Informed Empowerment in Committee Voting*

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Abstract

We present a theory of informed empowerment and investigate two mechanisms by which informed empowerment enhances committee performance: (1) when informed members form a majority and/or (2) when informed individuals hold formal authority, such as veto power. By applying this theory to examine the interaction between gender and accountable governance in a neutral context, we investigate *when* gender differences may lead to varying information levels and *how* women contribute to not only women's issues but also to general committee processes. Our experiments reveal that women-majority committees outperform women-minority committees in information aggregation due to women's higher likelihood of being informed, which facilitates the women-majority committees making informed decisions. Furthermore, even with only one woman on the committee, those where women have veto power outperform committees in which men serve this role, driven by women's more effective utilization of veto power, greater willingness to contribute, and higher risk aversion.

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Committee decision-making is ubiquitous in legislative bodies, courts of law, the Federal Reserve, and corporate boards (Fiorina and Plott 1978; Feddersen and Pesendorfer 1997). In executive decision-making bodies, women are often underrepresented (O'Brien 2015; Preece 2016; Karpowitz, Monson and Preece 2017). Some work demonstrates that the smaller the proportion of women to men in a group, the bigger the gender gap is in influence and participation in decision-making (Karpowitz, Mendelberg and Shaker 2012). Other work finds a backlash effect, where men tend to be more vocal and verbally aggressive when the committee has a higher proportion of women (Kathlene 1994). Still others suggest that women can wield as much, or more, influence than men even as the minority (Karpowitz et al. 2024; Anzia and Berry 2011; Thomas 1995), partially due to selection effects (Chaudoin, Hummel and Park 2024; Kanthak and Woon 2015).

The framework of most existing theories studying gender composition relies on two key conditions: (1) women and men prefer different policies or have distinct end goals; and (2) women are more effective policy-makers than men on issues specifically affecting women. However, these conditions may not always be applicable. In committees such as juries, environmental initiatives, and crisis response panels, even if men and women may have different perspectives and priorities, they often share aligned goals (e.g., convicting the guilty and acquitting the innocent) and common preferences (such as a more just, safe, and prosperous society). Moreover, not all decision tasks are strictly gendered, and in those gender-neutral contexts (e.g., setting speed limits or planning school funding), neither men nor women are inherently better decision-makers. Thus, existing explanations of the impacts of women's presence, while compelling and insightful, are incomplete.

This study explores how gender differences in committee behavior effect an essential, yet understudied, function of the committee system—information aggregation. Because "decisions are necessarily a function of information" (Arrow 1974, 49), committee decision-making surpasses individual decision-making when the committee system functions to aggregate information across individuals (Gilligan and Krehbiel 1987; Lupia 1992; Austen-Smith and Banks 1996; Feddersen and Pesendorfer 1997; Battaglini, Morton and

Palfrey 2010).[1] When discussing the impact of women's representation and authority on committees, we should therefore investigate how information empowers individuals to be better representatives, how information boosts the competence of those who hold positions of formal authority, and how these affect the outcomes of committee voting.

We present a theory of *informed empowerment* that highlights information in committee voting. Informed members increase the committee's competence, and when they leverage their expertise, the committee makes informed decisions. *All else equal*, committee performance improves when (1) informed members form a majority and/or (2) informed individuals have formal authority such as veto power. Since information is not always freely available, whether an individual is informed depends on both one's effort to acquire information and how hard the problem is to solve. We expect that women will exert more effort than men to gather information, and thus they are more likely to be informed in a neutral and general context in which committee members' end goals are aligned. Information empowers, leading to women's presence amplifying competence-driven decision making.[2] In scenarios in which everyone is informed or uninformed, that is when the decision task is incredibly easy or prohibitively hard, gender differences in information acquisition may yield no effect—because factors outside individual behavior drive the level of expertise. But when the level of expertise is attainable through effort, gender differences lead to women being more informed than men, and then women's presence and/or their holding formal power helps the committee make informed decisions.

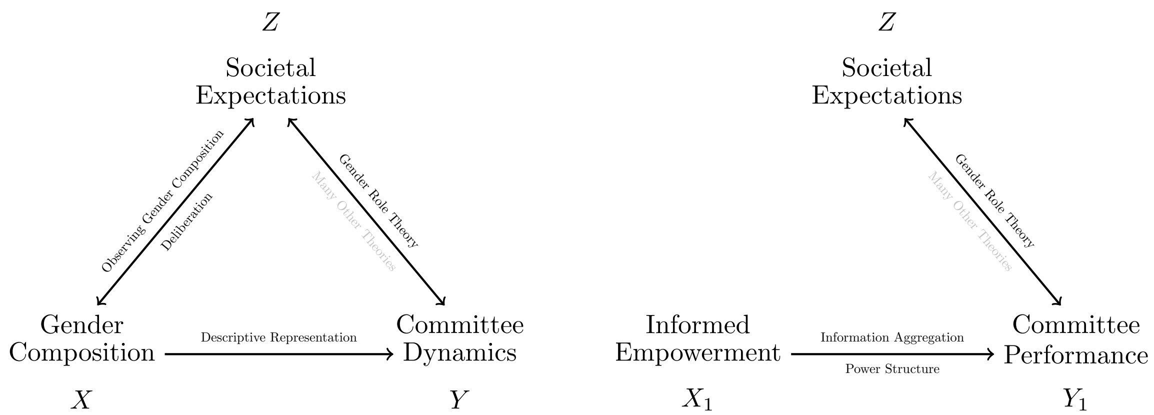
We examine the two mechanisms of informed empowerment through three experiments. To disentangle the effects of informed empowerment from other important drivers of gender differences—selection effects, careerism, stereotyping, and party affiliations, etc. (Folke and Rickne 2016; O'Brien and Rickne 2016; Klar 2018; Hassell and Visalvanich 2019; O'Brien 2018)—we construct fully anonymous environments in which gender com-

[1] To distinguish from stochastic noise, *information* in our theory always pertains to pertinent and essential knowledge about the outcomes of actions.

[2] As elaborated later, our claim is not that behavioral differences in information acquisition and the utilization of power are the exclusive cause of women's impact in committee voting. Nor do we suggest that these differences are innate or intrinsic in any way, as they may be the outcomes of long-standing patterns of socialization.

position is *not* observable, following Goldin and Rouse (2000) in studying blind auditions in orchestras. As our research design is practically impossible in the field, we conduct laboratory experiments to bypass these methodological hurdles and to establish counterfactuals and appropriate controls (Falk and Heckman 2009; Morton and Williams 2010; Woon 2012; Palfrey 2013). Some might argue that in "real-world settings," gender composition is almost always observed by committee members. Indeed, others have shown that knowledge of the gender composition of one's committee plays a role in decision-making (Karpowitz, Mendelberg and Shaker 2012; Mendelberg, Karpowitz and Goedert 2014). While these concerns reflect important variables well-documented in the literature, they are not limitations of our design, which seeks to tease out the unique effect of information on men and women decision-makers.

We illustrate this point in Figure 1a. Past work often relies on a framework where the independent variable (X) is the *observed* change in gender composition, and the dependent variable (Y) is the committee dynamics. While this canonical design provides compelling evidence that the *observed* change in gender composition may affect committee dynamics, the mechanisms explaining how X influences Y are linked to societal expectations (Z), such as women's comfort speaking authoritatively in a group of men. Whenever gender composition is observable, the results can more or less be attributed to existing explanations.



(a) Canonical Research Design (b) Our Design

Figure 1: Isolating the Effects of Informed Empowerment

We do not overlook or dismiss existing theories or any latent effects. Our choice of experimental design is intended to control for confounding factors. We do not remove the theoretical link between Z variables (i.e., alternative theories) and our dependent variable (Y_1, committee performance). We do, however, "erase the link" between committee voting and the perceived variation in gender composition that may shape societal expectations, as illustrated in Figure 1b. Our research design enables us to isolate the effects of informed empowerment (X_1) from the "bundled" alternative explanations provided by previous theories. Studying "bundled effects" both comprehensively and in isolation is crucial for fully understanding the dynamics at play and distinguishing the unique contributions of each factor (Falk and Heckman 2009; Morton and Williams 2010).

Our study represents a distinct contributing factor that complements existing explanations and improves the scholarly understanding of women's impact in committee voting in three ways. First, committees are generally employed for decision-making for two primary reasons: (1) to reconcile conflicting interests among members and (2) to aggregate information among members with aligned goals and preferences (Ostrom 1990). Little attention has been given to understanding how the presence of women enhances committee performance when all members' interests are aligned. Our theory complements previous work by addressing these significant yet understudied questions and emphasizing women's contribution in creating an informed majority, thereby improving the committee's ability to aggregate information about the state of the world. Moreover, we present one of the first scholarly attempts at explaining the critical role of informedness for the exercise of power and formal authority, which may answer how informed women leaders can improve committee performance even if women are the minority of the committee.

Second, we present a simple framework of how women improve the quality of decision-making in not only women's issues but also general committee voting. What constitutes a women's issue is challenging to define (Volden, Wiseman and Wittmer 2018), and limited scholarly work addresses the delineation of issues as distinctly belonging to women, men, or being gender-neutral. As scholarly understanding of gender grows, we see great value in understanding how men and women's behavior yields different outcomes in committee voting, without assuming *a priori* what issues women are most knowledgable about. However, there is little understanding of the effect of women's presence when gendered knowledge does not pertain to the decision. The literature has few scholarly insights on how we can generalize women's contribution in women's issues onto the more general issues that women do not have *ex-ante* expertise. Our framework does not require us to restrict women's ability to improve decision-making to women's issues, nor does it require us to strictly define such issues. Instead, we highlight the significance of behavioral patterns in information gathering and power utilization between women and men, which establishes a framework to generalize women's impact on committee outcomes.

Third, our study addresses *when* and *how* gender differences make a difference. Past work demonstrates that the number of women and/or whether women have authority sometimes but not always affects the outcomes of committee decision-making. We theorize that the impact of informed empowerment is contingent on (1) the extent to which individuals make their decisions independently and (2) the costliness of information. In committees where members merely function as instruments of political entities, individual behavior and information acquisition will not change based on the members' gender. However, when information empowers committee members' relatively independent decisions and gender differences in the effort for information lead to women being more informed, informed empowerment generates significant differences in committee voting.

Conceptual Framework

Information is pivotal for individuals to assess the *state of the world*—the underlying reality and facts of the matter at hand (Feddersen and Pesendorfer 1997). Intuitively, the ideal outcome for a committee is when its decision aligns with the state of the world. For example, in a jury, the underlying state of the world is whether the defendant is guilty. Jurors contribute to the jury when their presence helps to convict a guilty defendant or acquit an innocent one. Importantly, the committee process does not necessitate agreement among members regarding the state of the world (guilty or not guilty), but the information aggregated through the committee process mitigates the impact of individual biases on the final decision (Gilligan and Krehbiel 1989). Information diminishes uncertainty surrounding the implications of different perceptions and enhances the likelihood of collectively achieving the desired outcomes. *Information expertise*, being informed the pertinent and essential knowledge, is thus critical in the committee's decision process.

Expertise manifests in two forms. On certain matters, individuals draw upon their *priors*—personal experience, education, and professional skills. Women have been shown to be better informed on issues that they have already devoted more attention to than men (Reingold and Swers 2011; Volden, Wiseman and Wittmer 2018), especially those that disproportionately effect women. In these contexts, there is a higher likelihood that women have the informed answers in tasks involving these women's issues (Huddy, Cassese and Lizotte 2008). Increasing women's presence facilitates women's distinctive impact on women's issues (Mendelberg, Karpowitz and Goedert 2014).

However, many decision tasks of interest to political scientists are gender neutral or, at least, not strictly gendered. In these situations, especially when uncertainty surrounds the optimal course of actions, individual's gendered knowledge will not necessarily contribute to better decision-making. In tasks such as determining the guilt of a defendant, setting the optimal levels of agriculture subsidies, budgeting for the national defense, formulating policies to address pressing public health crises, and establishing ethical norms for sciences, relying solely on gendered knowledge or personal experience can lead to unclear and sometimes detrimental answers.[3] In such cases, individuals are compelled to invest effort in gathering information to understand the state of the world and discover the most appropriate response to the problem (Austen-Smith and Banks 1996).

Then, what are women's impacts in committee voting when the problem is gender-neutral, or when men and women possess identical *priors*? We propose that women's

[3]The distinction between gender-distinctive issues and gender-neutral topics can be as challenging as defining either one. Both encompass complex and multifaceted aspects that resist simple categorization. What specifically constitutes gendered or gendered-neutral topics falls outside the scope of this study.

presence will improve committee decision-making because they exert significantly more effort to gather information, and thus are more likely to be informed than men. This hypothesis is linked with several documented behavioral tendencies. First, women may work harder than men (Reingold 1996; Herrick 2010; Ashworth, Berry and Bueno de Mesquita 2023) because they anticipate different costs for participating and higher sanctions for deviating from stereotypes (Lawless and Fox 2005; Kanthak and Woon 2015; Preece and Stoddard 2015; Preece 2016; Pruysers and Blais 2017). Politics has long been viewed as a masculine domain (Woon and Kanthak 2019; Kanthak and Krause 2011), leading to the prevalence of double standards that disadvantage women (e.g., Huddy and Terkildsen 1993; Lawless and Pearson 2008).

Second, risk preferences influence individual proclivity toward acquiring knowledge about the state of the world. Risk-seeking decision-makers may rely on intuition and exert low effort in information acquisition, whereas risk-averse decision-makers may exert costly effort to become informed to avoid making decisions under strategic uncertainty (Kahneman and Lovallo 1993).[4] Women are generally more risk-averse (Niederle 2016; Croson and Gneezy 2009; Eckel and Grossman 2002; Byrnes, Miller and Schafer 1999) and less likely than men to guess in uncertain situations (Atkeson and Rapoport 2003; Mondak and Anderson 2004). As a result, women may exert more effort for information to make informed decisions.

Third, women's political priorities involve improving social welfare and serving their core constituencies (Barnes 2016; Betz, Fortunato and O'Brien 2021), while men tend to derive greater fulfillment from the personal, power-related, and political aspects of political influence (Thomas, Herrick and Braunstein 2002; Ou 2024). Women may have a stronger intrinsic drive to secure a desirable outcome for the group, perhaps due to higher collaborative preferences (Thomas 1995; Volden, Wiseman and Wittmer 2013) whereas men favor an individualistic approach (Thomas 2016; Kenney 1996). This may lead women to gather more information. Lazarus and Steigerwalt (2018) find that women

[4] *All else equal*, acquiring more information increases the likelihood of understanding the state of the world objectively, although individuals' beliefs of whether they are informed can be subjective.

legislators gather much more information than men, including relying on input from their constituents, requiring more information from staffers, and engaging with briefs at a higher level of intensity. Conversely, men typically rely more heavily on their own judgment and discretion (Herrick 2010).

We propose a behavioral theory of informed empowerment to explain how women's presence improves the *quality of committee voting*—the likelihood that the committee collectively identifies the true state of the world. The core of our theory is that in addition to normative arguments for equality, involving women in committee processes aims to tap into their unique information set, which may differ from that of men in the committee. A key objective of gender parity in decision-making bodies is that, through diversity and representation, the committee gains a better understanding of the state of the world. *All else equal*, informed empowerment can impact the quality of committee voting through two possible mechanisms:[5]

(1) *Informed Representation*: If everyone holds the same voting power as others and informed members constitute the majority, the committee will consistently identify the true state of the world with a higher likelihood than the committees in which informed individuals are the minority. Besides the reasons that women are half of the population and their opinions should be represented, as we expect women are more likely to be informed, women's presence will help the committee sustain the necessary level of information expertise to make reasoned choices.

(2) *Informed Authority*: If informed members hold positions of authority (e.g., procedural power such as vetoes, formal leadership roles, etc.), especially in situations where the committee's average level of informedness is low, their committees are more likely to identify the true state of the world compared to committees where uninformed individuals hold formal authority. If women are more likely to be informed and they will use power more effectively, then when women have authority, they can improve the committee's performance.

Experiment I

In naturally occurring situations, the counterfactual and the identification of a quality choice that matches the true state of the world is challenging to observe and measure. To bypass these constraints and disentangle the effects of informed empowerment from

[5] The formal analysis is reported in Appendix A.

existing theories, we conduct our study in a controlled environment to construct the measurement of decisions and eliminate alternative explanations documented in the literature. Experiment I was conducted with eighty registered student subjects at a standard experimental social science laboratory at a public U.S. university. We establish a stylized committee voting and investigate the effect of informed empowerment through *Informed Representation.*

Hypothesis 1 *Women-majority committees outperform women-minority committees in information aggregation and informed decision-making due to women's higher likelihood of being informed.*[6]

To empirically test our hypotheses, we use a between-subject design and examine the following experimental conditions: **4M1W** (each committee consists of 4 men and 1 woman) and **2M3W** (each committee consists of 2 men and 3 women). Participants are randomly assigned to experimental conditions. Once participants were assigned to a committee, it was fixed with no rematching. The gender composition is *unknown* to the committee members, so individual behavior should not change. The aggregate committee voting may vary depending on the gender composition. We elaborate on this point more in subsequent sections.

Committee members are asked to vote between two alternatives, $w = \{\mathbf{1}, \mathbf{2}\}$. Committee decisions are made by using *majority voting*: whichever alternative receives more than 50% of votes is the winner. Abstaining is not allowed. Voting is simultaneous and costless. For the purpose of identification, the state of the world is either $\omega = \mathbf{1}$ or $\omega = \mathbf{2}$, and each committee member receives a payoff of $\overline{u} = 100$ experimental currency units (hereafter, ECUs) if alternative $w = \omega$ is adopted in state of the world ω, and $\underline{u} = 0$ otherwise. Thus, as discussed in Feddersen and Pesendorfer (1997), the goal of the committee is to *collectively* choose an alternative that matches the true state of the world.[7]

[6] Our study and hypotheses were preregistered at [link redacted].

[7] Using the example discussed in the conceptual framework, neither women or men desire to convict an innocent or acquit a guilty defendant. However, their voting choices might diverge due to varying perceptions of the state of the world, influenced by the essential knowledge (i.e., information) they have acquired.

Substantively, this can be thought of as choosing the policy alternative that actually solves the problem at hand.

Procedures and Setting

Figure 2 illustrates the procedure for our experiments. We report full details and methodological matters in Appendix E. Here, we clarify several specific design choices regarding the measurement of information acquisition.

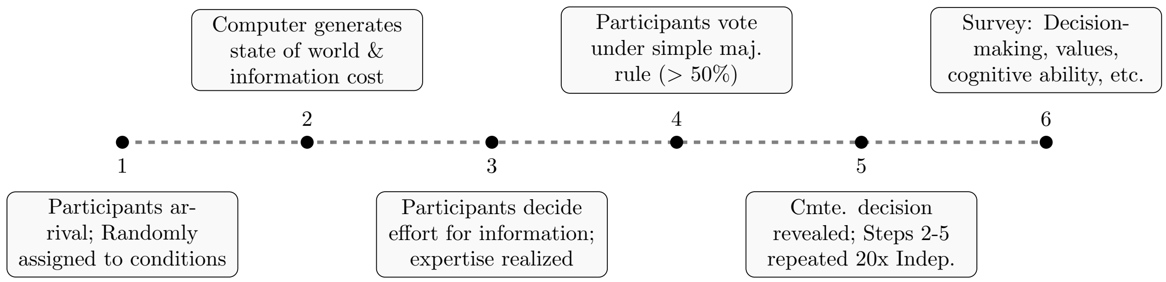


Figure 2: Timeline and Procedures of Experiment

To capture the uncertainty inherent to much committee decision-making, the state of the world is not *ex-ante* known to committee members. They need to exert effort to acquire information. To assess the level of effort exerted by a committee member, we consolidate various information gathering activities into a single measurement. This design is driven by two main considerations. First, information acquisition takes place through a variety of formal and informal channels, and individual members gather information both individually and collectively before, during, and after committee meetings. For example, jurors are not allowed to engage in discussions with attorneys, witnesses, fellow jurors, or anyone involved in the case while the trial is ongoing. They are required to focus on the evidence presented in the courtroom and the judge's instructions on the applicable law. After all evidence has been presented, jurors may share personal insights, reevaluate evidence, and seek clarification from fellow jurors before reaching a verdict.

Second, because the means by which information is obtained and the cost of obtaining it varies from committee to committee (e.g. juries, promotion and tenure committees, corporate boards, and so on), we take a context-neutral approach. We abstract diverse forms of information gathering activities as an overall effort for information, which enables us to examine our theory and explore the research questions without concern for the application of a precise context and definition of institutional factors (Brutger et al. 2022; Morton and Williams 2010; Smith 1982). By treating information acquisition as a unified process throughout the committee proceedings, we simplify our experimental procedure, enable a more straightforward measurement of effort for information expertise, and avoid the complexities that would arise from separately measuring information acquisition.

Before casting their votes, committee members make independent information acquisition decisions for themselves. Each participant is given 100 ECUs, which they may keep for exchange of cash payment in U.S. dollars, or they can spend some or all the 100 ECUs on acquiring expertise. An individual's *willingness to acquire information* is measured by how much they pay to be informed about the state of the world. The difficulty of the problem is measured by the *ex-ante* unknown cost of information generated by the computer, which is an integer between 1 and 100.[8] Participants were informed of the state of the world and paid for the stochastically generated information cost when their willingness to acquire information is equal to or higher than information cost. The cost of information is randomly and independently generated, so the more effort exerted by the participant, the more likely the participant is informed. This design captures the fact that, *all else equal*, whether an individual can obtain expertise is determined by both how much effort one is willing to exert and how hard the problem is.

Since we measure information acquisition as one single decision, we take steps to capture a set of potential effects generated through various information gathering activities. In our experiments, when informed, this information is true 90% of the time and wrong 10% of the time. Our design suggests that information expertise helps individuals to understand the state of the world with sufficiently high confidence. It also accommodates the critical functions of information exchange before and during meetings, because all the essential knowledge one gains from meetings and communication pertains to the state of

[8]This design enables us to estimate the distribution of treatment effects detailed in Appendix D.

the world (Fehrler and Janas 2021; Kartal and Tyran 2022; Ou and Tyson 2023). We capture the non-essential knowledge content introduced by social context and social cues through our "noisy" signal of the state of the world (i.e., being informed wrongly 10% of the time).

Controlling for Alternative Explanations

We implement necessary controls to ensure clean inference. First, we develop a neutral committee-based task that neither men or women have gendered advantages. We also make each state of the world equally likely so that any behavioral differences identified in our work are not driven by gendered differences in misperceptions about probabilities (Kahneman and Tversky 1979). Thus, gendered expertise and selection effects are controlled.

Previous work suggests that women participate more when observing a majority of women on the committee, a phenomenon explained by the interpersonal and social aspects of gendered social roles (see a survey in Mendelberg and Karpowitz (2016)). Briefly, they examine changes in individual behavior (dependent variable) as a function of committee gender composition (independent variable). Our study introduces a novel theory that, while not excluding the insights that knowing gender composition affects gendered behavior or other latent gendered dynamics within committees, emphasizes a mechanism of *informed empowerment*. This mechanism operates independent of knowledge of gender composition and, we argue, can still lead to improved committee performance. To isolate our findings from these alternative explanations, we construct a fully anonymous environment, following Goldin and Rouse (2000) in studying blind auditions in orchestras. In each session, five participants remotely joined the experiment over an application in which everyone's identity was purposely blinded. Participants did not know the gender composition and they made experimental decisions simultaneously. They can ask questions by messaging the experimenters, but they cannot message each other. The information acquisition process is private.

To reduce the likelihood of Type II error caused by stochastic factors (such as lucky guesses or extremely high/low information cost stochastically generated in the experiments), participants engaged in 20 periods of committee voting.[9] Each period operates independently with varying parameterizations; relevant parameters—such as the state of the world and information costs—are randomly and independently redrawn from period to period. We follow the methodological advice of Fréchette (2012)—participants received feedback on the previous period's committee decision but no feedback regarding the realized information cost, who acquired information, who obtained expertise, or the voting decisions of others. The limited feedback between periods and experiment parts cuts down on possible cross-decision influences and ensures that Experiment I's results are comparable to results in Experiments II and III (that we will report later).

In so doing, we believe we construct an experimental design that is arguably "least likely" to identify gendered committee behavior. Thus, our findings may actually be *lower* bounds of how *Informed Representation* affects committee voting.[10]

Results of Experiment I

We focus on *Informed Representation*'s effects on the quality of committee voting.[11] We use a regression-based statistical analysis and use cluster-robust standard errors, allowing for correlation between observations within a given committee. We also examine our findings by performing an arguably more conservative, analogous non-parametric analysis (reported in Appendix B.1). The two statistical analyses generate qualitatively identical results.

We find that on average men pay 20.61 to acquire information. Women pay 50.18, about 2.5 times the average amount men pay ($\Delta = 29.58$, $p < 0.001$, see Appendix B.4). It follows that women obtained expertise about 50% of the time, which is significantly

[9] Our findings are robust to these stochastic factors, see Appendix C.

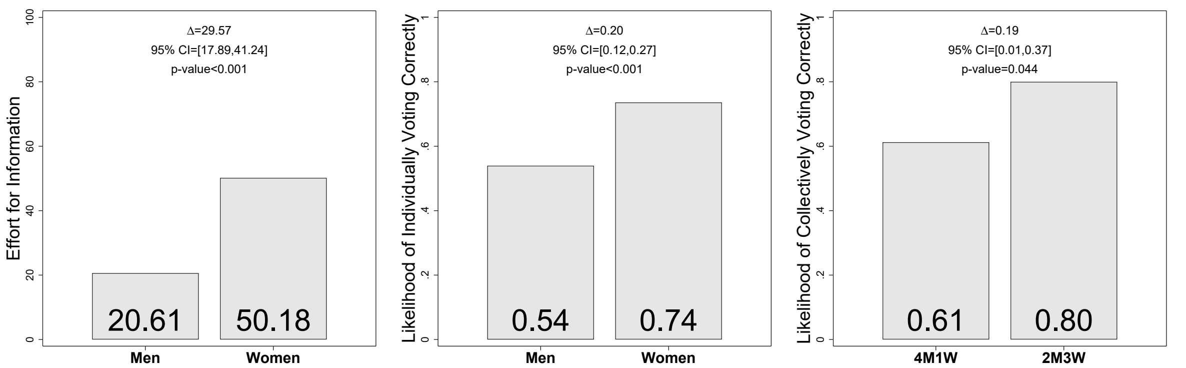
[10] In a less controlled environment, the comparisons between **4M1W** and **2M3W** should consider the behavioral effects that we controlled, such as women are more likely to engage in discussion and make their voice heard when they observe a majority of women on the committee. We believe those other behavioral effects will further amplify the effects of *Informed Representation* as the changed dynamics among committee members may decrease the cost of information acquisition.

[11] The analysis of individual behavior, distribution of treatment effects and the discussion of when gender differences make a difference are reported later in another section.

higher than the 21% likelihood that men obtained expertise. The gender differences in information acquisition results in different levels of information expertise between experimental conditions. In **4M1W**, the average level of expertise at the committee level is about 20%, but in **2M3W**, it is significantly higher at 45% ($p = 0.005$).

A higher level of expertise results in a higher likelihood of identifying the true state of the world. We measure the quality of committee voting by examining the likelihood committees' choices match the true state of the world. We find that the quality of committee voting is 61% in **4M1W**, but 80% in **2M3W**, a 19-percentage point difference and 31% increase in decision accuracy. This difference demonstrated in Figure 3 is both statistically ($p = 0.044$) and substantively significant. Our results suggest that a voting committee with three women is more likely to identify the true state of the world than a voting committee with only one woman, which supports Hypothesis 1.[12]

Figure 3: Gender Differences and Effects of Informed Representation



Notes: Results of Experiment I. The left panel reflects the effort for information. The middle panel reflects the likelihood of individually voting for the true state of the world. The right panel reflects the committee-level likelihood of collectively voting for the true state of the world. *Takeaway*: Since women on average exert more effort for information, they are more likely to be informed and thus vote correctly, and a committee in which women are the majority is more likely to collectively identify the true state of the world than a committee in which women are the minority.

Experiment II

To examine the effects of *Informed Authority*, we now consider a committee in which every committee member can vote, but one of them has veto power. We use veto in our experiment to represent authority for two reasons. First, this design enables us to

[12]Our preregistration, sample size calculation, and power analysis are reported in Appendices E.5, E.6.

precisely investigate how men and women, especially those who have formal authority, use their power differently, and how their level of informedness affects their decisions and committee outcomes. Second, our design is one of the more straightforward ways to convey to participants the procedural power and formal leadership of authority figures. Veto power is one of the most representative modes of formal authority (Gilligan and Krehbiel 1987; Tsebelis 1995; Bouton, Llorente-Saguer and Malherbe 2018) and vetoes are common across a variety of decision-making bodies (Hassell and Kernell 2016). The American House and Senate each have veto power over a bill after it is sent back from a conference committee; Provost or Deans at universities may have veto power over promotion and tenure decisions; and the chair of the board and CEOs also wield veto power over crucial decisions in corporate governance.

We heretofore refer to the committee members with veto power as the *veto players*. To make a decision, the committee first performs the voting procedure that is identical to *majority voting* implemented in Experiment I. The veto player makes information acquisition and voting decisions like the other committee members. After everyone casts the vote, the veto player is told how many votes are for **1** and how many votes are for **2**. Then the veto player decides whether to veto. We set **2** as the default winning option (i.e., status-quo). When the veto is not used, the voting outcome is the majority's choice. The veto player can block changes from the default winning option. When the veto is used, even if the majority votes for **1**, the veto player can block passing **1** and thus make **2** the voting outcome.[13]

A rational veto player should not use their veto when they are uninformed, but should follow the choice of the majority. Rational veto players will only use their vetoes when they are informed and when the majority chooses the alternative that is different from their informed state of the world. While a veto player may be a man or a woman, if women

[13]Setting either **1** or **2** as the winning default option does not affect the analysis. Consider a local committee, which votes to change the speed limit from 35mph to 45mph. A veto player may have private information about accidents on this road, which will increase if the speed limit is raised, and veto a change from the status quo to 45mph. She cannot, however, veto if the group decides to maintain the extant speed limit. So too in our study, the veto player can veto a change from status-quo **2** to new policy **1** but cannot veto the group's decision to maintain the status quo.

are more likely to exert costly effort to acquire information than men, then women in general should be more likely to be informed. Also, as discussed earlier, women are more inclined to utilize political power to drive policy changes, while men often leverage power for personal fulfillment. Consequently, informed women in the role of veto players are well positioned to wield their power. When the majority of the committee fails to identify the state of the world, veto power will play a critical role, particularly when women are veto players.

Hypothesis 2 *Women use veto with greater caution than men. A committee in which a woman serves as the veto player is more likely to identify the state of the world than a committee in which a man serves this role.*

We test Hypothesis 2 by examining another two experimental conditions in which each committee consists of 4 men and 1 woman: **MVeto-4M1W** (a man is the veto player) and **WVeto-4M1W** (a woman is the veto player). We use the same experimental procedures of **4M1W** as in Experiment I with one important variation—there is a veto player in each committee. Participants were randomly assigned to experimental conditions and the computer chose who were veto players. Only veto players knew whether a veto was used; the regular committee members received no feedback regarding how the others voted and were not told whether a veto was used.[14] The average duration of a session is about 75 minutes. The average earnings of each participant were about $23, including a $7 show-up payment. Another 80 subjects participated in Experiment II.

Results of Experiment II

In Table 1 we report the comparisons of committee performance across experimental conditions.[15] The default comparison treatment is **4M1W**. The dependent variable is the quality of committee voting. Note that each committee consists of 4 men and 1 woman among **4M1W**, **MVeto-4M1W**, and **WVeto-4M1W**. To identify the effects

[14]This ensures that regular members' behavior is comparable between Experiment II and Experiment I and enables us to perform relevant robustness checks reported in Appendix C.2.

[15]There is no demographic difference between participants in Experiment I and Experiment II.

of how women and men use veto power on political outcomes, we compare the results of **MVeto-4M1W** and **WVeto-4M1W** to the results of **4M1W**, respectively. We find that the likelihood of collectively identifying the state of the world in **MVeto-4M1W** is statistically identical to in **4M1W** ($p > 0.1$). However, even if the gender composition is identical and there is only one woman in each committee, when women serve as the veto player, committees in **WVeto-4M1W** are significantly more likely to collectively identify the true state of the world than committees in **4M1W** (87% vs 61%, $p = 0.004$). The comparisons between Column 1 and Columns 2-4 suggest that the effects of *Informed Authority* are not caused by *Informed Representation*.

Table 1: Effects on the Likelihood of Achieving High-Payoff Outcomes

	Dependent Variable: High-Payoff Outcome = 1				
Treatment Variables	(1)	(2)	(3)	(4)	(5)
2M3W ($\hat{\beta}_1$)	0.189**				0.180***
	(0.084)				(0.082)
MVeto-4M1W ($\hat{\beta}_2$)		0.019		0.017	0.016
		(0.055)		(0.049)	(0.046)
WVeto-4M1W ($\hat{\beta}_3$)			0.260***	0.281***	0.270***
			(0.065)	(0.071)	(0.069)
Observations	320	320	320	480	640
Clustered Analysis	Yes	Yes	Yes	Yes	Yes
No. of Committees	16	16	16	24	32
Log-Likelihood	-186.883	-212.145	-169.020	-274.346	-354.410

Notes: Probit specification. Each column reports average marginal effects from Probit regressions. Robust standard errors are reported in parentheses.
Results of Experiments I and II. The independent variables *2M3W*, *MVeto-4M1W*, and *WVeto-4M1W* are binary indicators that represent voters' experimental groups. The default comparison condition is *Regular-4M1W*.
Column 1 focuses on *Informed Representation.* Columns 2-4 investigate *Informed Authority.* Those results are qualitatively the same to the results reported in the Appendix B.1 based on the non-parametric Fish-Pitman permutation tests. ***, **, and * denote significance at the 1%, 5%, and 10% levels. *Takeaway*: Both *Informed Representation* and *Informed Authority* positively and significantly promote the committee's likelihood of achieving high-payoff outcomes, while the effect of the *Informed Authority* is significant only when women serve as the veto player.

The committees collectively achieve high-payoff outcomes only 63% of the time in **MVeto-4M1W**, but about 87% of the time in **WVeto-4M1W**, a 24 percentage point difference and a 28% increase in decision quality. The difference is both statistically ($p = 0.002$) and substantively significant, supporting Hypothesis 2. Column 5 of Table 1 suggests that the effects of *Informed Authority* are statistically identical to the effects of *Informed Representation* ($\hat{\beta}_3 - \hat{\beta}_1 = 0.090$, p-value> 0.1).

Experiment III

The committee voting may cause *strategic effects*, wherein information acquisition and voting decisions are affected by the conduct of fellow committee members. Individuals may strategically comport themselves to influence the decisions of others. While we have imposed the necessary controls of information in Experiments I and II, and we find no evidence showing that regular committee members' behavior is different between Experiment I and Experiment II, a remaining question is whether the gender differences identified in Experiment II are driven by informed empowerment or strategic considerations. The answer to this question is critical for us to interpret the implications of this study.

Strategic effects manifest in two forms. First, regular committee members may strategically respond to veto players' actions, and reduce their effort to acquire information under the assumption that the veto players will make a pivotal decision. Second, veto players may strategically deploy their veto authority to influence the decision-making of other committee members. For example, although the veto player is uncertain about who is informed and how much effort was exerted, the veto player may exert low effort to acquire information or engage in uninformed vetoing to "punish" other members. Experiment III is thus designed to examine whether our findings are robust to strategic effects. We used similar settings, procedures, and sixty new participants (half are women) recruited from the same subject pool to conduct Experiment III.

To isolate strategic effects, each participant was matched with four members (who did not participate in Experiment III) from a committee arbitrarily retrieved from Experiment II. We used the decision choices of four members from Experiment II, replicated identical variables such as information costs and the state of the world stochastically generated in Experiment II. As a result, participants of Experiment III made decisions under the same information and parameterizations as the participants of Experiment II, while their decisions only affect their own outcomes but have no consequences on the other committee members' decisions and payoffs. Thus, the two strategic effects are removed in Experiment III.

Participants were randomly assigned to experimental conditions, 20% became regular committee members, and 80% became veto players. They were explicitly instructed about the experimental setting. No deceptive practices were employed. More details of Experiment III are reported in Appendices B.2 and E.1. We find that both regular committee members' and veto players' behavioral patterns are statistically identical to the observations in Experiment II, suggesting that strategic effects cannot explain the main findings of Experiment II.

Individual Behavior

In this section we analyze patterns of individual behavior and decompose gender differences in committee. The unit of analysis is the averages of individuals' decision choices over the 20 periods committee voting. As participants of Experiment III made decisions under the same parameterizations as the participants of Experiment II, and both regular members' and veto players' behavioral patterns are statistically identical to the observations in Experiment II, we combine results of Experiments II and III in the analysis of veto players' behavior.[16]

Why do committees with women veto players perform significantly better than those with men veto players? We find that the performance differences are not driven by regular committee members' behavior, but the veto power induces veto players to exert significantly more effort to acquire information than the other same-gender participants.[17] Men veto players pay an average of 34.17 ECUs to be informed, which is significantly

[16] As detailed in Appendix C.2, whether we combine these data or analyze them separately will not change the conclusions.

[17] We do not find any statistically significant difference in the effort for expertise (1) among men who have no veto power between **MVeto-4M1W** and **WVeto-4M1W**, (2) among women between **MVeto-4M1W** and **4M1W**, or (3) between men who have no veto power in **MVeto-4M1W** and men in **4M1W**. We report more details of these results in Appendices B.2 and B.4.

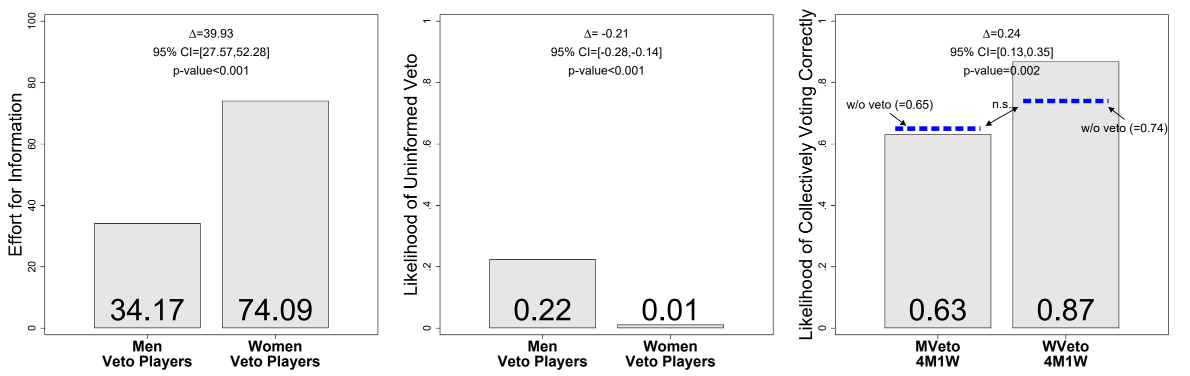
higher than the 19.96 ECUs paid by the other men who cannot veto ($p = 0.024$) and the 20.61 ECUs paid by the men in treatments without the veto power ($p = 0.015$). Likewise, women veto players pay 74.09 ECUs to be informed, which is significantly higher than the 47.84 ECUs paid by the women who cannot veto ($p = 0.001$), and the 50.18 ECUs paid by the women in experimental conditions without the veto power ($p < 0.001$). Importantly, although both men and women veto players exert significantly more effort to acquire information than their same-gender committee members, among veto players, women still pay significantly more than men in the information acquisition process ($\Delta = 39.93$, $p < 0.001$).

Information acquisition is correlated with individuals' values and beliefs. The added responsibility to veto players may explain why they exert more costly effort to acquire information as compared to other committee members. We conducted a post-experiment survey in which we asked participants: "How important was it to you to acquire information?" and "To what extent do you think your vote affects the voting outcomes?" As reported in Appendix B.3, relative to regular committee members, we find that both for men and women, veto players expressed that information acquisition was significantly more important to them, and veto players have a stronger belief that their votes affect the voting outcomes.

How veto players use their veto power also directly affects committee outcomes. A clear behavioral pattern observed in our experiment is that veto players are significantly more likely to exercise their veto power when the majority's choice differs from their individual vote (see Tables A8 and A9). There are significant gender differences in the behavioral patterns associated with the use of veto power. Among veto players, men on average were less likely to be informed than women; despite this, men ($N = 32$) used their veto 25% of the time, while women ($N = 32$) used veto power about 17% of the time ($\Delta = -0.089$, $p = 0.040$, Appendix B.6). When uninformed, men used their veto approximately 22% of the time, while women vetoed only about 1% of the time ($p < 0.001$, Table A7). Further, uninformed men used their veto power about 17% of the time when their initial votes *matched* the majority's choice, and more than 30% of the time when their initial votes differed from the majority's choices.

If veto players were uninformed but used their veto power, they may undermine the efforts of other committee members who are informed (Feddersen and Pesendorfer 1996; Battaglini, Morton and Palfrey 2010). As a result, men veto players' uninformed vetoes negatively affect the potentially-positive effect of the veto power. This may explain why when men serve as veto players, the veto mechanism has no or even a negative effect on the quality of committee decision-making. As illustrated in the right panel of Figure 4 and detailed in Appendix B.1, if we estimate committee outcomes based on individual members' vote before veto is used, then with the same set of participants, the quality of committee voting is about 0.65 in **MVeto-4M1W** and 0.74 in **WVeto-4M1W** ($p > 0.1$). This suggests that, because women almost only make informed vetoes, their serving as the veto players improves the quality of committee voting from 0.74 to 0.87 ($p < 0.05$) through *Informed Authority*.

Figure 4: Gender Differences and Effects of Informed Authority



Notes: Results of Experiments II and III. See Appendices B.2 and E.1 for additional details. A total of 64 participants (half are women) served as veto players. The left panel reflects veto players' effort for information. The middle panel reflects veto players' likelihood of using vetoes when uninformed. The right panel reflects the committee-level likelihood of collectively voting for the state of the world. The blue dashed lines report the quality of committee voting if vetoes are never used. *Takeaway*: Since women veto players on average exert more effort to acquire costly information, they are more likely to be informed and know the state of the world, and thus a voting committee in which women have veto power is more likely to collectively identify the true state of the world.

We find no difference in cognitive ability between women and men in our sample, measured by a cognitive reflection task (Frederick 2005) implemented post main experiment, between men and women veto players. We also find both men and women in general follow their acquired information to vote when informed. When they are uninformed, they vote for Jar 1 with a likelihood that is statistically identical to 50%. There is no significant difference in this behavioral pattern between regular committee members and veto players, or between men and women (as reported in Table A6).

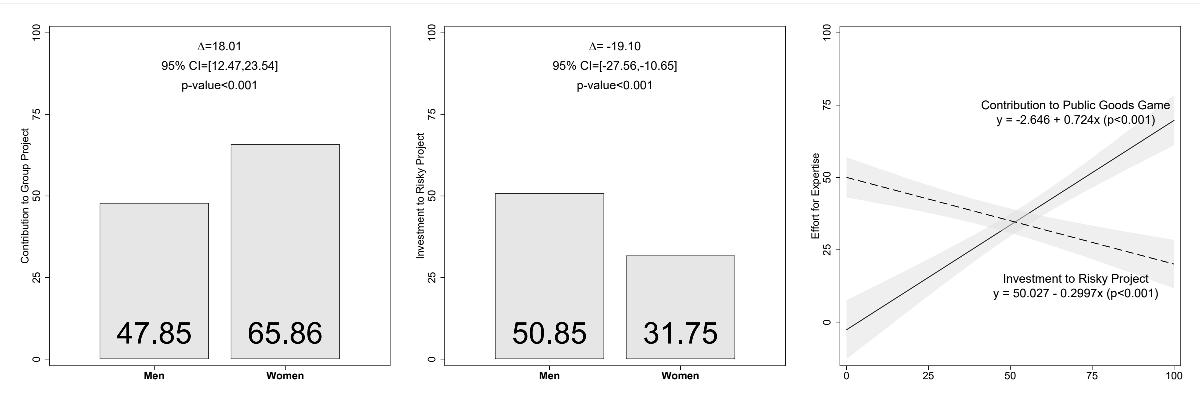
Risk and social preferences are correlated with individual behavior. To investigate whether women exhibit greater tendencies toward collaboration and risk aversion compared to men, we conducted supplementary measurements of social and risk preferences in all our experiments.[18] We first conducted a threshold public good game, which is widely applied to elicit social preferences, and use individuals' contributions as a proxy to examine whether women are more collaborative (Palfrey and Rosenthal 1984; Bicchieri 2016). In this game, each member of the same 5-person committee decides how much of 100 ECUs to contribute to the group account. Only when the group's aggregate contribution is equal to or over the threshold (i.e., 250 ECUs), individuals can benefit from the contribution. Otherwise, if the group's aggregate contribution is less than the threshold, everyone loses their contribution. We also measure risk preference using the method developed by Gneezy and Potters (1997). Each decision-maker chooses how much of 100 ECUs she or he wishes to invest in a risky option and how much to keep. Both of these two measurements are incentivized with real monetary consequences. They are incentive-compatible such that participants can achieve the best outcome by acting according to their true preferences.

We find women contribute 65.86 ECUs to the group account in the threshold public good game, which is significantly more than the 47.85 ECUs contributed by men ($p < 0.001$). Meanwhile, women invest 31.75 ECUs to a risky option, which is significantly less than the 50.85 ECUs invested by men ($p < 0.001$). These results suggest that, consistent with the general results of the literature, women are more collaborative (and/or other-regarding) in collective action and they are more risk-averse. These findings are supported

[18]Details of these measurements are reported in Appendix E. To avoid the influence of playing previous part(s) on the decisions in latter part(s), participants received no feedback regarding the outcomes of each part until the end of the session.

by the results of our post-experiment survey that it was more important for women to become informed voters because they care more about the group's aggregate benefits (Appendix B.3).

Figure 5: Summary of Gender Differences in Collaboration and Risk Preferences



Notes: The left panel reports gender differences in contributing to the threshold public goods game. The middle panel reports gender differences in investments to the risky project. The right panel reports the correlations between the effort for expertise and collaboration and risk preferences, respectively, based on individual averages (N=220). *Takeaway*: There are positive correlations between effort level and collaborative preference and negative correlations between effort level and risk preference.

We further find significant correlations between individuals' social and risk preferences and how much effort they exerted in acquiring costly expertise. These results are demonstrated in Figure 5 and Appendix B.5. Specifically, individuals' tendencies to contribute in collective action is positively correlated with their effort for expertise, and their risk preference is negatively correlated with their effort for information. Moreover, among veto players, more risk-seeking individuals are more likely to veto when uninformed, while more collaborative and other-regarding veto players are less likely to veto when uninformed (Appendix B.6).

Discussion

Whether or not the share of women and men in groups is related to committee performance does not depend only on the percentage of each gender on the committee but also on the information each member brings, how the committee is organized, and the interplay between information, gender composition, and the power structure. Numbers definitely matter, but how those numbers affect the committee's level of expertise and how they influence the committee's ability to make informed decisions is equally critical. Our study investigates these questions, complements existing explanations, and improves the scholarly understanding of women's impact in not only women's issues but also general and neutral committee voting.

When gender differences make a difference

While our findings provide robust support for our hypotheses, our interpretation of the results is more modest. We recognize that the impact of informed empowerment is conditional on various contextual factors within decision-making settings. Politics is complex. Gender is not the sole determinant of the committee process.

The extent to which individuals make independent voting decisions shapes the impact of gender differences in the committee process. In committees where members merely function as proxies or instruments of political entities, the prioritization of information acquisition in the committee process may diminish. In such cases, gender differences do not translate into the choices of committee members. Consequently, how many men or women are on the committee and who has formal authority may not change outcomes.

While party, interest group, and/or constituency may play a role in certain contexts, the autonomy of committee members in decision-making processes is evidence across a wide range of scenarios and different forms of committees. Even in committees in which political entities may exert significant influence, women often exhibit distinct viewpoints or approaches compared to their male counterparts from the same party (Mendelberg and Karpowitz 2016). Thus, the extent to which individuals make independent voting decisions bounds but does not eliminate the generalizability of informed empowerment.

To understand when gender differences make a difference, it is also critical to study gender effects across contexts and committee tasks. Previous studies have analyzed gender effects within specific contexts, such as legislatures, corporate governance, juries, or public health crises, but variations in committee tasks and contexts are rarely discussed. Since the context of the political process, which reflects the difficulty in being informed, decides whether gender effects are empirically significant, the lack of investigation into the distribution of gender effects across diverse committee tasks may contribute to the seemingly contradictory findings regarding women's impact documented in the literature. To address this issue, the use of meta-analysis that combines results from multiple studies conducted on different samples, in different contexts, or at different times is needed (Imbens 2010; Slough and Tyson 2023).

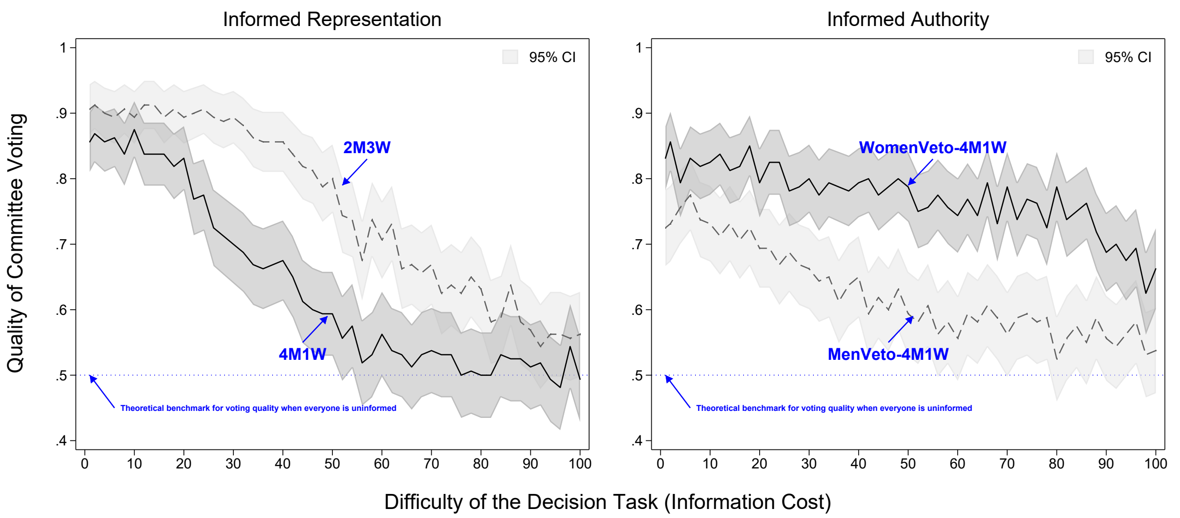
As we report a single study's results, we could not directly investigate the effects of informed empowerment across samples, contexts, and times as a meta-analysis does. However, the comparisons between *Informed Representation* and *Informed Authority* will help us understand alternative mechanisms that empower women to contribute to committee voting across gender compositions and power structures. Moreover, if we abstract the variations of contexts as a function of information cost (i.e., how hard the problem is to solve) and map such variations into a distribution of information cost, then within our theoretical framework, we can leverage our experimental design and extend the investigation of gender effects beyond simple aggregate comparisons to encompass distributional analysis.[19]

Notably, at the aggregate level, the substantial positive impact of women veto players on the quality of committee voting closely mirrors the effects of increasing women's numbers within the committee, which may answer how informed women leaders can improve the outcomes of committee decision-making even if women are the minority of the committee. Figures 6 illustrate the distribution of effects with respect to varying levels of information cost for *Informed Representation* and *Informed Authority*, respectively. We show that when the task is either sufficiently simple (i.e., the cost of information is low such that everyone is informed) or complex (i.e., the cost of information is high such that everyone is uninformed), gender differences in information acquisition may not yield discernible effects. Moreover, when the information cost is relatively low, increasing the numbers of those who are more likely to be informed generates about the same or slightly better effects as when informed individuals hold veto power. But when the information

[19]Detailed methodologies are reported in Appendix D.

cost is relatively high, *Informed Authority* is likely more effective than *Informed Representation*. Our theory and experiments thus offer insights into the scope and nuances of gender effects across information cost.

Figure 6: Distribution of the Informed Empowerment's Effects by Information Cost



Notes: Our experimental design elicits individuals' decision rules, allowing us to extend the investigation of effects from point comparisons to distributional comparisons without relying on stronger methodological assumptions than the methods widely applied in the literature. We detail the protocols of the estimation in Appendix D.

We speculate that the decision-making tasks of committees often are of moderate difficulty. Whether determining the guilt of a defendant, devising the strategic plan for an organization, allocating budgets in governments, making decisions on hiring or promotions/tenure, addressing pressing public health crises, or establishing ethical norms for sciences, these decision tasks are not overly simplistic or excessively complex. We speculate and as Figures 6 illustrate, informed empowerment, either through *Informed Representation* or *Informed Authority* or both, affects the committee's performance in a significant proportion of cases. Through a different theoretical perspective, our results qualitatively align with the findings of women's positive impacts on political decision-making documented in extensive literature.

Though conducted in the lab, our study resembled the naturally occurring committees in important aspects, as these committee processes essentially consist of information acquisition and voting. While some concerns may revolve around the mundane realism of experiments, with comments arguing that committee interactions and dynamics in one or other settings differ from our abstract and controlled environment, it is important to reiterate that the neutral committee tasks, anonymity, and incentive schemes were designed to disentangle the effects of informed empowerment from otherwise bundled effects. Our approach aligns with established practices (Brutger et al. 2022; Morton and Williams 2010), which show that abstraction and careful controls, like those employed in our study, can yield results with equal or even greater levels of generalizability compared to experimental protocols attempting to mimic specific, complex political scenarios. As we report a single study's result, our primary focus is on understanding how gender differences and information expertise affect general committee processes. Following established conventions in experimental methodology, we anticipate that the conclusions drawn from our experiments can provide valuable insights into human behavior at different levels of committees when decision makers face the same tradeoff outside the laboratory (Morton and Williams 2010; Druckman and Kam 2011; Falk and Heckman 2009; Snowberg and Yariv 2021), and the behavioral differences between women and men identified in our study offer substantive implications that can extend to the understanding of gender effects in committees that require information acquisition (Potters and Van Winden 2000; Fréchette 2015; Kertzer and Renshon 2022).

Implications

Informed Representation emphasizes the significance of an informed majority, aligning with the effects of women's participation and authority discussed by past work. Women contribute to addressing (women's) issues because they devote more attention than men to these issues (Reingold and Swers 2011; Volden, Wiseman and Wittmer 2018), resulting in a higher likelihood of being informed. Women are more likely to participate when they possess the essential knowledge about the question at hand (Payson, Fouirnaies and Hall 2023). According to our theory and experimental results, women use their power with more caution. Thus, an interpretation of *Informed Representation* is that during committee meetings, the cost of information acquisition may be correlated with gender composition and the overall level of informedness of the committee, and women may be more willing to participate in deliberation when they are more confident of their informedness. If so, an implication of our theory is that women's numbers matter not only because of the social effects of holding the majority, but also because the overall level of informedness is increased and the cost of acquiring information expertise is lowered within a predominately female group.

Our theory also suggests that the share of women and men in committee does not mechanically contribute to committee outcomes, which echoes the findings of Karpowitz and Mendelberg (2018) and Mendelberg, Karpowitz and Goedert (2014). We argue that having an informed majority is critical. This implies that gender composition's impact may be nonlinear. Indeed, Mendelberg, Karpowitz and Goedert (2014) find that under majority voting the effect of gender composition is significant when women's number increases from one to three in a five-member committee, but not when the number of women increases from three to five. Our theory complements these explanations by highlighting that an informed majority consistently drives desirable outcomes. Thus, discussions of how gender diversity affects committee decision-making shall not only focus on the specific number of women involved but also how women's presence contribute to the formation of an informed majority.

Informed Authority adds to past work demonstrating that women serving critical roles in governance yield positive outcomes. Instead of solely emphasizing gender-based value differences and/or selection effects, we underscore the importance of information expertise in discussions of leadership and power structures. Our work extends previous work which finds that the unanimity rule helps women's participation and authority when they are in the minority. Relative to previous work, we demonstrate that whether a veto player holds information expertise will decide the quality of committee voting, and uninformed veto may negatively affect the expected positive effect of veto power, suggesting a tradeoff between the unanimity rule and desirable outcomes. On one hand, the unanimity rule promotes equity since every member is effectively a veto player, which helps women's voice being heard even if they are the minority of the committee. On the other hand, extensive studies show that the unanimity rule is less efficient and often generates socially suboptimal outcomes than the simple majority rule (Feddersen and Pesendorfer 1998; Austen-Smith and Feddersen 2006). *All else equal*, the quality of committee voting decreases as the number of uninformed veto players increases. When women are in the minority, even if the unanimity rule helps them voice, the committee outcome is not necessarily better than under the majority rule. Bouton, Llorente-Saguer and Malherbe (2018) show that the majority-veto voting rule is more efficient than the unanimity rule meanwhile it protects the minority as the unanimity rule. Our study extends previous work by demonstrating that whether men or women serve as the veto player makes a difference, providing a viable topic for future work.

Our findings open a novel set of questions for political scientists. Are women who exert significant effort in pre-meeting activities more likely to participate during deliberations? To what extent are informed members affected by "loud" but uninformed members' opinions during committee meetings, and are there gender differences in this regard? What is the relative weight of individual information acquisition (before committee meetings) compared to collective information acquisition (during committee sessions) for women and men, respectively? These remain fascinating open questions and suggest the need for field studies to test our hypotheses in a less controlled setting.

Overall, informed empowerment examines how gender differences in information acquisition influence the committee's functioning, complements existing theories by encompassing the meaningful impact of women's presence beyond gender-specific contexts, and offers insights on when gender differences make a difference to committee outcomes. We highlight that information empowers committee members such that gendered differences in information acquisition affect the effects of representation and authority, which introduces a distinctive and potent dimension that stands as a promising avenue for future scholarly exploration.

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Online Appendix: Informed Empowerment in Committee Voting

A Models and Theoretical Analysis

We examine the behavior of fully rational committee members and investigate the key factors that affect political outcomes through committee decision-making. Voting behavior can then be analyzed through a pivotal voting model. Consider a committee in which there are N individuals deciding between two alternatives, $w \in \{\mathbf{1}, \mathbf{2}\}$, who make a collective decision by holding a vote. There are two equally likely states of the world, $\omega \in \{\mathbf{1}, \mathbf{2}\}$, and committee members receive a payoff of $\overline{u}$ if alternative $w = \omega$, and 0 otherwise. Thus, the goal of the committee is to collectively choose an alternative that matches the state of the world.

First, nature randomly selects $\omega = \mathbf{1}$ or $\omega = \mathbf{2}$ as the state of the world with equal common prior probability for each, $\pi = \pi_1 = \pi_2 = 0.5$. While π is common knowledge, the realization of the state is unknown to the committee. Each committee member can independently exert costly effort to acquire information about ω. An individual either obtains a private signal or not. If an individual obtains a private signal, it informs the individual about ω with a likelihood of p. As a result, even if ω is the same for every member of the committee, the informed committee members may still have different understanding about ω because of the moderate uncertainty inherent in the private signal.

Whether a committee member i is informed of ω depends on two factors: (1) the effort $e_i \in \mathbb{R}_{\geq 0}$ exerted by the individual during the information acquisition process and (2) the level of difficulty in obtaining information. For simplicity, we introduce an information cost variable c_i, representing the difficulty of acquiring information. This cost c_i is independently and equally likely drawn from a uniform distribution on the interval $[\underline{u}, \overline{u}]$. If $e_i \geq c_i$, then individual i acquires a private signal about ω and incurs a cost of c_i. Conversely, if $e_i < c_i$, the individual remains uninformed about ω and does not incur any cost c_i.

Following the realization of private information, everyone in the committee, informed or not, independently and simultaneously votes at no cost for **1** or for **2**. As a benchmark, in *Majority Voting* (hereafter, MV), the alternative with the majority of votes is the committee's choice. The timing of the *Majority Voting* game is as follows:

1. The state ω and private information costs are independently drawn.
2. Each committee member decide $e_i \in \mathbb{R}_{\geq 0}$.
3. Each committee member, whether informed ($e_i \geq c_i$) or uninformed ($e_i < c_i$), decide $w_i \in \{\mathbf{1}, \mathbf{2}\}$.
4. The collective decision w_g is reached and payoffs are received.

We define the *quality of committee voting* as the likelihood that the committee collectively chooses the policy that matches the state of the world

$$Q = \Pr(w_g = 1 | \omega = 1) = \Pr(w_g = 2 | \omega = 2).$$

We begin our analysis by examining how the level of information expertise among committee members affects the voting committee's collective choice.

Our analysis focuses on the following equilibrium strategies: Informed committee members will follow their private signal to vote; uninformed committee members will randomly and equally likely vote for alternative **1** or **2**. There are two reasons for us to perform this analysis. First, voting according to one's signal is a weakly dominant strategy because in pivotal voting the expected benefits of voting following one's signal outweigh the expected benefits of voting against one's signal. Second, because the common prior is $\pi = \pi_1 = \pi_2 = 0.5$, uninformed members do not have incentives to favor either alternative with a likelihood of more than 50%.

When we normalize e_i and c_i, and because c_i is randomly and independently drawn from a uniform distribution, an individual's effort for information maps into a function that decides the likelihood $\rho \in [0, 1]$ that the individual receives the private signal: $\rho(e) = e$. The quality of committee voting, then, can be written as

$$Q(\rho) = \sum_{\theta=0}^{N} \binom{N}{\theta} \rho^{\theta}(1-\rho)^{N-\theta} r_{\gamma}(\theta) \tag{1}$$

, where θ indicates the number of informed members and r_γ is likelihood of correct state identification conditional on θ and the voting rule of political processes γ. Specifically,

$$r_{\gamma}(\theta) = \Pr(w = \omega|\theta, \gamma) = \sum_{\alpha=0}^{\theta} p^{\alpha}(1-p)^{\theta-\alpha} \sum_{\beta=0}^{N-\theta} \pi^{\beta}(1-\pi)^{N-\theta-\beta} \tag{2}$$

, where α indicates how many informed members voted correctly and β denotes how many uninformed members voted correctly such that $\alpha + \beta \geq \frac{N+1}{2}$. At given parameterizations of θ and γ, the value of $r_\gamma(\theta)$ is fixed. Therefore:

Lemma 1 *The quality of committee voting is strictly increasing in the level of information expertise among committee members, determined by the level of effort exerted in the information acquisition process.*

Proof: Notice that $Q(\rho)$ depends on the cumulative distribution function of a binomial random variable with parameter $\rho(e)$. Applying the definition of first-order stochastic dominance to expression (1) implies that $Q(\rho)$ is strictly increasing in the effort e that individuals exert to acquire costly information about ω. ■

A.1 Benchmark: Symmetric BNE Predictions

As a benchmark for our analysis, we calculate the point predictions for the equilibrium behavior. We focus on the symmetric strategy in which voters with the same information and tradeoff follow the same equilibrium strategy. Based on a pivotal voting model, each individual member is motivated by her assessment of the likelihood that she will be pivotal. In general, a member must consider how many other members have acquired information and voted for the alternative that matches the true state of the world. The symmetric best-response for a committee member is characterized by the effort for information e^* that solves

$$Piv(\rho, r_\gamma, e^*) = \rho(e^*). \tag{3}$$

The left-hand side is the probability an individual committee member is pivotal in voting, and the right-hand side is the function of the private cost of information acquisition that decides the likelihood that an individual is informed. Denote a solution to equation (3) by $e^*(\rho, r_\gamma)$, which gives the vector of symmetric Bayesian Nash equilibrium information acquisition cutoffs. Let m be the random variable representing an individual committee member's conjecture regarding the number of *other* members who acquired information. In pivotal events, if the member does not acquire information, the likelihood that the member will vote for the matching alternative is $\pi = \frac{1}{2}$ such that the committee collectively chooses the matching alternative with probability $\pi = \frac{1}{2}$. If the member acquires information, the Bayesian updated likelihood of voting for the matching alternative is

$$B(p|\alpha, m) = \frac{p^\alpha(1-p)^{m+1-\alpha}}{p^\alpha(1-p)^{m+1-\alpha} + p^{m+1-\alpha}(1-p)^\alpha}.$$

Let $\overline{N} = N - 1$. Solving equation (3) becomes solving the following equation:

$$\sum_{m=0}^{\overline{N}} \binom{\overline{N}}{m} \rho^m (1-\rho)^{\overline{N}-m} \sum_{\alpha=0}^{m} \binom{m}{\alpha} p^\alpha (1-p)^{m-\alpha} \binom{\overline{N}-m}{\beta} \pi^{\overline{N}-m} \left[B(\cdot) - \frac{1}{2}\right] = \rho(e^*). \tag{4}$$

In our experiment $N = 5$, the private signal reveals the true state of the world $p = 0.9$ of the time and wrong state of the world $1 - p = 0.1$ of the time. We solve equation (4) with our parameterizations and get $e^* = 23.53$ and $\rho(e^*) = 0.24$.

A.2 Informed Representation

Drawing from Lemma 1 as our foundational premise, we deduce that for committee decision-making to consistently yield favorable outcomes, a pivotal prerequisite is that $\theta > \frac{N}{2}$, signifying that the majority of the committee should comprise informed members. This serves as the theoretical underpinning of *Informed Representation*, as elaborated upon in the main text. *All else equal*, it can be anticipated that in a committee where informed individuals constitute the majority, there will be a heightened average level of informational expertise compared to one where informed members are the minority.

Proposition 1 (Informed Majority) *Under Majority Voting, the committee in which the number of informed individuals θ_1 satisfies $\theta_1 > \frac{N}{2}$ consistently identify ω with a higher likelihood than the committee in which the number of informed individuals θ_2 satisfies $\theta_2 < \frac{N}{2}$.*

Proof: When $\theta_1 > \frac{N}{2}, \theta_1 > N - \theta_1$, the likelihood of correct state identification is

$$r_{MV}(\theta_1) = \Pr(w = \omega|\theta_1) = \sum_{\alpha=0}^{\theta_1} p^\alpha (1-p)^{\theta_1-\alpha} \sum_{\beta=0}^{N-\theta_1} \pi^\beta (1-\pi)^{N-\theta_1-\beta}.$$

When $\theta_2 < \frac{N}{2}, \theta_2 < N - \theta_2$, the likelihood of correct state identification is

$$r_{MV}(\theta_2) = \Pr(w = \omega|\theta_2) = \sum_{\alpha=0}^{\theta_2} p^\alpha (1-p)^{\theta_2-\alpha} \sum_{\beta=0}^{N-\theta_2} \pi^\beta (1-\pi)^{N-\theta_2-\beta}.$$

Since $\theta_1 > \theta_2$, $p > 0.5$, and $\pi = 0.5$, $r_{MV}(\theta_1) > r_{MV}(\theta_2)$. ■

Based on Lemma 1 and Proposition 1, we introduce gender differences in the willingness to exert effort for information expertise and simulate the relationship between the quality of committee voting, effort for information expertise, and the level of informedness. We assume that women will exert more effort than men, $e^{women} = \delta e^{men}$, where $\delta > 1$, such that women are more likely to be informed than men. We consider **4M1W** (4 men 1 woman) and **2M3W** (2 men and 3 women), representing situations where women constitute the minimum minority and the majority, respectively. The simulated results are presented in Figure A1. As anticipated, a voting committee in which women are the majority is more likely to collectively and correctly identify ω than a voting committee in which women are the minority.

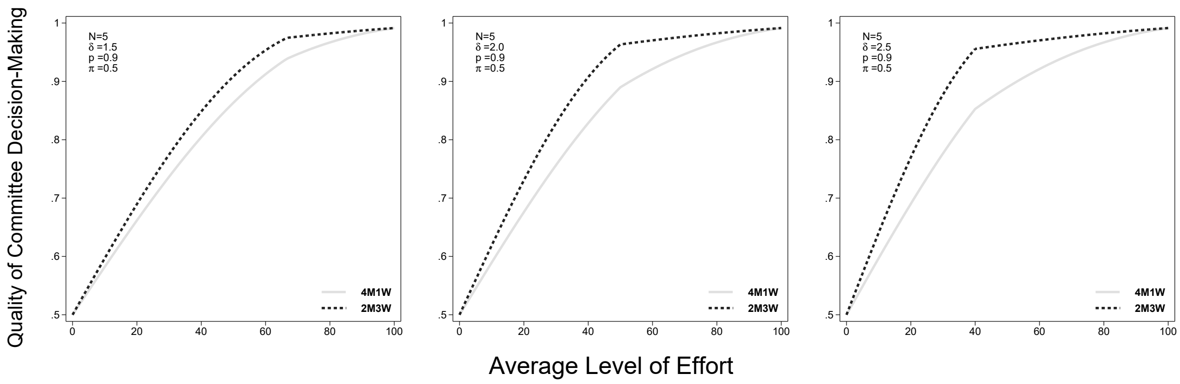

Figure A1: Effects of Informed Representation

A.3 Informed Authority

We now consider *Majority Voting with Veto* (hereafter, MVV) in which someone from the committee has veto power (i.e., the veto player) and the committee's choice is determined by the majority's choices and whether veto is used. The timing of the *Majority Voting with Veto* game is as follows:

1. The state ω and private information costs are independently drawn.
2. Each committee member, including the veto player, must decide $e_i \in \mathbb{R}_{\geq 0}$.
3. Each committee member, whether informed ($e_i \geq c_i$) or uninformed ($e_i < c_i$), decide $w_i \in \{\mathbf{1}, \mathbf{2}\}$.
4. The veto player is told the w_g and decides whether to veto.
5. The collective decision is reached and payoffs are received. Veto changes the committee's decision only when the majority's votes are for alternative **1**.

Let $r_{MVV}(v = 1)$ denote the probability of correct state identification under *Majority Voting with Veto* when the veto player is informed, and $r_{MVV}(v = 0)$ denote the probability when the veto player is uninformed, where $v \in \{0, 1\}$ represents whether the veto player is informed or not.

Proposition 2 (Informed Authority) $r_{MVV}(v = 1) > r_{MVV}(v = 0)$.

Proof: Case 1: If $v = 1$, the veto player possesses information about ω. This means they can make an informed decision about whether to use the veto based on their private information. If the majority's votes are correct, the veto player does not need to do anything, ensuring that the correct decision is made.
Case 2: If $v = 1$ and the majority's votes are incorrect, the veto player can veto, preventing an incorrect decision from being made.
Case 3: If $v = 0$, the veto player lacks information about ω. In this case, they do not have the ability to make an informed decision about whether to use the veto, and they should not use veto but count on the majority's choices, because uninformed veto is (weakly) dominated. Taken together, having an uninformed veto player provides no advantage in terms of making the correct decision. This concludes the proof. ■

Based on Proposition 2, we introduce gender differences in the veto players' willingness to exert effort for information expertise and simulate the relationship between the quality of committee voting and the level of informedness. We assume that women veto players will exert more effort than regular men committee members, $e^{women}_{v=1} = \delta e^{men}_{v=0}$, where $\delta > 1$, such that women veto players are more likely to be informed than regular men committee members. We also assume that men veto players will exert more effort than regular men committee members, $e^{men}_{v=1} = \zeta e^{men}_{v=0}$, where $\zeta > 1$, such that men veto players are more likely to be informed than regular men committee members. We consider **MVeto-4M1W** (4 men 1 woman, the veto player is a man) and **WVeto-4M1W** (4 men and 1 women, the veto player is a woman). The simulated results are presented in Figure A2. As anticipated, *all else equal* and when there are four men and one woman on the committee, a committee in which a woman serves as the veto player is more likely to identify ω than a committee in which a man serves this role.

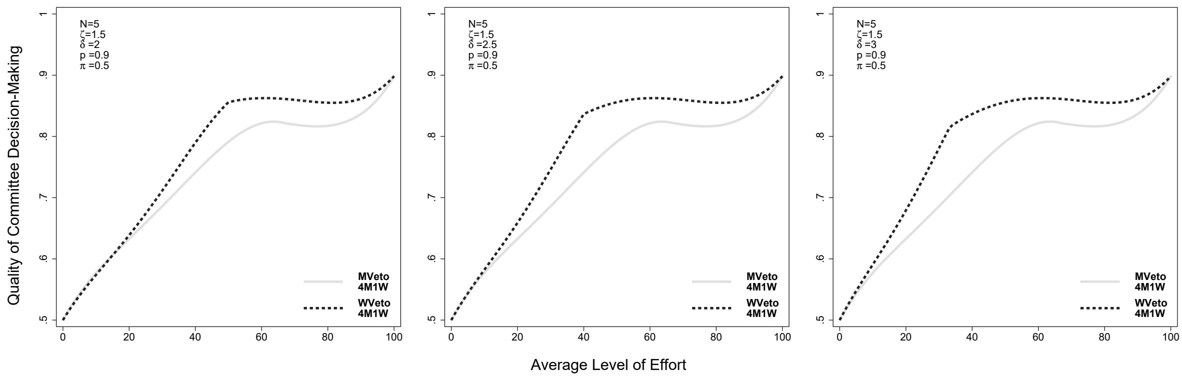

Figure A2: Effects of the Critical Role Mechanism

B Experimental Results

B.1 Non-parametric Analysis

We report the parametric and regression-based analysis in the main text. We also conducted arguably more conservative non-parametric tests to examine the robustness of these findings. Fréchette (2012) discusses the advantages and disadvantages of both approaches. Reporting both shows that our main results are robust to the statistical approach.

In the regressions, the data analysis is always clustered at the committee level. The clustering is used to account for arbitrary correlation between observations within a given committee. In the non-parametric analysis, to approach the clustering analysis used in the regressions, following Fréchette (2012), we use committee averages as the unit of analysis.

For the comparisons between experimental conditions, we use the non-parametric Fisher-Pitman permutation tests, which rely on fewer and weaker assumptions and have the highest power (100%) compared to related tests (Siegel 1957). Using a Monte-Carlo study, Moir (1998) shows the Fisher-Pitman permutation tests have statistically reliable power for as few as eight observations per treatment category. For the comparisons of matched data, we use the non-parametric signed-rank tests. We have 16 committees in Experiment I (eight committees for each experimental condition) and 16 committees in Experiment II (eight committees for each experimental condition).

Table A1 replicates the key findings from the paper except using non-parametric tests on committee averages. This leads to the same main conclusions as those drawn from its counterpart in the paper.

Table A1: The Quality of Committee Voting – Non-Parametric Tests

	Comparisons	Effect	$p-$value
Exp. I	**2M3W – 4M1W**	↗	0.044
Exp. II	**WVeto-4M1W – MVeto-4M1W**	↗	0.002
	WVeto-4M1W† – MVeto-4M1W†	n.s.	0.162†
	WVeto-4M1W – WVeto-4M1W†	↗	0.025
	MVeto-4M1W – MVeto-4M1W†	n.s.	0.778
Exp. II vs Exp. I	**WVeto-4M1W – 4M1W**	↗	0.003
	MVeto-4M1W – 4M1W	n.s.	0.768

Notes: The p-values are the results of two-tailed tests.
† If we estimate committee outcomes based on individual members' vote before veto is used, then with the same set of participants, the quality of committee voting is about 0.650 in MVeto-4M1W and 0.738 in WVeto-4M1W, which is not significantly different.

B.2 Comparisons of Behavior Across Experiments

In Table A2, we report information acquisition behavior by gender and committee role.

Table A2: Committee Members' Effort by Experiment

	Dependent Variable: Effort for Information			
	Regular Members		Veto Players	
	Men	Women	Men	Women
Variables	(1)	(2)	(3)	(4)
Experiment II	-0.654	-12.120		
	(6.038)	(11.456)		
Experiment III		4.180	-14.833	-14.733
		(9.537)	(9.320)	(10.454)
Constant	20.614***	50.183***	45.294***	85.144***
	(3.880)	(5.789)	(7.806)	(9.345)
Exp. II vs *Exp. III*		-16.300		
		(12.456)		
Number of participants	104	52	32	32
Clustered S.E.	Yes	Yes	Yes	Yes

Notes: OLS specification. *Experiment II* is a binary indicator that represents whether subjects participated in Experiment II. *Experiment III* is a binary indicator that represents whether subjects participated in Experiment III. Columns 1 and 2 include observations of regular committee members' choices. Columns 3 and 4 report only the results of veto players who participated in Experiments II and III. Robust standard errors are clustered at the committee level and reported in parentheses.
***, **, and * denote significance at the 1%, 5%, and 10% levels. *Takeaway*: The information acquisition behavior of regular committee members and veto players is statistically similar across experiments.

B.3 Post-Experiment Survey

Before revealing participants' earnings or the results from the three parts of the experiment, participants were asked to answer a short post-experiment survey. Based on the results reported in Table A3, we find that information acquisition was significantly more important to women than men. Veto players express that information acquisition is significantly more important to them than regular committee members. These survey results are consistent with the main findings that both for men and women, veto players exert significantly more effort to acquire information than regular committee members. We also find that in the process of making information acquisition and voting decisions, women care more about the group's total benefits than men. Moreover, veto players express that they feel their vote is pivotal to the group outcome at a significantly higher level than regular committee members. These survey results are consistent with the main findings that both for men and women, veto players exert significantly more effort to acquire information than regular committee members.

Table A3: Summary Statistics of Post-Experiment Survey

	Pooled			Women			Men		
	Men	Women	$p-$value	Regular Member	Veto Player	$p-$value	Regular Member	Veto Player	$p-$value
Information	4.257	5.964	$p_t < 0.001$ $p_{FP} < 0.001$ $p_{KS} < 0.001$	5.519	6.688	$p_t = 0.015$ $p_{FP} = 0.029$ $p_{KS} = 0.087$	3.904	5.406	$p_t < 0.001$ $p_{FP} = 0.002$ $p_{KS} < 0.001$
Welfare	4.522	6.524	$p_t < 0.001$ $p_{FP} < 0.001$ $p_{KS} < 0.001$	6.481	6.594	$p_t = 0.804$ $p_{FP} = 0.830$ $p_{KS} = 0.984$	4.413	4.875	$p_t = 0.332$ $p_{FP} = 0.350$ $p_{KS} = 0.511$
Pivotality	4.868	6.143	$p_t < 0.001$ $p_{FP} < 0.001$ $p_{KS} < 0.001$	5.635	6.969	$p_t = 0.002$ $p_{FP} = 0.002$ $p_{KS} = 0.024$	4.548	5.906	$p_t = 0.002$ $p_{FP} = 0.001$ $p_{KS} = 0.022$

Notes: We report the mean value of individuals' responses by gender and role in the committee.
Information: "How important was it to you to acquire information?" This question was measured based on a scale of 1-9, 1 being not important at all, 5 being neutral, and 9 being extremely important.
Welfare: "When you make information acquisition and voting decisions, to what extent you think about your own benefits and to what extent you think about the group's total benefits?" This question was also measured based on a scale of 1-9, 1 being only thinking about own benefits and not thinking about group's total benefits at all, 5 being thinking about own benefits and group's total benefits equally, and 9 being only thinking about group's total benefits and not thinking about own benefits at all.
Pivotality: "To what extent do you think your vote affects the voting outcomes?" This question was again measured based on a scale of 1-9, 1 being thinking the vote does not affect the outcomes at all, 5 being neutral, and 9 being thinking the vote is decisive and pivotal.
We perform a regression to investigate the difference of means. Individual-level observations are nested within committees, so we cluster robust standard errors by committee, and the corresponding p-value is denoted by p_t. We also use a Fisher-Pitman permutation test, an analogous non-parametric test, to examine the difference of means, and the corresponding p-value is denoted by p_{FP}. We conduct a Kolmogorov-Smirnov test, an non-parametric test, to check the distribution of individuals' responses, and the corresponding p-value is denoted by p_{KS}.
Takeaway: Information acquisition was significantly more important to women than men. Veto players express that information acquisition is significantly more important to them than regular committee members. In the process of making information acquisition and voting decisions, women care more about the group's total benefits than men. Moreover, veto players express that they feel their vote is pivotal to the group outcome at a significantly higher level than regular committee members. Our results are robust to different statistical methods.

B.4 Gender Differences in Effort for Information

Individual-level observations are nested within committees, so we report cluster robust standard errors. Cell entries are ordinary least squares regression coefficients; cluster-robust standard errors in parentheses (clustered by committee). Even after we have controlled for the influence caused by demographic factors, as reported in Tables A4, our findings are consistent and qualitatively identical.

B.5 Determinants of Individuals' Effort for Information

In Table A5, we report regression results of factors that affect individuals' effort decisions.

Table A4: Gender Differences in Effort for Information Expertise

	Dependent Variable: Effort for Expertise								
	Experiment I		Experiment II			Experiment III		Pooled	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Women	29.569***	31.689***	38.477***	18.103	22.810*			29.021***	29.576***
	(6.255)	(6.709)	(9.750)	(10.946)	(11.898)			(4.906)	(4.857)
VetoPlayer				25.334**	25.764**			13.907**	14.285***
				(9.409)	(9.992)			(5.414)	(5.381)
Women×*VetoPlayer*				21.747	25.266			10.904	12.958
				(18.115)	(18.045)			(8.105)	(8.267)
Constant	20.614***	5.037	23.127***	19.960***	-14.414			20.262***	-4.214
	(3.945)	(22.631)	(4.164)	(4.706)	(28.417)			(2.034)	(17.401)
Women veto players versus Men veto players				39.850***	48.076***	39.950***	39.892***	39.925***	42.534***
				(12.380)	(11.347)	(6.868)	(6.868)	(6.165)	(6.218)
Women veto players versus Women regular members				47.081***	51.031***			24.811***	27.243***
				(13.842)	(15.146)			(6.094)	(6.141)
Men veto players versus Men regular members				25.334**	25.764**			13.907**	14.285***
				(9.409)	(9.992)			(5.414)	(5.381)
Number of participants	80	80	80	80	80	60	60	220	220
R-squared	0.198	0.303	0.176	0.300	0.404	0.228	0.328	0.277	0.330
w/ Control Variables	No	Yes	No	No	Yes	No	Yes	No	Yes
Clustered S.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS specification. Robust standard errors are clustered at the committee level and reported in parentheses.
***, **, and * denote significance at the 1%, 5%, and 10% levels.
Control variables include *Age*, *Academic Major*, *Ethnicity*, *GPA*, and *Cognitive Ability* (measured by the Cognitive Reflection Task, Frederick 2005. Participants were asked to respond to the following three non-incentivized questions with no time limit in the post-experiment survey: (1) A bat and a ball cost $1.10 in total. The bat costs a dollar more than the ball. How much does the ball cost? [Answer: 5 cents] (2) If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? [Answer: 5 minutes] (3) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake? [Answer: 47 days]). The default major is *Natural Science*. The default race is *White*. None of our participants reported her/him/them as *Native American* or *American Indian*.

Table A5: Determinants of Individuals' Effort for Information

	Dependent Variable: Effort for Information													
	Regular Committee Members							Veto Players						
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Collaboration	0.657***					0.495***	0.408***	0.806***					0.564***	0.387***
	(0.130)					(0.127)	(0.115)	(0.142)					(0.139)	(0.112)
Risk		-0.318***				-0.213***	-0.181***		-0.259**				0.085	0.035
		(0.063)				(0.062)	(0.057)		(0.109)				(0.096)	(0.117)
Information			5.122***			3.491***	3.449***			9.904***			5.116**	5.635***
			(0.822)			(0.742)	(0.741)			(1.657)			(2.375)	(1.642)
Welfare				3.142***		-0.511	-1.179				7.445***		1.912	1.250
				(1.150)		(1.053)	(1.084)				(1.518)		(1.650)	(1.636)
Pivotality					4.557***	0.168	0.214					6.064***	0.456	-0.051
					(1.002)	(1.067)	(0.999)					(1.683)	(1.745)	(1.578)
# participants	156	156	156	156	156	156	156	64	64	64	64	64	64	64
R-squared	0.168	0.094	0.160	0.051	0.065	0.292	0.403	0.268	0.046	0.226	0.175	0.097	0.372	0.471
Controls	No	No	No	No	No	No	Yes	No	No	No	No	No	No	Yes
Clustered S.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS specification. Robust standard errors are clustered at the committee level and reported in parentheses.
Collaboration preference is measured by individuals' contribution to an incentivized threshold public good. *Risk* preference is measured by individuals' investment to an incentivized risky option. *Information* Importance is measured by individuals' response (on a scale of 1-9) to the survey question "How important was it to you to acquire information?" *Welfare* consideration is measured by individuals' response (on a scale of 1-9) to the survey question "When you make information acquisition and voting decisions, to what extent you think about your own benefits and to what extent you think about the group's total benefits?" *Pivotality* is measured by individuals' response (on a scale of 1-9) to the survey question "To what extent do you think your vote affects the voting outcomes?"
Control variables include *Gender*, *Age*, *Academic Major*, *Ethnicity*, *GPA*, and *Cognitive Ability* (measured by the Cognitive Reflection Task, Frederick 2005. Participants were asked to respond to the following three non-incentivized questions with no time limit in the post-experiment survey: (1) A bat and a ball cost $1.10 in total. The bat costs a dollar more than the ball. How much does the ball cost? [Answer: 5 cents] (2) If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? [Answer: 5 minutes] (3) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake? [Answer: 47 days]). The default major is *Natural Science*. The default race is *White*. None of our participants reported her/him/them as *Native American* or *American Indian*.
***, **, and * denote significance at the 1%, 5%, and 10% levels.

B.6 Individual Behavior

In Table A6, we focus on the behavioral analysis of how individuals make voting decisions conditional on whether they are informed, whether they are regular committee members or veto players, and whether women or men hold the veto power. In Table A7, we study aggregate gender differences in using vetoes. In Table A8, we further break down the analysis of veto players' use of veto by their own choice, the majority's choice, and whether they are informed. In Table A9, we explore the determinants of veto players' uninformed veto.

Table A6: The Likelihood of Voting for Jar 1 by Informedness, Role, and Gender

	Dependent Variable: Pr(Voting Jar 1)					
	Regular Member	Veto Player	p-value	Men	Women	p-value
Informed $\omega = 1$	0.906	0.906	n.s.	0.931	0.890	n.s.
Informed $\omega = 2$	0.075	0.081	n.s.	0.107	0.062	n.s.
Uninformed	0.565	0.593	n.s.	0.581	0.541	n.s.

Takeaway: Individuals in general follow their acquired information to vote when informed. When they are uninformed, they vote for Jar 1 with a likelihood that is statistically identical to 50%. There is no significant difference in this behavioral pattern between regular committee members and veto players, or between men and women.

Table A7: Aggregate Level Gender Differences in Using Veto Power

	Dependent Variable: Veto = 1		
Variables	(1)	(2)	(3)
Women	-0.089**	-0.101*	
	(0.042)	(0.057)	
Uninformed		-0.097**	
		(0.047)	
Women× *Uninformed*		-0.112*	
		(0.058)	
Constant	0.255***	0.322***	0.286***
	(0.032)	(0.045)	(0.087)
Pr(Veto|Uninformed), women relative to men		-0.213***	
		(0.034)	
Pr(Veto|Informed $\omega = 1$), women relative to men			-0.197**
			(0.093)
Number of participants	64	64	64
Clustered S.E.	Yes	Yes	Yes

Notes: LPM specification. We use linear probability models to report the results of regression based analysis because it facilitates the presentation of linear combined effects as shown in this table. In Column 3, we narrow down our investigation into the cases where they are informed that the state of the world is Jar 1 so that their veto can not change the majority's vote. Robust standard errors are clustered at the committee level and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels. *Takeaway*: Women use veto power almost only when they are informed. Men use veto power even if they are uninformed. Even if the use of veto is not consequential, men are significantly more likely to use the power anyway.

Table A8: The Likelihood of Using Veto by Own Choice and Majority's Choice

	Majority Choice $w_g = 1$		Majority Choice $w_g = 2$	
	Men	Women	Men	Women
Informed $\omega = 1$, own choice $w_i = 1$	0.04 (54)	0.08 (135)	0.75 (28)	0.00 (44)
Informed $\omega = 1$, own choice $w_i = 2$	1.00 (1)	0.54 (13)	0.00 (1)	0.00 (12)
Informed $\omega = 2$, own choice $w_i = 1$	0.86 (7)	0.56 (16)	0.00 (5)	0.00 (3)
Informed $\omega = 2$, own choice $w_i = 2$	0.79 (29)	0.90 (72)	0.15 (74)	0.07 (176)
Uninformed, own choice $w_i = 1$	0.18 (204)	0.03 (59)	0.30 (64)	0.00 (35)
Uninformed, own choice $w_i = 2$	0.47 (55)	0.00 (23)	0.15 (118)	0.00 (52)

Note: Number of observations are given in parentheses. *Takeaway*: All else equal, men in general are more likely to use veto regardless of whether the veto decision is constructive.

Table A9: Determinants of Individuals' Uninformed Veto

	Dependent Variable: Uninformed Veto = 1							
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Collaboration	-0.002***						-0.002***	-0.001*
	(0.000)						(0.000)	(0.000)
Risk		0.001***					0.001***	0.000
		(0.000)					(0.000)	(0.000)
Information			-0.014				-0.003	-0.005
			(0.009)				(0.008)	(0.004)
Welfare				-0.011			0.000	0.005
				(0.007)			(0.006)	(0.003)
Pivotality					-0.007		0.004	0.004
					(0.007)		(0.005)	(0.003)
$w_i \neq w_g$						0.053**	0.043**	0.030***
						(0.024)	(0.018)	(0.011)
# participants	64	64	64	64	64	64	64	64
log-likelihood	-324.9	-333.9	-347.2	-348.2	-351.5	-347.5	-307.1	-267.6
Controls	No	No	No	No	No	No	No	Yes
Clustered S.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Probit specification. Each column reports average marginal effects from Probit regressions. *Collaboration* preference is measured by individuals' contribution to an incentivized threshold public good. *Risk* preference is measured by individuals' investment to an incentivized risky option. *Information* Importance is measured by individuals' response (on a scale of 1-9) to the survey question "How important was it to you to acquire information?" *Welfare* consideration is measured by individuals' response (on a scale of 1-9) to the survey question "When you make information acquisition and voting decisions, to what extent you think about your own benefits and to what extent you think about the group's total benefits?" *Pivotality* is measured by individuals' response (on a scale of 1-9) to the survey question "To what extent do you think your vote affects the voting outcomes?" $w_i \neq w_g = 1$ is a binary indicator that represents whether a veto player's own choice differs from the majority's choice.
Control variables include *Gender*, *Age*, *Academic Major*, *Ethnicity*, *GPA*, and *Cognitive Ability* (measured by the Cognitive Reflection Task, Frederick 2005. Participants were asked to respond to the following three non-incentivized questions with no time limit in the post-experiment survey: (1) A bat and a ball cost $1.10 in total. The bat costs a dollar more than the ball. How much does the ball cost? [Answer: 5 cents] (2) If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? [Answer: 5 minutes] (3) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake? [Answer: 47 days]). The default major is *Natural Science.* The default race is *White.* None of our participants reported her/him/them as *Native American* or *American Indian.*
Robust standard errors are clustered at the committee level and reported in parentheses.
***, **, and * denote significance at the 1%, 5%, and 10% levels.

C Replications and Robustness Check

A critical feature of our experimental design is that we elicit how much effort a decision maker would like to exert for expertise. After a participant reports the willingness to pay for information, the computer randomly generates a private cost of information. If this cost falls below the participant's effort, the participant is not informed of the state of the world. Since whether a participant is informed affects the voting choice, our experimental design implies that the outcome of our experiment, both for men and women, depends on the actual realization of the information cost. A concern might be whether the information costs drawn at random by the computer produced our treatment effects, and thus, our results are due to chance rather than a real effect. Importantly, this is the case in any empirical study.

In this section, we conduct a robustness check and examine to what extent we can replicate the main results with simulations. Specifically, we can simulate a counterfactual experiment where the effort for expertise reported by each experimental subject remains the same, but where the randomly generated information costs are different. To achieve this, we assume that participants will vote for Jar **1** (Jar **2**) when they are informed that the state of the world is Jar **1** (Jar **2**), and equally likely and randomly vote for Jar **1** or Jar **2** when they fail to acquire expertise. This assumption is not arbitrary, but highly consistent with our experimental observations. Based on this decision rule, we conduct a Monte Carlo simulation by redrawing the information cost for each round following the same algorithm used to produce the actual information costs in the experiment.

We use participants' decisions on how much effort they would like to exert for expertise that we observed in the experiments, and this information directly decides the likelihood that they acquired expertise, and therefore, the likelihood of the committee collectively chooses the matching policy. Notice that participants did not receive any feedback regarding the randomly generated information costs in the experiment, and thus, the information costs, whether they are generated in the experiment or after the experiment, do not affect participants' decisions to acquire expertise or what to vote for if they are (un)informed. In other words, our experimental design allows us to conduct this replication and robustness check while keeping the conclusions as valid as the conclusions derived from the information costs generated in the experiment. We then compare our estimates of the treatment groups, for the likelihood that the matching policy is collectively chosen by the committee. We run this simulation 1000 times and average over these 1000 simulations.

C.1 Informed Representation

The comparisons of the quality of committee voting between **4M1W** and **2M3W** are illustrated in Figure A3. The estimate is 0.648 in **4M1W** and within the 95% confidence interval of the experimental observation at 0.613. The estimate is 0.7995 in **2M3W** and almost identical to the experimental observation at 0.800. The difference between the simulated results and the experimental observations is about 0.035 in **4M1W** and less than 0.001 in **2M3W,** respectively. None of these differences are significantly different from zero.

Importantly, the effects of *Informed Representation* are robust. Comparing the simulated means, we find that the quality of committee voting is significantly higher in **2M3W** than in **4M1W** at the 0.001 level, which is consistent with the results reported in the main text. These results suggest that the identified effects of *Informed Representation* are not likely to be driven by stochastic factors.

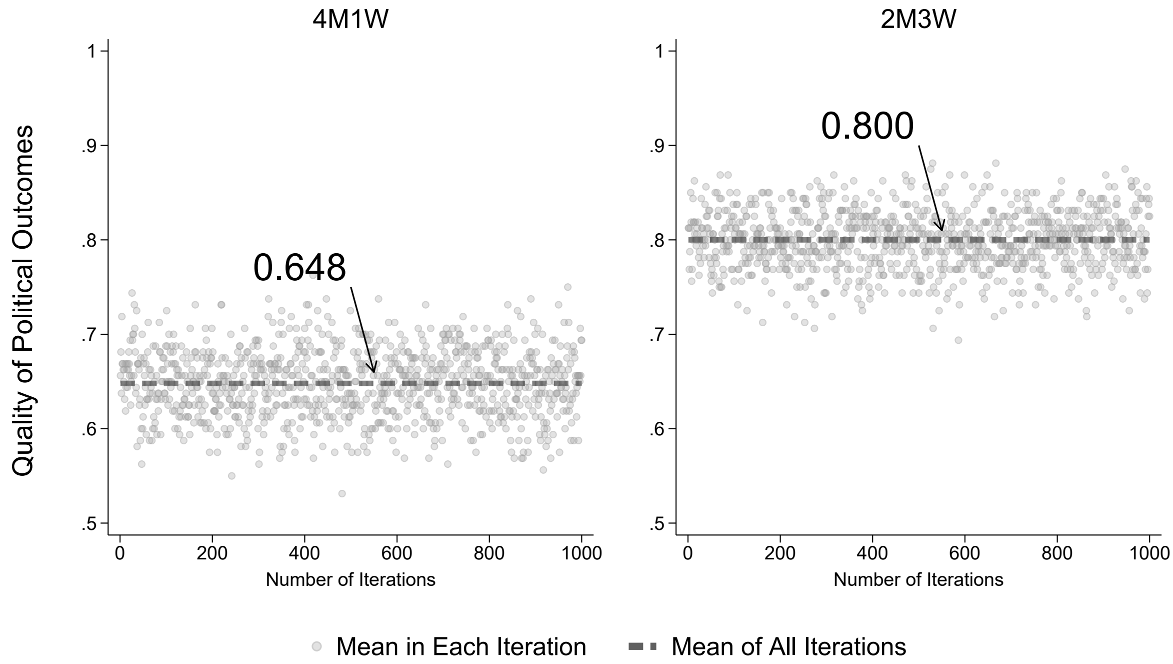

Figure A3: Replications of the Effects of Informed Representation

C.2 Informed Authority

We now turn to replicate *Informed Authority.* Regular committee members received no feedback regarding veto players' decisions (i.e., what veto players vote for and whether they have used their veto), but veto players were informed of how many votes there are for Jar 1 and Jar 2 before making a veto decision. Importantly, our design implies that regular committee members' behavior is not influenced by veto players' behavior, but veto players' decisions may be affected by the choices of the regular committee members. Matching regular committee members with different veto players could therefore lead to different outcomes. We have examined and identified that regular committee members' behavior is identical between Experiment I and Experiment II. As a result, in Experiment II the focus of our analysis is on veto players' choices.

We keep the decision rule of information acquisition and voting the same as above so that the results of the experimental conditions with veto can be comparable to the experimental conditions without veto. However, because of gender differences in information acquisition and political power usage, we expect in theory and observe in experiments that whether women or men have veto power affects the political outcomes through collective decision-making. Importantly, women veto almost only when they are informed that the state of the world is Jar **2** while the majority's vote is Jar **1**, so their veto is consequential and highly likely to switch the outcome of the committee from low payoffs to high payoffs. But men veto even if they are uninformed or the veto is not consequential (i.e. the state of the world is Jar **1** but the majority's vote is Jar **2** or the majority has voted for Jar **2** regardless of the state of the world).

We incorporate the gender differences in using veto power into the simulation, run this simulation 1000 times, and average over these 1000 simulations. The comparisons of the quality of committee voting between **WVeto-4M1W** and **MVeto-4M1W** are illustrated in Figure A4. The treatment effect of women having veto power on the quality of committee voting continues to hold.

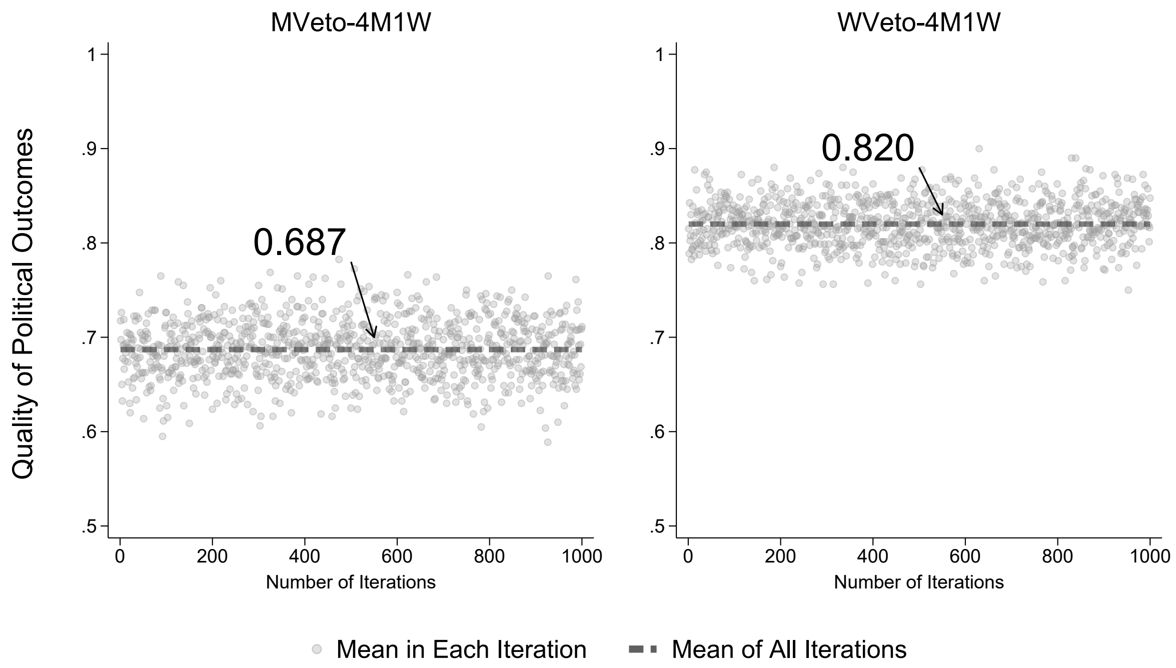

Figure A4: Replications of the Effects of Informed Authority

The estimate is 0.687 in **MVeto-4M1W** and within the 95% confidence interval of the experimental observation at 0.631. The estimate is 0.820 in **WVeto-4M1W** and within the 95% confidence interval of the experimental observations at 0.869. The difference between the simulated results and the experimental observations is about 0.056 in **MVeto-4M1W** and 0.049 in **WVeto-4M1W**, respectively. None of these differences are significantly different from zero.

Comparing the simulated means, we find that the quality of committee voting is significantly higher in **WVeto-4M1W** than in **MVeto-4M1W** at the 0.01 level, which is consistent with the results reported in the main text. These results suggest that the identified effects of how women improve political outcomes when they play a more critical role in the political process, such as having veto power, are not likely to be driven by stochastic factors but that they are more likely to make informed voting and veto the majority's uninformed or biased vote.

Finally, we perform the last thread of robustness checks by randomly matching regular committee members with different sets of veto players ($N = 64$) and estimating the average quality of committee voting for **WVeto-4M1W** and **MVeto-4M1W**, respectively. We are able to conduct these robustness checks because veto players and regular committee members made decisions under the same set of information and parameterizations in Experiment III as in Experiment II. Regular committee members receive no feedback regarding whether the others are informed or how the others voted.

We retrieve the vote choices of regular committee members participated in the previous 16 sessions (i.e., Experiment II) and use the same information as feedback for veto players in Experiment III. Thus, matching regular committee members with different veto players will not change subjects' choices but different veto players' behavior may affect the group decision and therefore the quality of committee voting.

To perform this analysis, we conduct a Monte Carlo simulation by redrawing the set of veto players and their corresponding decisions 1000 times. These results are reported in Figure A5. We find that on average the quality of committee decision-making is 0.600 in **MVeto-4M1W** but 0.819 in **WVeto-4M1W**, which is qualitatively identical to our main findings.

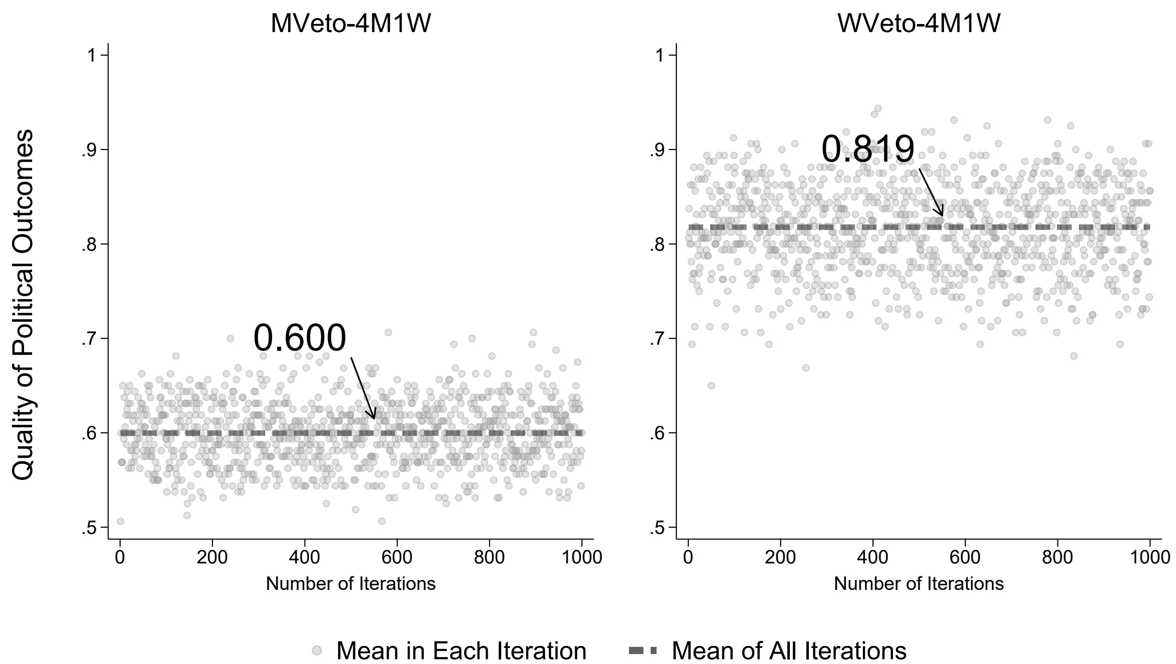

Figure A5: Robustness of Informed Authority's Effects with Rematched Veto Players

D Distribution of Treatment Effects

D.1 Estimation Strategy

Since we elicit individuals' willingness to pay for information when subjects do not yet know their information cost, we can generate and assign specific information costs (from the lowest to the highest) to decision-makers post-experiment using simulations. This enables us to estimate the distribution of treatment effects in a simple and straightforward fashion. Meanwhile, the distribution of treatment effects has the same claim to validity as the average effects reported in the main text, because the only difference is the realization of information cost, which was unobserved by subjects. In other words, the unobserved information cost will not change decision-makers decision rules. In estimating the distribution of effects by varying the information cost, we use a decision rule system that replicates the decision rules used by participants and follows the individual behavioral patterns observed in our experiments (as reported in Appendix B.6).

D.2 Informed Representation

In estimating the distribution of *Informed Representation*'s effects, we use the following decision rules.

Rule 1: When a subject was informed in a decision period and voted for a particular jar in the experiments, then when the subject is informed in the simulations, the subject will vote for that particular jar.

Rule 2: When a subject was informed in the experiments but uninformed in the simulations, the subject will vote for Jar 1 and Jar 2 with the same probability we observed individuals vote for each jar in the experiments.

Rule 3: When a subject was uninformed in the experiments and voted for a particular jar in experiments, then when the subject is uninformed in the simulations, the subject will vote for that particular jar.

Rule 4: When a subject was uninformed in the experiments but informed in the simulations, the subject will follow the signal and vote for the jar shown to them with the likelihood observed in the experiments for men and women.

D.3 Informed Authority

In estimating the distribution of *Informed Authority*'s effects, we use the decision rules for *Informed Representation* but with the following additional decision rules.

Rule 5a: When a veto player was informed in the experiments, the majority's collective choice was identical to the veto player's choice, and the veto player did use veto, then when the veto player is informed in the simulations, the veto player will veto the majority's collective choice when it is identical to the veto player's vote.

Rule 5b: When a veto player was informed in the experiments, the majority's collective choice was different from the veto player's choice, and the veto player did not use veto, then when the veto player is informed in the simulations, the veto player will not veto the majority's collective choice when it is different from the veto player's vote.

Rule 6a: When a veto player was uninformed in the experiments, the majority's collective choice was identical to the veto player's choice, and the veto player did use veto, then when the veto player is uninformed in the simulations, the veto player will veto the majority's collective choice when it is identical to the veto player's vote.

Rule 6b: When a veto player was uninformed in the experiments, the majority's collective choice was different from their choice, and the veto player did not use veto, then when the veto player is uninformed in the simulations, the veto player will not veto the majority's collective choice when it is different from the veto player's vote.

Rule 7: When a veto player was informed in the experiments but uninformed in the simulations, then in the simulations men veto players will veto the majority's choice with the likelihood observed in the experiments for men and women respectively.

Rule 8: When a veto player was uninformed in the experiments but informed in the simulations, then in the simulations men veto players will veto the majority's choice with the likelihood observed in the experiments for men and women respectively.

E Experimental Design and Methodological Matters

E.1 Ethics, Logistics and Procedures

This research was reviewed and deemed exempt from review by the [redacted] Institutional Review Board. The experiments were programmed using z-Tree (Fischbacher 2007) and conducted at a large U.S. university using z-Tree unleashed (Duch, Grossmann and Lauer 2020). Adult subjects (> 18 years old) were drawn from a database with about 2600 registered subjects operated by a standard experimental social science laboratory at the large university in the United States. Subjects are registered undergraduate students. A computer routine used a random process to send out e-mail announcements, and sign-ups occurred as voluntary responses to those announcements.

After participants gave their consent to participate in a session, the experimenters produced the unleaded z-Tree links and sent the links to the registered participants. When all participants were connected to the server over the links, the experimenters started the session. Participants remotely joined the session over Zoom in which everyone's identity was purposely blinded. The experiment instructions were read out loud by the same experimenter. The experimenters shared their screen with participants while reading the instructions so that participants could also read the instructions by themselves. If subjects had questions in the middle of the experiment, they could send a private text message to the experimenters over Zoom chat. The experimenters then answered their questions instantly and privately. Participants were paid over a widely used online payment platform immediately after a session was concluded.

In Experiment I, 80 subjects, 48 men and 32 women, were randomly assigned to experimental conditions of **4M1W** and **2M3W**. Each session consisted of one committee only. All these 80 subjects were regular committee members without veto power. In Experiment II, another 80 subjects, 64 men and 16 women, were randomly assigned to experimental conditions of **MVeto-4M1W** and **WVeto-4M1W**. Among men, 56 served as regular committee members, 8 served as veto players. Among women, eight served as regular committee members and eight served as veto players. As in Experiment I, committee members had no information regarding the gender composition of their committee or the session. Only participants with veto power knew whether a veto was used; the regular committee members received no feedback regarding how the others voted and were not told whether a veto was used. In Experiment III, another 60 subjects were recruited from the same subject pool used for Experiment I and Experiment II. They were randomly paired with decisions from previous simultaneous vote sessions. Within this group, 20% ($N = 12$) became regular committee members, while 80% ($N = 48$) became veto players. All the 12 regular committee members are women. Half of the veto players are women. These 60 participants were explicitly informed that they were matched with decisions from prior sessions, and no deceptive practices were employed. For these 60 participants, we applied the same random assignment procedure as in Experiment II, replicating identical variables such as information costs, the state of the world, and the voting choices of fellow committee members. Consequently, these 60 participants made decisions under the same information and parameterizations as the 80 subjects in Experiment II.

We designed our experiments and controlled the decision-relevant information that participants had, so that the decision task and information structure is identical for subjects who participated in different sessions. We find no evidence showing session effects; the behavior of subjects who serve the same role is statistically identical across experiments (as shown in Table A2). Our empirical strategy enables us to maintain the rigorous controls of laboratory experiments while examining the robustness of our findings to alternative explanations such as strategic effects. Moreover, it helps us achieve high data collection efficiency in conducting group-based experimental studies.

E.2 Comprehension Quiz

After we read the experiment instructions and before subjects made decisions in the experiment, subjects were asked to complete a comprehension quiz. There was no time limit for the comprehension quiz. Subjects were allowed to ask questions. Only when one answered all the comprehension quiz correctly, and when all subjects completed the comprehension quiz, subjects could start making formal decisions.

1. The composition of your group changes from round to round. Yes or No? [Answer: No]
2. In each round, there are five participants in your group (you and four other participants). Yes or No? [Answer: Yes]
3. At the beginning of each round, the computer randomly determines which Jar, Jar 1 or Jar 2, is assigned to your group. Each Jar is equally likely to be chosen. Yes or No? [Answer: Yes]
4. Because each Jar will be chosen with a 50% chance, if Jar 1 was chosen for your group in the last three rounds, does it mean that Jar 2 will be chosen for your group in this round (with a somewhat higher probability)? Yes or No? [Answer: No]
5. Suppose you decide to acquire information, your willingness to pay for the information is less than the randomly generated information fee, do you receive the information? Yes or No? [Answer: No]
6. Suppose you decide to acquire information, your willingness to pay for the information is higher than the randomly generated information fee. You will receive the information and the amount you pay is the information fee but not your willingness to pay. Yes or No? [Answer: Yes]
7. If you acquire and receive the information, you will receive correct information

about the Jar assigned to your group 90% of the time and incorrect information 10% of the time. Yes or No? [Answer: Yes]

8. Whether you acquire information or not, you can vote and your ballot affects your group's choice. Yes or No? [Answer: Yes]
9. If you voted for the right Jar but your group's decision is not the right Jar, your benefits will be the high payoff (i.e. 100 Points). Yes or No? [Answer: No]
10. If you voted for the wrong Jar but your group's decision is the right Jar, your benefits will be the low payoff (i.e. 0 Points). Yes or No? [Answer: No]

E.3 Experiment Instructions

Experiment instructions can be found at this (identity redacted) link. We report the experiment instructions for the majority voting as a sample. The instructions for the other experimental conditions are similar and available upon request. The experiment consists of three parts. The instructions for Part 2 were distributed to participants after Part 1 has been finished. The instructions for Part 3 were announced at the completion of Part 2. In each session, everyone received the same instructions. In all sessions, the instructions were read by the same experimenter. There was no feedback of each part's results and earnings until the end of the session. Participants were allowed to ask question at any time. If subjects had questions in the middle of the experiment, they could send a private text message to the experimenters over Zoom chat. The experimenters then answered their questions instantly and privately.

E.4 Interface and Screenshots

The screenshots of the computer interface that subjects used in the experiments can be found at this (identity redacted) link.

E.5 Pre-Registration

A pre-registration and pre-analysis plan for the study was filed with [redacted for peer review]. The screenshots of the pre-registration can be found at this (identity redacted) link. The analysis choices reported in the main text and our research hypotheses were pre-specified in the preregistration. There are no departures from the pre-analysis plan.

E.6 Sample Size and Power Analysis

One of the most critical components of experimental research design is sample size estimation. A prevalent concern in controlled laboratory experiments is the statistical power of the findings, particularly when compared to larger-scale studies, such as surveys, where sample sizes tend to be more substantial. We recognize the importance of addressing these concerns in our sample size calculations and power analyses. A sufficient sample size is essential for reliably detecting true differences and ensuring that the study is not underpowered. Additionally, researchers must be cautious to avoid excessive sample sizes, which can increase the risk of Type I errors and lead to wasted resources. As a result, our experiment design includes careful estimation of the minimum required but sufficient sample size needed to achieve the desired power for detecting scientifically meaningful differences while maintaining a fixed Type I error rate.

Our experimental research design involves committee voting experiments that feature fixed matching and a series of 20 voting periods with varying parameterizations, conducted without communication and with limited feedback. Participants are randomized into committees. All members within a committee receive the same intervention (including the voting rule, gender composition, and the stochastically generated state of the world and information cost). To accurately account for the intra-committee correlation—given that committee performance across the 20 voting periods within the same committee are likely to be correlated—we incorporate this consideration into both our sample size calculation and statistical analysis. Specifically, our sample size calculation involves two key tasks: (1) determining the number of committees required, and (2) establishing the number of participants needed within each committee. The outcomes of the committee voting are binary variables, while individual willingness to engage in costly information acquisition is measured using a continuous variable. To our knowledge, there is no available software package, built-in algorithm, or easy formula that directly addresses the power analysis behind our research designs. Thus, we draw upon existing theoretical and methodological discussions in the literature (Donner and Koval 1987; Morton and Williams 2010; List, Sadoff and Wagner 2011; Ahn, Heo and Zhang 2014; Chandar et al. 2019; Clifford, Sheagley and Piston 2021) to guide our sample size calculations, and we develop formal protocols for these calculations and power analysis, as detailed below.

E.6.1 How many committees do we need?

In each experiment, subjects are randomly assigned to one of two experimental conditions. For clarity in the power analysis, we refer to these as the *control group* ($\texttt{c}$) and *treatment group* ($\texttt{t}$). In both groups, there are $n_1 = n_2 = n_{cmte}$ committees, each consisting of a fixed number of members, conducting committee voting over $m = 20$ periods with varying parameterizations. In each period, committees vote between two alternatives, $w \in \{\mathbf{1}, \mathbf{2}\}$. There are two equally likely states of the world, $\omega \in \{\mathbf{1}, \mathbf{2}\}$, and committee members receive a payoff of $\overline{u}$ if the adopted alternative $w = \omega$, and 0 otherwise. The goal of the committee is to collectively choose the alternative that matches the state of the world.

Let Y_{ijk} be the binary random variable representing the jth ($j = 1, \ldots, m$) observation in the ith ($i = 1, \ldots, n$) committee of the kth experimental condition ($k \in \{\texttt{c}, \texttt{t}\}$). Here, $Y_{ijk} = 1$ indicates that the committee's choice matches the state of the world, while $Y_{ijk} = 0$ indicates otherwise. Following the general setup of existing theoretical and methodological discussions in the literature, we assume that observations within a committee are exchangeable; given m, the observations $Y_{i1k}, \ldots, Y_{imk}$ share a common marginal probability $P(Y_{ijk} = 1) = p_k$ ($0.5 < p_k < 1$) and a common intra-committee correlation coefficient, $\rho = \frac{\sigma_B^2}{\sigma_B^2 + \sigma_W^2}$, where σ_B^2 is the between-committee variance, and σ_W^2 is the within-committee variance. The correlation coefficient ρ reflects the clustering effect and the degree of similarity of observations both within and between committees.

Carefully addressing ρ is crucial for sample size calculations and power analysis in experimental designs like ours (Morton and Williams 2010; List, Sadoff and Wagner 2011; Fréchette 2012). On one hand, the play of a series of committee voting games with varying parameterizations offers several advantages. For instance, it reduces the likelihood of Type II errors caused by stochastic factors (Morton and Williams 2010), such as random guesses about the state of the world by uninformed participants and extremely high/low information cost stochastically generated in the experiments. Each period operates independently, with relevant parameters—such as the state of the world and information costs—redrawn from period to period. Communication is prohibited both within and between periods. Participants receive limited feedback on the previous period's committee decision, but no information about realized costs, who acquired expertise, or the voting decisions of others. This limited feedback minimizes potential cross-decision influences (Fréchette 2012) while preserving the nature of committee voting, where outcomes are collectively determined by the committee rather than by an individual. On the other hand, treating each period's observation as independent can underestimate the true p-value and inflate the type I error rate, since correlations among observations within a committee are typically positive (Blainey, Krzywinski and Altman 2014). Therefore, it is essential to employ statistical methods that account for the dependence of within-committee observations when conducting sample size calculations and power analyses in similar experimental designs.

We aim to test for differences in the quality of committee voting, denoted as $P(Y = 1)$, between the control and treatment groups. We use one-tailed hypothesis testing for the calculations of sample size and power analysis. The null H_0 and alternative H_1 hypotheses are as follows:

> H_0: The quality of committee voting is equal between the control and treatment groups, $p_{\texttt{c}} \geq p_{\texttt{t}}$.
>
> H_1: The quality of committee voting differs between the control and treatment groups, $p_{\texttt{c}} < p_{\texttt{t}}$.

The quality of committee voting in group k is estimated by

$$\hat{p}_k = \frac{\sum_{i=1}^{n} \sum_{j=1}^{m_{ik}} Y_{ijk}}{\sum_{i=1}^{n} m_{ik}}, \ k \in \{\texttt{c}, \texttt{t}\}.$$

Conditional on the empirical distribution of m_{ik} and the estimate $\hat{\rho}$, the variance estimate $\hat{p}_k$ is

$$V(\hat{p}_k) = \hat{p}_k(1 - \hat{p}_k) \frac{\sum_{i=1}^{n} m_{ik} \left[1 + (m_{ik} - 1)\hat{\rho}\right]}{\left(\sum_{i=1}^{n} m_{ik}\right)^2}, \ k \in \{\texttt{c}, \texttt{t}\}$$

Since $n_1 = n_2 = n_{cmte}$, the variance of $(\hat{p}_{\texttt{c}} - \hat{p}_{\texttt{t}})$ is estimated by

$$V(\hat{p}_{\texttt{c}} - \hat{p}_{\texttt{t}}) = \left[\hat{p}_{\texttt{c}}(1 - \hat{p}_{\texttt{c}}) + \hat{p}_{\texttt{t}}(1 - \hat{p}_{\texttt{t}})\right] \frac{1 + (m - 1)\hat{\rho}}{n \cdot m}.$$

We reject the null hypothesis $H_0 : p_{\texttt{c}} = p_{\texttt{t}}$ at the significance level α if

$$\left| \frac{\hat{p}_1 - \hat{p}_2}{\sqrt{V(\hat{p}_1 - \hat{p}_2)}} \right| > z_{1-\alpha},$$

where $z_{1-\alpha}$ is the $100(1-\alpha)$th percentile of the standard normal distribution. Under the alternative hypothesis, the power of the test is approximated by

$$\Phi\left(\frac{|p_{\mathtt{c}}-p_{\mathtt{t}}|}{\sqrt{[p_{\mathtt{c}}(1-p_{\mathtt{c}})+p_{\mathtt{t}}(1-p_{\mathtt{t}})]\,\frac{1+(m-1)\hat{\rho}}{n_{cmte}\cdot m}}}-z_{1-\alpha}\right).$$

The sample size estimate needed to achieve a power of $1-\beta$ can be obtained by solving the following equation:

$$\frac{|p_{\mathtt{c}}-p_{\mathtt{t}}|}{\sqrt{[p_{\mathtt{c}}(1-p_{\mathtt{c}})+p_{\mathtt{t}}(1-p_{\mathtt{t}})]\,\frac{1+(m-1)\hat{\rho}}{n_{cmte}\cdot m}}}-z_{1-\alpha}=z_{1-\beta}.$$

As a result, with the significance level of α and a power of $1-\beta$, the sample size for comparing the quality of committee voting has the form

$$n_{cmte}=\frac{p_{\mathtt{c}}(1-p_{\mathtt{c}})+p_{\mathtt{t}}(1-p_{\mathtt{t}})}{(p_{\mathtt{c}}-p_{\mathtt{t}})^2}\cdot\frac{1+(m-1)\hat{\rho}}{m}\cdot(z_{1-\alpha}+z_{1-\beta})^2. \tag{5}$$

Different experimental designs lead to varying data generation processes, resulting in different estimates of $\hat{\rho}$. Based on our simulations, experiences, and our previous research, we anticipate significant variance driven by the varying parameterizations (e.g., the state of the world and information cost) across the 20 committee voting periods, and thus we assume $\hat{\rho}=0.1$ for this study. Based on our predictions and simulations, in a committee with five members, when none of the committee members are informed, $P(Y=1|N_5^{i=0})=0.5$; when one is informed, $P(Y=1|N_5^{i=1})=0.65$; when two are informed, $P(Y=1|N_5^{i=2})=0.8$; when three are informed, $P(Y=1|N_5^{i=3})=0.92$; when four are informed, $P(Y=1|N_5^{i=4})=0.97$; when all are informed, $P(Y=1|N_5^{i=5})=0.99$. We use $p_{\mathtt{c}}=0.65$ and $p_{\mathtt{t}}=0.85$ to calculate the required sample size. To achieve 80% power at a 5% significance level with $m=20$, the required number of committees for each intervention group is the smallest integer greater than or equal to n_{cmte} from Equation 5:

$$n_{cmte}=\frac{[0.65(1-0.65)+0.85(1-0.85)]\cdot[1+(20-1)\cdot 0.1]\cdot(1.645+0.842)^2}{(0.65-0.85)^2\cdot 20}\approx 7.96.$$

Thus, in each experiment we need eight committees for the control group and eight for the treatment group. It is important to note that the statistical method for sample size estimation aligns with the statistical method for data analysis. We will use clustered parametric tests to examine the difference between $p_{\mathtt{c}}$ and $p_{\mathtt{t}}$. Additionally, we will double-check our findings by performing a more conservative, analogous non-parametric Fisher-Pitman permutation test, which relies on fewer and weaker assumptions and exhibits the highest power (100%) compared to related tests (Siegel 1957). A Monte Carlo study by Moir (1998) demonstrates that Fisher-Pitman permutation tests maintain statistically reliable power with as few as eight observations per treatment category.

E.6.2 How many subjects do we need?

To understand the why there are differences in committee performance, we also explore differences between women and men in terms of their preferences for cooperation and risk. We conduct a threshold public good game, which is widely applied to elicit social preferences, and we use individuals' contributions as a proxy to examine whether women are more collaborative (Palfrey and Rosenthal 1984; Bicchieri 2016). In this game, each member of the same 5-person committee decides how much of 100 experimental currencies (ECUs) to contribute to the group account. Only when the group's aggregate contribution is equal to or over the threshold (i.e., 250 ECUs), individuals can benefit from the contribution. Otherwise, if the group's aggregate contribution is less than the threshold, everyone loses their contribution. We measure risk preference using the method developed by Gneezy and Potters (1997). Each decision-maker chooses how much of 100 ECUs she or he wishes to invest in a risky option and how much to keep. Both of these two measurements are incentivized with real monetary consequences. These measurements are incentive-compatible such that participants can achieve the best outcome by acting according to their true preferences.

Preferences for cooperation and risk are elicited separately from the committee voting process, ensuring that the experimental conditions and interventions do not influence the measurement of these preferences. Following Bicchieri (2016); Gneezy and Potters (1997), we assume $|\bar{y}_1 - \bar{y}_2| = 15$ and $\sigma_1^2 = \sigma_2^2 = 20$. To achieve 80% power at a 5% significance level, the required sample size is $n = \frac{(\sigma_1^2+\sigma_2^2)(z_{1-\alpha}+z_{1-\beta})^2}{(\bar{y}_1-\bar{y}_2)^2}$. This implies that we need, at minimum, 22 women and 22 men to detect the predicted gender differences in preferences for cooperation and risk.

In the experiments, we treat information acquisition as a unified process throughout the committee proceedings, and we measure individuals' willingness to exert costly effort to obtain information. Specifically, before casting their votes, each committee member independently decides how much effort to invest in acquiring information for themselves. Each participant is given 100 ECUs, which they can either keep for exchange into U.S. dollars or spend on acquiring information about the true state of the world. An individual's *willingness to acquire information* is measured by the amount they are willing to pay for this information. The difficulty of the problem is measured by the *ex-ante* unknown cost of information, which is randomly generated by the computer as an integer between 1 and 100. Participants are informed of the true state of the world and paid for the stochastically generated information cost when their willingness to acquire information meets or exceeds the cost. Since the cost of information is randomly generated, the more effort a participant exerts, the more likely they are to be informed.

While gender differences in preferences for cooperation and risk are well documented in the literature, we find little empirical studies that offer clear expectations regarding the differences in means and variances of gender differences in information acquisition. However, since we use the same 0-100 scale to measure effort in information acquisition, and given the context of committee voting, we expect correlations between individuals' information acquisition decisions and their preferences for cooperation and risk. Based on our experience and simulation analysis, we speculate that the differences in means and variances of information acquisition are similar to those observed for cooperation and risk preferences.

Let n_1 be the sample size estimate from a simple randomization trial without clustering effects. We assume $|\bar{y}_1 - \bar{y}_2| = 15$ and $\sigma_1^2 = \sigma_2^2 = 20$. To achieve 80% power at a

5% significance level, the required sample size is $n_1 = \frac{(\sigma_1^2+\sigma_2^2)(z_{1-\alpha}+z_{1-\beta})^2}{(\bar{y}_1-\bar{y}_2)^2} \approx 22$. In our experiments, the gender composition is unknown to the participants. Since we construct anonymous experimental environments and control the interactions and feedback within committees, the sample size calculations and power analysis only need to account for the aggregate clustering effects on individuals' information acquisition decisions. We address the clustering effects by multiplying n_1 by the variance inflation factor $1 + (\zeta - 1)\hat{\rho}$, where $\zeta = 5$ represents the constant committee size across all committees and $\hat{\rho} = 0.1$ is the intra-cluster correlation coefficient that reflects the clustering effect and the degree of similarity of observations both within and between committees. This gives $n_1(1 + (\zeta - 1)\hat{\rho}) \approx 31$. Since we have eight committees for each experimental condition, and to maintain symmetry, we require at least 32 women and 32 men to detect the predicted gender differences in information acquisition. Based on these analysis, the gender compositions that include both women and men, and vary as women-majority and men-majority committees, are fixed. These analyses also apply when calculating the sample size and performing power analysis for experimental designs that incorporate veto players.

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