player 2 voted for A. Table 4 presents the corresponding results. We start with the analysis of whether or not the chair label itself contributed to the follower effect. If the saliency of the chair label influenced behavior, player 2 would have been expected to vote for the chair's preferred alternative A more often in the chair-label than the neutral-label treatment. We found no support for this hypothesis as there was no significant effect of the *Neutral Label* dummy in any of the models. In grand total, player 2 chose A with a frequency of 22% in the *chair-label* treatment and with 33% in the *neutral-label* treatment.[8] As in Experiment 1, the propensity to side with the chair was, however, declining over time as evidenced by the negative and significant coefficient of the *Period* variable. Learning opportunities created by our repeated voting design diminished player 2's propensity to vote for A over

[8] 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 only reasonable way to influence the outcome of the committee 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% in the neutral-label treatment.

Table 4: Probit regressions on player 2's behavior, Experiment 2.

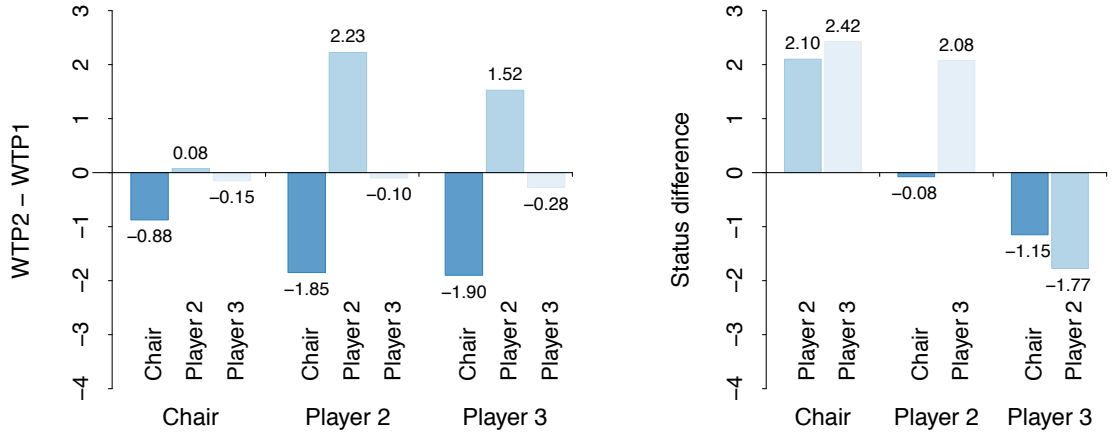
DV: vote for A	(1)	(2)	(3)
Neutral Label	-0.188	-0.216	-0.140
	(0.691)	(0.739)	(0.695)
Period	-0.486**	-0.571**	-0.569**
	(0.224)	(0.253)	(0.243)
Neutral Label × Period	0.234	0.242	0.245
	(0.264)	(0.303)	(0.290)
Adaption motive to chair		0.205***	
		(0.073)	
Status chair			0.173***
			(0.040)
Decisiveness belief		-0.009	-0.004
		(0.006)	(0.005)
WTP1 chair		-0.019	-0.031
		(0.062)	(0.067)
WTP1 player 2		-0.172**	-0.175**
		(0.071)	(0.070)
WTP1 player 3		0.232***	0.164***
		(0.056)	(0.045)
Constant	0.347	0.920**	0.977**
	(0.481)	(0.382)	(0.476)
Average marginal effects			
Adaption motive to chair		0.051***	
		(0.013)	
Status chair			0.042***
			(0.008)
Number of observations	160	160	160
Number of participants	40	40	40
Number of clusters	10	10	10

Notes: Probit regressions with standard errors clustered at the matching-group level in parentheses. Dependent variable is dummy, 1 if player 2 voted for A. We report average marginal effects of adaption motive to chair and status chair separately. Significance codes: *** at 1% level, ** at 5% level, * at 10% level.

time. We again found no impact of chair-label on this learning effect, the interaction term between the *Neutral Label* dummy and *Period* variable was insignificant. These observations establish that our results are driven by how regular members perceive formal tie-breaking power of the chair. Our treatment comparisons provide no evidence for the salience of the 'chair' position (labeling effect) to influence behavior of regular members. We omit a more thorough discussion of the above presented behavioral results in favor of a focused analysis of the follower effect and its underlying psychological mechanism.

Next, we investigate behavioral correlates shedding light on the motives that underlie follower effects. Each participant answered questions about the extent to which they felt the need to adapt their decisions to other members and about the perceived status of other members post-voting. We included both measures relating to the chair role into our analysis, *Adaption motive* towards chair and *Status chair*, with higher values indicating a higher need to adapt behavior and a higher status of the chair, respectively. Both measures were highly correlated with player 2's propensity to vote for A, the corresponding coefficients being significant and positive. To quantify this relationship, we estimated the average marginal effects of the variables on the probability to vote for A. A one-point increase in the adaption motive was associated with a 5.1 percentage points higher probability to vote for A. Similarly, a one-point increase in perceived status of the chair was associated with a 4.2 percentage points increase in the probability to vote for A. These results confirm our conjecture from Experiment 1 (see Section 2.4) that the formal decision-right to break ties influenced follower behavior through the channel of perceived status and an adaption motive to follow the one holding formal power.

We included further variables in the regression analysis to control 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 the player-2 and player-3 roles 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 who followed the reasoning process outlined in Section 2.1 and expected committees to implement C. Participants who evaluated the player 3 role as relatively positive did not seem to be aware of this effect. The *Decisiveness belief*, expressing the participant's belief that the tie-breaking rule (the direct effect) will decide the outcome, was found insignificant.



(a) Stated attractiveness difference ($WTP2 - WTP1$)

(b) Stated status difference (ex-post)

Figure 2: Change in elicited role attractiveness and relative role status.

Finally, we provide an analysis of how different (outcome) experiences in committee decision making changed participants perception regarding the attractiveness of member roles in the committee. The following observation serves as our point of vantage: there is a discrepancy in chair and player-2 role rankings derived from stated attractiveness WTP1 (Chair > Player 2) and implemented outcome experiences (Chair = Player 2). Pre-voting, participants ascribed the highest attractiveness to the chair role (mean WTP1=8.85), followed by the role of player 2 (WTP1=5.21), and player 3 (WTP1=4.26). At the same time, committees implemented the chair's favorite alternative A with the approximately same frequency as they implemented player 2's favorite alternative C, see Figure 1(a). If the post-voting attractiveness measures WTP2 were guided by committee voting experience, we should expect a post-voting decrease in the relative role attractiveness WTP2 between the chair role and player-2 role.

Figure 2(a) plots the differences between post-voting and pre-voting attractiveness assessed by all participants ($WTP2 - WTP1$), for each member role. The horizontally aligned labels identify participants, the vertically aligned labels identify the role that was to be assessed. For example, the first bar corresponds to chair participants' assessment of

the chair role, the fourth bar to player 2 participants' assessment of the chair role, and so forth. Figure 2(a) reveals a heterogeneous pattern in attractiveness adjustments over time and a pronounced difference in the role attractiveness elicited from regular members and the chair. Whereas regular member participants adjust the attractiveness of each member role according to committee voting experience (chair role ↓, player 2 role ↑), the chair participants are reluctant to revise their own-role attractiveness and do not adjust for the relative attractiveness of the player 2 role post-voting. The failure to recognize the pivotal role of player 2 establishes that the heterogeneous pattern of attractiveness adjustments is not driven by potential own-role biases.

To investigate this pattern more thoroughly, we run a series of OLS regressions on our attractiveness measures. To economize exposition we have condensed our attractiveness measures into one dependent variable representing the post-voting attractiveness difference between the chair and player 2 role net of their pre-voting differences:[9]

$$(WTP2_{Chair} - WTP2_{Player2}) - (WTP1_{Chair} - WTP1_{Player2}). \tag{1}$$

Model (1) in Table 5 presents the corresponding regression results. As can be seen, the dummies for the *Player 2* role and *Player 3* role are negative and significant. Importantly, a post-estimation hypothesis test does not detect any difference in attractiveness adjustments between regular members. This confirms statistically our previous observation of heterogeneous patterns in attractiveness adjustment between chair and regular member participants. Net of pre-voting differences, regular members in comparison to the chair assessed the chair role as less attractive than the player 2 role post-voting. We have also included two variables to the analysis which capture important experiences about the real power of the chair. Those events are *See* (A, C, C) and *See tie-breaking power*. The former counts (from 0 to 4) how often a participant saw regular members simultaneously vote for C fixing the chair's vote at A, i.e, regular members tacitly coordinating against the chair. The latter counts (again from 0 to 4) how often a participant saw the chair exercising her tie-breaking power. As one would expect, seeing the detrimental consequences of the

[9] Notice that the expression can be rewritten as $(WTP2_{Chair} - WTP1_{Chair}) - (WTP2_{Player2} - WTP1_{Player2})$ which captures a difference in difference measure of the attractiveness assessments between player roles and over time. The two terms in brackets correspond to the chair and player 2 role attractiveness changes presented in Figure 2(a).

Table 5: OLS regressions on WTP2, WTP differences, and perceived status, Experiment 2.

DV:	WTP difference (chair vs player 2)			Status difference (chair vs player 2)		
	(1)	(2)	(3)	(4)	(5)	(6)
Player 2	-3.125***			-2.025***		
	(1.169)			(0.724)		
Player 3	-2.475**			-2.725***		
	(1.081)			(0.769)		
Neutral Label	-1.175	-1.276	-1.126	0.688	1.171	0.459
	(0.955)	(1.592)	(1.208)	(0.636)	(0.922)	(0.838)
See (A, C, C)	-1.850***	-1.944	-1.816***	-1.747***	-1.021	-2.005***
	(0.583)	(1.253)	(0.669)	(0.343)	(0.635)	(0.403)
See tie-breaking power	0.979*	0.843	1.040*	-0.168	-0.241	-0.099
	(0.520)	(1.098)	(0.597)	(0.410)	(0.679)	(0.513)
Constant	1.437	1.755	-1.495	4.493***	3.249**	2.546**
	(1.428)	(2.855)	(1.486)	(0.893)	(1.538)	(1.059)
Number of observations	120	40	80	120	40	80
Robust std. err.	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions with robust standard errors in parentheses. Dependent variable in Model (1) to (3): WTP2(Chair) − WTP1(Chair) − (WTP2(Player 2) − WTP1(Player 2)); Model (4) to (6): Status difference Chair - Player 2. Model (2) and (5) restrict sample to chair participants only, Model (3) and (6) restrict sample to regular-member participants only. Significance codes: *** at 1% level, ** at 5% level, * at 10% level.

formal decision-right was associated with a significant decrease in the relative attractiveness of the chair role in comparison to the player 2 role, whereas seeing the chair wielding her tie-breaking power was associated with an increase in the same. Model (2) and (3) separate the sample into chair and regular members, respectively. They show that that reactions to actual payoff experiences (outcomes) were only significant for regular members, but not for chair participants. Holders of the formal decision right did not show a reaction to experience-relevant variables in their attractiveness assessment. Similar results are derived for our post-voting elicited measure of social status, see Figure 2(b) and Table 5, Models (4) to (6). Finally, the treatment dummy for the Neutral Label treatment did not have any significant effect in any model specification, i.e. did not influence the individual attractiveness statements.

The heterogeneous integration of voting experiences in the committee into post-voting measures of attractiveness and social status is consistent with an increasing literature 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, for instance, motivated to retain control over own payoffs (Owens et al., 2014) and cherish non-interference by others (Neri and Rommeswinkel, 2016).

4 Conclusion

We derived a concise and empirically measurable definition of formal and real power within a stylized committee voting model with asymmetric tie-breaking power. Within this framework, we investigated the determinants of effective influence over outcomes. More broadly, the formal tie-breaking power in the committee setting is a crisp way to formalize small asymmetries in authority, leadership, or bargaining power which exist in many real-world committees but are often difficult to identify for external observers.

Results from two independent laboratory experiments revealed that small asymmetries in voting power strongly influenced decision making in ad hoc committees, which stands in contrast to predicted behavior based on rational choice theory. The chair's advantage is explained to a large part by follower effects and only partially by direct effects of exercising ties in case of a deadlock. We also provided causal evidence that holding tie-breaking

power legitimately leads to an increase in real power of the chair through follower effects in the short run. To preserve this for the chair advantageous effect in the long run, a simple but effective way might be to remind regular members about the legitimacy of the leader if the goal is to increase compliance with the chair in the committee. Regarding the perception of tie-breaking power before members have gained experience in the voting environment, behavior and elicited attractiveness measures support the view that real power resides where committee members believe it resides (de la Boétie, 1975). With experience, however, the size of the treatment effect decreases because regular committee members have a strong monetary incentive to vote against the chair's preferred alternative and partially learn to do so successfully.

Our study provides a first step towards an evidence-based understanding of committee decision making in the presence of small voting asymmetries. Apart from issues regarding the information structure in the committee, other exciting issues, such as, the size of the committee, the distribution of preferences, or communication channels between members in the committee are all potentially important determinants of real power. Their impact on committee behavior can assessed systematically in future research, using controlled laboratory experiments or formal models of strategic interaction.

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Appendix

for

"Where Power Resides in Committees"

A Additional tables

Table A.1: Main dependent variables by period.

	Experiment 1				*Experiment 2*			
	P1	P2	P3	P4	P1	P2	P3	P4
Outcome A	0.72	0.50	0.47	0.56	0.58	0.47	0.40	0.45
Follower effects								
Total	0.82	0.75	0.80	0.77	0.78	0.47	0.50	0.50
Due to (A, A, C)	0.60	0.50	0.67	0.55	0.43	0.28	0.33	0.18
Due to (A, A, B)	0.22	0.18	0.06	0.05	0.25	0.06	0.00	0.11
Individual votes for A								
Chair	0.94	0.97	0.84	0.97	0.90	0.95	0.80	0.90
Player 2	0.53	0.38	0.41	0.31	0.50	0.25	0.20	0.15
Player 3	0.00	0.03	0.06	0.13	0.05	0.10	0.08	0.10

Notes: $P1$ to $P4$ represent the four periods in the experiments. Outcome A: percentage of elections won by A. Follower effects: percentage of outcome A due to follower effects. (A, A, C): chair voting for A, player 2 for A, player 3 for C. (A, A, B): chair voting for A, player 2 for A, player 3 for B. Individual votes for A: percentage of voting decisions in favor of A.

Table A.2: 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

Table A.3: 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