THE DUAL FUNCTION OF ORGANIZATIONAL STRUCTURE:

AGGREGATING AND SHAPING INDIVIDUALS' VOTES

HENNING PIEZUNKA*
INSEAD
1 Boulevard de Constance
77305 Fontainebleau, France
e-mail: henning.piezunka@insead.edu

OLIVER SCHILKE*
The University of Arizona
405GG McClelland Hall, 1130 E. Helen St.
Tucson, AZ 85721, USA
e-mail: oschilke@arizona.edu

ABSTRACT

How do organizational structures influence organizational decision making? This article reveals organizational structures' dual function: they both (1) aggregate and (2) shape individuals' decisions. What makes this dual function so remarkable is that the two effects are diametrically opposed to one another. Ceteris paribus, a less stringent decision-making structure reduces the amount of support required for a given project to be greenlit at the organizational level, which should result in more investments getting approved. However, we find that this ceteris paribus assumption does not hold, because a less stringent decision-making structure also reduces individuals' tendency to provide their support for an investment. Our experimental investigation of organizational voting provides evidence for our position that organizational structure plays an important role beyond mere aggregation: voting thresholds also affect individuals' voting behavior. The combination of both effects explains why the organizational adoption of a new voting threshold may not yield the intended outcome.

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When the committee of the venture capital firm Draper Fisher Jurvetson (DFJ) voted on possible investments, promising (and eventually successful) ventures often did not receive the necessary number of votes. In response, the firm implemented a lower threshold such that only a single committee member voting in favor of investing in a given venture would trigger the go-ahead (Liu et al. 2017). The managers believed that a key reason for the firm's limited growth was that they required too high an internal consensus to invest. The adoption of a lower threshold was intended to fix this problem and substantially increase the number of investments. However, once the new, lower threshold was adopted, it turned out it did more than just change the number of votes needed to make an investment: it also changed the voting behavior of the committee members, who suddenly became much less inclined to vote yes. Ultimately, the newly introduced voting structure did not have the desired effect.

In this article, we examine the consequences of alternative thresholds when aggregating votes into an organizational decision. Following Sah and Stiglitz (1986), research on organizational structure has pointed to the crucial role that voting thresholds play (Csaszar 2012, Csaszar and Eggers 2013). Ceteris paribus, a lower threshold should result in a greater number of investments by the organization, simply because it reduces the required number of yes votes (Christensen and Knudsen 2010, Knudsen and Levinthal 2007, Sah and Stiglitz 1986). However, we argue that relaxing this ceteris paribus assumption can help advance research in this domain because the adoption of a lower threshold has a neglected but important side effect: it leads individual members of the organization[1] to adjust their voting behavior conditional on the voting rule. Specifically, we examine whether a lower voting threshold reduces members' tendency to cast a yes vote in favor of an investment, which may ultimately keep the organization's number of investments from rising. In other words, more liberal organizational aggregation rules may be counteracted by more conservative individual voting—with the result that these two countervailing forces may (in part) cancel each other out. Therefore, in order to capture the true effects of differences in organizational structure, it is necessary to account for the ways in which these differences change individuals' behavior.

Understanding the effects of organizational design choices—and specifically of voting thresholds—is crucial for understanding the behavior of organizations, including their evaluation and selection of investments. All organizations face the challenge of deciding in which alternatives to invest (Bardolet et al. 2011, Boudreau et al. 2016,

[1] In this paper, we use the terms "organizational members," "committee members," "decision makers," "actors," and "agents" interchangeably to refer to the individuals participating in the organizational vote.

Criscuolo et al. 2017, Lane et al. 2022, Piezunka and Dahlander 2015, Sengul et al. 2019). Because eventual success of these investment opportunities is uncertain, managers often need to go beyond the information given to them and attend to even subtle cues in their immediate environment (Phadnis et al. 2015). In this article, we investigate how organizational structure, and specifically the voting threshold employed in committee decisions, provides such clues and in turn affects the degree to which organizational members support investments in uncertain alternatives.

Our investigation builds on and extends the literature on aggregation structures in organizations. Beginning with the seminal work by Sah and Stiglitz (1986), a growing body of organizational design research has examined how different ways of designing thresholds shape organizations' decisions (Christensen et al. forthcoming, Christensen and Knudsen 2010, Csaszar 2013, Csaszar and Eggers 2013, Knudsen and Levinthal 2007, Piezunka et al. 2022). The majority of this work has developed formal models in which individual voting behavior is assumed to be unaffected by the aggregation mechanism. In our article, we overcome this limitation and offer a theory that focuses on how thresholds can substantially affect individual voting behavior. For this purpose, we introduce the notion of *strategic voting* from political science (e.g., Baltz 2022, Battaglini et al. 2010, Feddersen and Pesendorfer 1996, 1998, Guarnaschelli et al. 2000) in order to develop and test the argument that voters take into account information beyond signals directly related to the alternative they evaluate. Specifically, we suggest they attend to the organization's aggregation structures which thus have an effect that goes far beyond their mere calculus (transforming individual votes into an organizational decision) in that they affect the very votes they use as inputs.

Our core claim is therefore that organizational structures have a *dual function*: they (1) aggregate, *and* (2) shape voting behavior. What makes it critical to account for and disentangle these two functions is that they are in direct opposition: a lower, more liberal threshold reduces the bar for a project to be greenlit at the organizational level, but it also reduces individuals' tendency to vote in favor of an investment. Conversely, a higher, more conservative threshold raises the bar for a project to be greenlit at the organizational level, but it also increases individuals' tendency to vote in favor of an investment. As a result, changing the organizational voting threshold may not yield the anticipated effect, because individuals' behavior is a function of the voting regime.

Our theory emphasizes that decision-making structures and individual behavior are deeply intertwined and thus must not be studied in isolation. It is critical to account for the macro-to-micro implications of organizational structures in order to capture their true effects (Gavetti et al. 2007, Greve 2013). Our findings

therefore shed new light on the microfoundations of aggregation (Barney and Felin 2013, Cowen et al. 2022, Davis and Aggarwal 2020), in an effort to better understand how organizational structure manifests itself in individual cognition and behavior, which ultimately shape organizational action. More broadly, our study showcases the usefulness of Coleman's (1990) bathtub for examining organizational behavior that it is fundamentally shaped by cross-level processes.

To test our theory empirically, we build on and extend an emerging body of research that deploys experiments to study organizational design (e.g., Christensen et al. forthcoming, Csaszar and Laureiro-Martínez 2018, Johnson et al. forthcoming, Keum and See 2017, Knudsen et al. 2018). In our experimental task, participants assume the roles of partners in a venture capital firm voting on whether to invest in certain startups. While participants vote autonomously, their votes are aggregated into an organizational decision, which allows us to manipulate the voting threshold. Taken together, our article offers a bridge between research on information aggregation in political science and organizational design, elucidates the dual function of thresholds, and provides empirical evidence for the effect of thresholds on voting behavior.

THEORETICAL BACKGROUND

Organizational Aggregation

This investigation follows a microstructural approach to organizational design, which places aggregation structures linking the individual and the organizational level at center stage (Christensen and Knudsen 2010, Knudsen and Levinthal 2007, Puranam 2018, Turco 2016). Organizational decisions are commonly not made by unitary actors but rather by multiple agents who make decisions jointly (Csaszar 2012, Mack and Szulanski 2017, Sah and Stiglitz 1986). Rich empirical evidence illustrates how organizations rely on multiple agents who make decisions without resorting to authority as the primary coordination mechanism. Such decision making among equals is particularly widespread in top management teams (Eisenhardt 1989, Hambrick et al. 1996), boards (Garg 2013, Westphal and Fredrickson 2001), steering committees (Loch et al. 2017), investment committees (Csaszar 2012, Hu et al. 2022), and panels (Boudreau et al. 2016, Criscuolo et al. 2017).

The question of how to organize in the absence of authority while integrating individual agents' choices has endured as one of the most fundamental and pressing issues in organizational design (Maciejovsky and Budescu 2020, Puranam 2014). Organizational structures that deploy *voting* to aggregate people's choices and to make

decisions at the organizational level represent one key approach to addressing this issue (Csaszar 2012, Sah and Stiglitz 1986). Indeed, it has become increasingly common for decisions involving multiple individuals to take place via voting (Csaszar and Enrione 2015, Mack and Szulanski 2017). Organizational members vote whether or not an organization should pursue a particular initiative, in an effort to separate the good alternatives from the bad (Christensen and Knudsen 2009). Voting is often seen as the natural way to aggregate individual choices at the organizational level (Turco 2016), particularly in the case of strategic decisions,[2] which tend to be high-stakes, complex, and nonroutine. The types of strategic decisions on which organizational agents regularly vote range from firms narrowing down innovation ideas to pursue (Reitzig and Maciejovsky 2015) to mutual funds choosing stocks in which to invest (Csaszar 2012) to venture capitalists picking ventures to support (Liu et al. 2017, Wu 2016).

Voting has several desirable features that explain its popularity in the context of organizational decision making. Most notably, it is a mechanism that allows tapping into the knowledge of several individuals. The knowledge held by multiple organizational members tends to be more diverse and more comprehensive than that held by a single individual. As a result, organizations that pool the knowledge of many members have the potential to benefit from error cancelation and thus engage in more effective organizational decision making (Csaszar 2018, Csaszar and Steinberger 2022, Surowiecki 2004). Therefore, it is not surprising that many organizations rely on voting; however, there is considerable variety in how organizations aggregate individual votes (Piezunka et al. 2022).

Research in organization design, going back to Sah and Stiglitz (1986), has examined the implications of various kinds of aggregation structures. The majority of research in this domain has deployed formal models (Christensen and Knudsen 2009, 2010, Csaszar 2013, Csaszar and Eggers 2013, Knudsen and Levinthal 2007, Piezunka et al. 2022). These formal models have more recently been complemented by empirical studies investigating the different kinds of aggregation structures deployed by organizations and how these structures shape organizational decisions (Christensen et al. forthcoming, Keum and See 2017, Reitzig and Maciejovsky 2015). In a large-scale archival study of investment funds, for instance, Csaszar (2012) shows that organizations in this field vary considerably in the way they aggregate votes and that such variations translate into differences in these organizations' investment decisions. A key concern in this literature is how alternative voting thresholds affect organizational

[2] We follow Phadnis et al. (2015) in defining strategic decisions as those that deal "with the long-term allocation of existing resources and the development of new ones essential to assure the continued health and future growth of the enterprise".

decisions. Specifically, research has focused on the linkage between the magnitude of the voting threshold and the likelihood that an organization pursues a particular course of action.

However, what has largely been neglected so far is how the magnitude of the voting threshold may affect individual agents' voting behavior—a key mechanism linking voting thresholds to organizational action. While Sah and Stiglitz (1986, p. 722) did mention that the deployed structure may influence individuals' voting behavior, this claim has yet to be examined empirically and elaborated theoretically. The research on organizational structure that has built on Sah and Stiglitz (1986) has been mostly conceptual in nature and has to the most part focused on the mechanical aggregation of votes, studying how different voting thresholds affect organizational selection, but assuming that individual voting behavior is exogenous (Christensen and Knudsen 2009, 2010, Csaszar 2013, Csaszar and Eggers 2013, Knudsen and Levinthal 2007). Csaszar (2012) constitutes a rare exception in that he examines empirically the combined overall effect (i.e., the structural and behavioral effect). But what is still lacking is a theoretical account (and empirical examination) of the behavioral effect—that is, how aggregation structures shape individual decision makers' voting behavior. Such a cross-level account can add both greater precision and much-needed insight into the theoretical mechanisms of organizational design choices. In developing our theoretical argument, we draw on relevant insights from political science, specifically the *Condorcet jury model* and *strategic voting*.

Condorcet Jury Model

In its original formulation, the Condorcet (1785) theorem refers to juries in criminal and civil trials voting on the innocence or guilt of a defendant; however, the framework can be readily generalized to collective decision making under uncertainty more broadly (Guarnaschelli et al. 2000), and to organizational decision making specifically (Csaszar 2018). The general model features a committee of size $n > 1$ that is deciding on a binary policy. The policy is determined by an election, in which each committee member can cast either a yes or a no vote, after which the individual votes are aggregated into a group decision according to some voting rule (Ali et al. 2008). In this model, one can compare voting behavior under alternative voting rules, consistent with our interest in the effect of different forms of organizational aggregation on strategic decision making.

Both the Condorcet model and our investigation assume general objective utility in the group decision and that individuals all have the same ultimate preferences (Austen-Smith and Banks 1996, Guarnaschelli et al. 2000). For instance, when a committee of decision makers decides whether to invest in a project, it is assumed that all

committee members will seek the decision that maximizes organizational performance (i.e., greenlight the promising projects and avoid the lemons). Correspondingly, there is no conflict of interest or heterogeneity of preferences among the committee members as they strive to invest in successful projects. However, under conditions of uncertainty regarding the true quality of the projects, differences among individual committee members' judgments as to which investment will yield high performance are likely. These differences create an information aggregation problem, making it harder for the group to reach a consensus on the "right" conclusion and producing a need for formalized voting regimes (Guarnaschelli et al. 2000). The presence of such differences underscores the crucial importance of voting thresholds, as they determine the minimum degree of consensus required.

Strategic Voting

Until relatively recently, the literature employing the Condorcet jury model has assumed that actors engage in "naïve voting" (cf. Feddersen and Pesendorfer 1998). Naïve voters simply vote "sincerely," based on their private signal and individual judgment of the considered alternative; they vote as if they were the only voter and completely ignore the organizational context of the decision. Naïve voting thus implies that there will be no difference between individuals' private judgment of a project and their vote on it. As a result, organizational design—and more specifically, how votes are aggregated—will have no effect on naïve voters' behavior.

However, many voters do not behave this way and instead engage in "strategic voting" (Austen-Smith and Banks 1996, 1999, 2005, Austen-Smith and Feddersen 2009, Battaglini et al. 2010, Bhattacharya et al. 2017, Coughlan 2000, Feddersen and Pesendorfer 1996, Guarnaschelli et al. 2000). On the most general level, the notion of strategic voting highlights that the way people vote as part of an organization is typically not the same as the way they would make choices on their own as they take the organizational context into account (e.g., elements such as the voting threshold, the reward structure, the opinions of others, etc.). That is, when voting as a member of a collective, an individual may not make the same decision as when that individual alone selects the outcome. A strategic voter determines whether to vote for an alternative based upon both their private information *and* the information they infer from the organizational context. Research has repeatedly demonstrated that strategic voting constitutes an apt description of decision makers' behavior (Baltz 2022). For example, Maug and Rydqvist (2008) illustrate how shareholders engage in strategic voting when screening management proposals at annual general meetings. Similarly, Oraiopoulos and Kavadias (2020) show how strategic voting considerations are at play in the

context of executive committees in pharmaceutical firms deciding on drug development projects. We can thus expect strategic voting to be common in organizational settings. Building on Sah and Stiglitz (1986), our hypotheses focus on how a particular feature of the organizational context—the voting threshold—shapes strategic voting.

In developing and testing our theory, we start from the assumption that all decision makers know the aggregation rule ex ante and vote in secrecy and simultaneously (i.e., decision makers do not observe how their peers vote). We further assume—consistent with the Condorcet model—that voters have no conflictual goals or heterogenous preferences and instead all try to identify the "correct" choice that maximizes the collective outcome. In other words, they are performing an expected utility maximization, where their own utility is aligned with the utility of the organization.

HYPOTHESES

Voting Thresholds and Strategic Voting

We suggest that the voting threshold (i.e., how many organizational members must vote 'yes' to trigger an organizational investment) has a substantial effect on people's voting behavior. We test this proposition and elaborate the underlying logic based on the strategic voting model. We start with the observation that voters hold some private information about a given project that shapes their individual beliefs about project success. Because this belief must ultimately be translated into a binary yes/no vote, voters determine the *reservation level* they deem necessary in order to vote 'yes'—that is, how strongly convinced they must be of project success in order to cast a yes vote. But how do voters set their reservation level? Voters are aware that their private information may be biased or that they may have made a mistake in their interpretation of the information; that is, they are well aware of the uncertainty that underlies the probability distribution. Given this uncertainty, voters may act strategically in setting their reservation level while taking the voting context into account. Specifically, we suggest they will condition their reservation level—and thus, their vote—on the voting threshold.

Consider the case of a low voting threshold and let us assume the extreme case where each voter has the power to trigger the organizational investment with their single yes vote. A voter who strives to maximize organizational performance will likely be especially careful to vote 'yes' under this voting regime. Because their yes-vote alone is sufficient to make the organization invest in the project, even if all others voted no, the voter will be particularly concerned that their private information may be noisy or positively biased or that their

interpretation of this information may be misguided. To avoid triggering an organizational investment when project success is uncertain but the voting threshold is low, the individual is thus likely to set a rather high reservation level—that is, they will only support the project when they are strongly convinced of its success.

Compare this scenario to a case of a high voting threshold, such as a regime in which the majority of voters has to show their support in order for the organization to invest (e.g., three out of five). Here the individual voter is likely to set a lower reservation level, rendering them more likely to vote yes. A higher organizational threshold helps to attenuate people's concern that their noisy or biased information may misguide them to vote favorably and trigger an organizational investment in an unsuccessful project given that the organization is "protected" from individual outliers by the higher threshold.

The proposed linkage between the organizational voting threshold and the personally set reservation level is consistent with the logic suggested by research in political science (Austen-Smith and Feddersen 2009, Feddersen and Pesendorfer 1998, Guarnaschelli et al. 2000). This research suggests a focal agent assumes they turn out to be the *pivotal voter*—that is, the voter whose vote makes the difference between a go and a no go. As Palfrey (2016, p. 401) puts it, a person's vote depends "not only on his or her private signal, but also on the information implied by the event that he or she is a pivotal voter." Specifically, we suggest that the individual voter assumes to be the pivotal voter when setting his or her reservation level.

Consider again the case of a low voting threshold, where each voter has the power to trigger the organizational investment with their single yes vote. If a voter now imagines to be the pivotal voter, this means he or she is the only member of their committee who voted yes. We expect an individual is only willing to be the only supporter and thus to single-handedly trigger the organization's investment if they are highly convinced of the project's quality. The individual knows that a scenario in which he or she is the only one who votes yes is most likely to occur if his or her private information is noisy or positively biased or their interpretation of this information is misguided. Anticipating this scenario will thus lead them to set a very high reservation level that accounts for potential noise and positive bias they may be subject to. In contrast, in the case of a high threshold (e.g., three out of five), turning out to be the pivotal voter does not suggest that their private information is noisy or positively biased or their interpretation of this information is misguided—after all, several other committee members must have voted yes. In brief, the pivotal voter logic suggested by political science supports our position that a higher

voting threshold (i.e., more votes are needed to trigger the organizational investment) will result in a lower reservation level, rendering the individual voter more likely to vote yes. Conversely, a lower voting threshold (i.e., fewer votes are needed to trigger the organizational investment) will result in a higher reservation level, in turn reducing the likelihood of an individual voting yes. Our core argument can thus be captured by the following hypotheses:

H1: Actors are more likely to support strategic projects when the aggregation structure's voting threshold is high (rather than low).

H2: Inferred information mediates the positive effect of the voting threshold on actors' project support, such that a higher threshold has a positive effect on inferred project quality, which in turn has a positive effect on actors' project support.

Asymmetric information

Beyond the proposed main and mediating effects, our article also aims to establish an important boundary condition with the potential to "turn off" the influence of thresholds on voting behavior, offering further insight into the underlying mechanism. Expanding upon our above theorizing regarding information inferred from the pivotal vote constellation, we develop the argument that voters' private *level of information*—that is, whether they feel worse vs. better informed relative to the other voters—will interact with the organization's voting threshold. Accounting for differences in the private level of information reflects the fact that committee members are usually not equally well informed about the project on which they are voting. Better-informed voters may have more experience or education pertinent to the project, or they may have invested more time and effort in doing their research and preparing for the committee vote. Other voters may be more poorly informed, lacking relevant expertise and/or background information.

Building on theory about asymmetric information in group decisions (Battaglini et al. 2010, Bhattacharya et al. 2017, Piketty 1999), we argue that strategic voting—that is, the process of taking into account the organizational context and specifically the voting threshold—will be comparatively more pronounced when the focal voter has a lower information level than other committee members. A poorly informed voter who has little private information about project quality relative to the other voters is more concerned about their information being noisy and are thus (even) more focused on the organizational voting threshold when determining how to vote. Poorly informed voters effectively strive to delegate the decision to the better-informed ones so as not to interfere with the efficient information aggregation performed by voters with more information. If this is true, the threshold-level effect proposed in our first hypothesis should be especially pronounced among voters with

low levels of information, as these voters are particularly attuned to inferring how others may vote and conditioning their own vote accordingly. By contrast, high levels of private information should increase the decision maker's tendency to vote sincerely—i.e., based upon their private judgment of project quality. If a voter has reason to believe they are better informed relative to the other voters, this better-informed actor is likely to abstain from inferring the behavior of voters with lower information levels. This position is consistent with prior research finding private information to weaken heuristic types of judgments (See 2009). In sum, poorly informed voters will pay more attention to situational cues stemming from the pivotal vote constellation than highly informed voters who are confident in their private assessment of project quality. Hence:

> *H3: When actors' level of information is high (rather than low) relative to the other voters, the effect of the aggregation structure's threshold on actors' voting behavior is attenuated. In other words, there will be a negative interaction between actors' level of information and the aggregation structure's threshold in predicting actors' support of strategic projects.*

STUDY OVERVIEW

We conducted a series of ten experimental studies to test our three hypotheses. In addition to their well-known ability to pinpoint causality and disentangle micro-level mechanisms (e.g., Agarwal et al. 2010, Levine et al. 2017, Li et al. 2018, Lonati et al. 2018), experiments are particularly useful for research on information aggregation because of the sensitivity of results to exact voting parameters and because of the lack of relevant archival datasets (Ali et al. 2008, pp. 181-182, Csaszar and Laureiro-Martínez 2018, p. 515). In our setting, specifically, it would be virtually impossible to obtain the required data (e.g., exact voting parameters, committee member characteristics, their level of information, and vote outcomes) from either archival sources or surveys. And even if that data were available, we could still not be entirely sure that any observed effect is causally due to the voting threshold. In experiments, participants are randomly assigned to conditions such that potentially confounding factors can be held constant, affording high internal validity. It is thus not surprising that Cyert et al. (1959, p. 94) insist that "(m)any of the major propositions in organization theory depend on evidence generated by studies in the laboratory." More and more researchers are now following this call, and experimental designs are becoming commonplace in organization theory and strategy alike (Billinger et al. 2021, Bitektine et al. 2022, Bolinger et al. 2022, Croson et al. 2007, Di Stefano and Gutierrez 2019). A possible drawback pertains to external validity, in that generalizing from the laboratory to real-world settings may be seen as more difficult than generalizing from one real-world setting to another (Krause et al. 2014). We believe that we have struck an

acceptable balance between internal and external validity by closely following pertinent recommendations regarding experimental design in organizational research (e.g., Aguinis and Bradley 2014, Bitektine et al. 2018). For instance, our samples include MBA students (Study 1A), Executive MBA participants (Study 3), and working professionals (Studies 1B-2C) rather than undergraduates, the population traditionally used in experimental research (Falk and Heckman 2009). Perhaps more importantly, we devised an experimental task that resembles key decision-making features found in organizations while also ruling out extraneous factors that would be difficult to isolate in complex field settings.

Specifically, we developed an experimental setting that fits the particular needs of the current investigation. Consistent with the parameters of the general Condorcet model, the experimental task needed to involve a *group* of size $n > 1$ that decides over a *binary* policy with *uncertain* outcomes and uses a *voting rule* to aggregate individual votes. Consistent with our objective to inform organization theory and strategy research, the nature of the decision needed to be one that is *organizational*, of *strategic import*, and commonly made through *managerial committees*. To create such a setting, we built on experimental procedures established in the jury literature in political science (Ali et al. 2008, Guarnaschelli et al. 2000) while adapting the decision context to a venture capital scenario, an organizational setting in which group decision making among partners is particularly common (Bottazzi et al. 2016, Sahlman 1990). Different venture capital firms use different voting rules (Liu et al. 2017), and individual venture capital partners differ in their level of information about specific ventures brought up for consideration (Wu 2016); for these reasons, the venture capital setting is an ideal fit to test our hypotheses.

In the experimental task, each participant assumed the role of a member of the investment committee of a venture capital firm and cast a series of go/no-go votes on a total of nine early-stage ventures, in a set-up very similar to typical real-life scenarios in which investors need to make decisions based on limited information (Huang and Pearce 2015). After learning about the voting threshold (which varied across conditions, as explained below), participants were asked to read a brief dossier for each company containing background information about the product, market, and team (see Online Appendix A). While the information provided was relevant for making funding decisions, the complexity of the dossiers was kept at a relatively manageable level. To make sure the company dossiers were high in psychological realism (Colquitt 2008), we based them on pitch-day profiles of companies graduating from the prominent startup accelerator Y Combinator but changed the identifying details. We chose

the nine companies so as to cover a wide variety of different industry sectors and—unknown to the participants—eventual success outcomes (i.e., five of the companies failed, while four of them demonstrated steady growth—see Online Appendix A). Companies graduating from prominent accelerators constitute highly plausible targets for venture capitalists, as they are pre-selected and nurtured by the accelerator (Hallen et al. 2020, Krishnan et al. 2021). The order in which the nine profiles were presented was randomly determined but consistent across all participants.[3] The presentation of these profiles was followed by a vote on each of the nine companies (see Online Appendix B).

We employ this task as the basis for all experimental studies reported in the paper. The first six studies test the proposed main effect (H1) across various different settings, the next three consider mediation models (H2), and the final one analyzes moderation (H3). More specifically, Study 1A establishes the effect of thresholds of one vs. two yes votes on voting behavior. We then examine whether the effect generalizes to thresholds of different magnitudes (Study 1B), the possibility to abstain (Study 1C), thresholds framed in terms of no votes (Study 1D), groups of different sizes (Study 1E), and the presence of pre-vote deliberation among group members (Study 1F). Having demonstrated robustness, we move on to shed light on the mediation mechanism proposed in H2, using both a measurement-of-mediation (Study 2A) and a causal-chain design (Study 2B) and also examining a potential alternative explanation (Study 2C). The final study, Study 3, investigates the moderating role of the level of information (H3). Table 1 provides an overview of the studies reported in this article. We posted data collection and analysis plans for Studies 1B–2C prior to commencing data collection using the preregistration template developed by van 't Veer and Giner-Sorolla (2016), and we made the study materials, along with the data (in anonymized form, with personal identifiers removed) and log files for all studies, publicly available—links are provided below. Power analyses for all studies are reported in Online Appendix C.

---Insert Table 1 about here---

MAIN EFFECT STUDIES

Study 1A

Sample. Participants in the first study were full-time students in a first-year MBA entrepreneurship course at a large business school with a Financial Times Top 10 MBA program. These students identified entrepreneurship as their primary major or area of study, which ensured that all participants possessed some

[3] Manual inspection of participants' responses across the nine trials failed to reveal any discernable order effects.

degree of interest in and knowledge related to evaluating entrepreneurial ventures (Lee and Huang 2018). The class consisted of 16 sessions containing a total of 140 students (39 women, 101 men), all of whom volunteered to participate in the study. Three students left some sociodemographic information unanswered, but we retained their data for the remaining analyses. Participants were between 25 and 36 years of age (M = 29.48 years, SD = 2.23), and their working experience ranged from 2.5 to 15 years (M = 6.32 years, SD = 2.31). A total of 15.1% of the participants indicated they had some first-hand work experience related to venture investments, such as work in private equity or venture capital. Online Appendix D shows condition-specific summary statistics for this and the other studies.

Procedures. The study was conducted as part of a class activity, and participants expected to be graded as a team according to their venture capital firm's performance. Specifically, they were told that team investments in successful ventures would earn them two points, team investments in failures would result in losing one point, and the team abstaining from investment would leave their score unchanged.[4] How their score on the task would translate into a class grade was deliberately left unspecified. The teams used for the study ranged in size from three to five participants (M = 4.76 participants, SD = 0.56); these teams had been formed at the beginning of the semester for various group-based class activities, and hence the participants were familiar with their teammates. Participants cast their votes individually and without any prior team discussion. The activity was conducted during class without prior announcement, was self-paced, and took approximately 40 minutes to complete. Once the signal to begin was given, all instructions were provided through the written study materials, which participants completed individually in a paper-and-pencil format.

Teams and their members were randomly assigned to one of the two experimental conditions in this single-factor between-subjects design: (1) a threshold of one yes vote vs. (2) a threshold of two yes votes (n = 70 and n = 70 participants, respectively). That is, study materials were identical across conditions, except for the information on how individual votes were aggregated at the team level. In both conditions, participants were told that although they and their teammates would vote on each investment opportunity individually, their votes would subsequently be aggregated at the team level via a predefined voting rule, and their performance on the task would

[4] This scoring scheme was meant to reflect the fact that "good" investments in successful ventures tend to overcompensate venture capitalists for their "bad" investments in flops (e.g., Gompers and Lerner 2004).

be assessed at this aggregated level. In the one-vote-threshold condition, participants were told that the following voting rule would be applied to form a team decision: "If just one team member votes yes, your team will invest in the specific venture." In contrast, in the two-vote-threshold condition, participants were shown the following voting rule: "If two or more of the team members vote yes, your team will invest in the specific venture." Participants then saw a table showing illustrative examples of how team members' individual votes would be aggregated into a team decision and reminding them of the names of their teammates. Participants were then asked to respond to a comprehension question,[5] to read the nine company profiles, and to indicate their yes/no investment vote for each of these nine companies. Finally, participants were asked to provide some basic personal information (including sex, age, work experience, experience with venture investments, number of siblings, and an abbreviated version of Snyder's (1974) self-monitoring scale) and were debriefed. Instruments, data, and log files for Study 1A are available on OSF.[6]

Results. Random assignment to treatment conditions allowed us to assume that all other relevant factors were controlled for by design. Because including unnecessary control variables can decrease statistical power and heighten the chances of Type I and Type II errors, our paper presents the results using only the hypothesized independent variable as predictor. As a robustness check, we included controls for sex, age, work experience, number of siblings, and self-monitoring in our analyses, and results remained virtually unchanged (see Online Appendix E). Because participants voted on nine different companies, we computed the total number of yes votes for each participant to capture the count-dependent variable of "support of uncertain projects." A Poisson regression was run to predict this count based on the study condition.[7] Online Appendix F presents the regression tables for this and the other studies. Results revealed a difference across conditions that was statistically significant at $p = 0.001$: individuals in the two-vote-threshold condition greenlighted 1.36 (95% confidence interval [*CI*], 1.13 to 1.63) times as many investments ($M = 3.89$; $SD = 1.31$) compared to participants in the one-vote-threshold condition ($M = 2.86$; $SD = 1.21$). The obtained effect size is large ($d = 0.82$; 95% bootstrapped *CI*, 0.51 to 1.11).

[5] The comprehension question was: "Imagine you turn out to be the only one in your team who votes in favor of investing in a specific venture, but all other members of your team voted 'No'. Will your team then invest in that venture?" The correct response differed between conditions. We also ran the analyses excluding those seven participants who either answered this question incorrectly or left it unanswered (i.e., with $n = 133$); the results were substantively similar.

[6] https://osf.io/gfc26/

[7] To demonstrate robustness, we also (1) ran a Wilcoxon-Mann-Whitney test and (2) transformed the data from wide to long to estimate a random intercept logistic regression with participant as clustering variable (Rabe-Hesketh and Skrondal 2012). The results of these analyses are consistent with those of the Poisson regression (please see Online Appendix E).

Discussion. The results of Study 1A were consistent with H1. As predicted, we identified a main effect of the aggregation structure's threshold on participants' support of investment projects, with greater support in the two-yes-votes than in the one-yes-vote condition. Study 1B was designed to extend our investigation by examining whether the effect generalizes to a threshold of three yes votes.

Study 1B

Sample. Participants in Study 1B (as well as in Studies 1C–3) were attendees of an online seminar on entrepreneurship with a particular focus on venture capital. Hosted on Zoom, the seminar was offered in multiple sessions between July 2020 and July 2021. The seminar was primarily advertised to alumni of a large business school. Using a snowball sampling approach, we asked participants to recommend the seminar to some of their colleagues and friends who might find it relevant. The three-hour seminar covered a variety of issues in entrepreneurship, and one of the activities involved an online version of the investment task described above. A total of 351 individuals took part in Study 1B. Of these study participants, 68.66% were male. The average age was 36.52 years (*SD* = 9.05), the average full-time work experience was 13.19 years (*SD* = 8.60), and 29.63% had some first-hand work experience related to venture investments. Among the 86.32% of the participants that were not full-time students at the time of data collection, many participants' job function was in finance (22.11%) or operations (13.86%). The most frequently reported job levels included manager (16.50%) and owner (15.84%).

Procedures. Toward the beginning of the seminar session, participants were assigned to a breakout group that would engage in several short activities. One of the group-related activities involved completing the "investment simulation task." Procedures resembled those of Study 1A, except that the task was administered online via Qualtrics (rather than via paper and pencil). Participants were provided with a study link and asked to complete the task individually within 30 minutes. Teams and their participants were randomly assigned to one of the three experimental conditions in this single-factor between-subjects design: (1) a threshold of one yes vote (n = 115), (2) a threshold of two yes votes (n = 126), and (3) a threshold of three yes votes (n = 110). Our pre-registration, instruments, data, and log files for Study 1B are publicly available on OSF.[8]

We would like to acknowledge the tradeoffs associated with collecting data in online seminars (rather than in person). On the one hand, this approach allowed us to continue data collection during the Covid-19

[8] https://osf.io/rjc8x/

pandemic, when most in-person classes at our institutions got canceled, and to obtain access to a managerial population with considerable relevant work experience. On the other hand, the online seminar setting means that we had to give up a certain amount of control compared to collecting data in a traditional laboratory environment or in-person classes (Hergueux and Jacquemet 2015). Specifically, we had to sacrifice some control regarding the sizes of the groups, because some people dropped out while others joined the seminar late (in Study 1B, for example, we aimed for groups of five, but the eventual number of participants per group was 4.13 on average, $SD = 1.97$). Fortunately, as Study 1E will reveal, our key result holds for groups of different sizes. Relatedly, people dropping out early can produce a discrepancy between what instructions told participants about the number of group members and the actual number of responses obtained per group. While this discrepancy will not influence the main effect on individuals' voting behavior, it complicates actual aggregation of individual responses to the group level. We therefore limit our post-hoc group-level analyses, reported further below, to data obtained in person (i.e., the data from Studies 1A and 3).

Results. We ran a Poisson regression to predict the number of yes votes (ranging from 0 to 9) based on the study condition (threshold of 1, 2, or 3) and found a strong effect ($p = 0.001$). Individuals in the three-yes-votes condition greenlighted the greatest number of ventures ($M = 4.65$; $SD = 1.18$), followed by those in the two-yes-votes condition ($M = 4.22$; $SD = 1.50$), and those in the one-yes-vote condition ($M = 3.74$; $SD = 1.53$).

Discussion. The findings from Study 1B lend additional support to H1, also showing that the proposed threshold effect holds for the higher threshold of three yes votes. The first two studies only gave participants the option of voting in favor or against a given investment opportunity. In many settings, however, investors have the option to abstain (e.g., Mallin 2012). Study 1C examines whether the proposed threshold effect continues to hold when the third option of abstaining is introduced.

Study 1C

Sample. For Study 1C, we recruited a total of 328 participants. Of these participants, 66.16% were male, and the average age was 35.77 years ($SD = 12.02$). Participants had an average of 13.03 years ($SD = 11.00$) full-time work experience, and 29.27% of them had venture investment work experience. The vast majority of participants (78.96%) were not currently enrolled as full-time students. Among these non-students, operations (14.29%) and finance (12.74%) were frequently reported job functions, and owner (22.01%) and analyst/associate

(14.67%) the most frequently reported job levels.

Procedures. Participants were randomly assigned to one of two experimental conditions in this one-factor between-subjects design: (1) a threshold of one yes vote (n = 150) or (2) a threshold of two yes votes (n = 178). The procedures were based on those of Study 1B, except that in addition to voting yes or no on each project, participants were also given the third option to abstain. The pre-registration, instruments, data, and log files for Study 1C are posted on OSF.[9]

Results. A Poisson regression predicting the number of yes votes showed that the voting threshold has an effect that is significant at p = 0.02. Participants in the two-vote-threshold condition supported 1.14 (95% confidence interval [*CI*], 1.02 to 1.27) times as many investments (M = 4.16; SD = 1.34) as participants in the one-vote-threshold condition (M = 3.67; SD = 1.44), which corresponds to a medium effect size (d = 0.36; 95% bootstrapped *CI*, 0.14 to 0.58).

Supplementary Analyses. Our main analysis focused on the number of yes votes as the dependent variable, consistent with the other studies reported in this article. In separate Poisson regressions, we explored whether the number of abstentions and no votes, respectively, differ across experimental conditions. In the first model, we found no difference in the number of abstentions (p = 0.70) across the threshold-of-two condition (M = 1.22; SD = 1.30) and the threshold-of-one condition (M = 1.27; SD = 1.49). However, the number of no votes was lower (p = 0.04) in the threshold-of-two condition (M = 3.62; SD = 1.48) than in the threshold-of-one condition (M = 4.07; SD = 1.80).

Discussion. The results of Study 1C provided additional empirical support for H1, even when participants had the option of abstaining from the vote. The first three studies framed the voting rule in terms of yes votes—that is, the number of yes votes required to trigger the investment. Study 1D was designed to investigate whether our findings generalize to voting rules framed in terms of no votes.

Study 1D

Sample. Data were obtained from 232 study participants, 166 (or 71.55%) of whom were male. On average, participants were 35.66 years (SD = 11.13) old and had 12.57 years (SD = 9.65) of work experience. A total of 58 (or 25.00%) participants had first-hand experience investing in ventures. While 43 participants were

[9] https://osf.io/bnvmp/

students, most were employed adults, many of whom reported job functions in operations (19.05%) and research & development (12.17%) and job levels of manager (16.93%) and analyst/associate (14.81%).

Procedures. Participants were randomly assigned to either a threshold of three no votes (n = 111) or threshold of one no vote (n = 121) in this one-factor between-subjects design. Specifically, participants in the former condition were informed that "(i)f three or more of the team members vote no, your team will not invest in the specific venture" whereas participants in the latter condition learned that "(i)f just one team member votes no, your team will not invest in the specific venture." Based on our first hypothesis, we expected that the number of greenlighted projects should increase across these two conditions. The rest of the procedures mirrored those of Study 1B. The pre-registration, instruments, data, and log files for Study 1D are available on OSF.[10]

Results. We ran a Poisson regression to predict the number of yes votes based on the threshold condition (coded as 0 for the threshold of three no votes and 1 for the threshold of one no vote). Results revealed a statistically significant effect at p = 0.04. Participants in the threshold-of-one-no-vote condition supported 1.13 (95% confidence interval [*CI*], 1.01 to 1.27) times as many ventures (M = 5.26; SD = 1.55) as participants in the threshold-of-three-no-votes condition (M = 4.66; SD = 1.59); d = 0.39; 95% bootstrapped *CI*, 0.11 to 0.67.

Discussion. Study 1D provided evidence that the effect proposed in H1 can be generalized from voting thresholds framed in terms of yes votes to those framed in terms of no votes. The first four studies had only little variation in the number of group members, leaving the question of whether group size may affect the threshold effect open. Specifically, one might expect the threshold effect to be comparatively weaker among smaller groups. Study 1E's objective was to analyze if the threshold effect continues to hold when group size is systematically varied.

Study 1E

Sample. Considering the lack of control over group size discussed above, we decided to overrecruit and obtained data from 685 participants. Because the goal of this study was to systematically analyze the role of group size, we dropped 165 participants whose group size deviated from our study conditions of three and six group members,[11] for 510 usable responses. A total of 351 of these 510 participants (or 67.50%) were male. The mean age was 35.31 years (SD = 9.59), and the mean full-time work experience was 12.42 years (SD = 8.77). Of the

[10] https://osf.io/gcpw7/
[11] Results for the threshold effect are substantially similar when retaining these 165 responses.

participants, 148 (or 28.46%) had venture investment work experience, and 456 (or 87.69%) were not enrolled as full-time students. Among the frequently reported job functions were finance (18.98%) and operations (11.70%), and the most frequent job levels included manager (17.00%), owner (16.56%), and analyst/associate (15.01%).

Procedures. Participants were randomly assigned to one of the four experimental conditions in this 2 (threshold: 1 vs. 3 yes votes) × 2 (group size: 3 vs. 6) between-subjects design. Except for the group size, procedures mirrored those of Study 1B. The pre-registration, instruments, data, and log files for Study 1E are posted on OSF.[12]

Results. We first ran a Poisson regression predicting the number of yes votes based on the voting threshold alone and found an effect ($p = 0.004$). We then estimated a Poisson regression in which only the group size, coded as a dummy, predicted the number of yes votes, but failed to find an effect ($p = 0.24$). Finally, we conducted a Poisson regression with voting threshold, group size dummy, and their interaction included and found no clear evidence for an interactive effect ($p = 0.68$). (A factorial ANOVA produced highly comparable results.) We then conducted Poisson regressions in subgroups, which revealed an effect of the voting threshold both among groups of six ($p = 0.02$) and among groups of three ($p = 0.09$). Online Appendix G displays the condition means, further illustrating that the threshold matters for groups of both six and three, whereas no notable main effect of group size and no interaction of group size with threshold are discernible.

Discussion. The results from Study 1E offer additional support for H1 and demonstrate that the threshold effect is robust across different group sizes. Notably, we did not observe an interaction between voting threshold and group size. We can only speculate that the difference between three and six group members might not have been salient enough for participants to substantially alter their strategic voting behavior and encourage future research that employs stronger group size manipulations (e.g., contrasting groups of two vs. groups of ten). The first five studies had participants vote in isolation and without communicating with their teammates. While this approach to voting may not be uncommon in certain organizational settings, other organizational votes are preceded by a debate among committee members (e.g., Guo 2016). Study 6 was thus designed to examine whether the threshold effect is robust when allowing for pre-vote deliberation among teammates. Specifically, we manipulated both the voting threshold and the presence (vs. absence) of deliberation to examine whether deliberation might mute the threshold effect.

[12] https://osf.io/txkqp/

Study 1F

Due to high show-ups in the four seminar sessions in which we ran this experiment, a total of 623 individuals participated in Study 1F. Of these 623 participants, 423 (or 67.90%) were male. On average, participants were 36.85 years (*SD* = 11.93) old and had 14.23 years (*SD* = 10.79) of full-time work experience. A total of 182 (or 29.21%) participants had first-hand venture investment experience, and 521 (or 83.63%) were not enrolled as full-time students. Many participants worked in finance (14.20%) or research & development (12.48%) and held positions as owner (17.85%) or director (14.01%).

Procedures. Participants were assigned to one of four experimental conditions in this 2 (threshold: 1 vs. 3 yes votes) × 2 (deliberation: absent vs. present) between-subjects design. Like before, we manipulated the voting threshold at the group level using Qualtrics question randomization, while we manipulated deliberation at the seminar session level, such that two sessions excluded and two sessions included a pre-vote deliberation task. To minimize other differences between sessions, we scheduled all four at around the same time of the day and during weekdays; as usual, in Online Appendix E, we also report results while including a number of controls in the model. The pre-registration, instruments, data, and log files for Study 1F are available on OSF.[13]

Results. We started by estimating a Poisson regression that only contained the threshold condition as the predictor of the number of yes votes and found a main effect that was statistically significant at $p = 0.000$. We then ran another Poisson regression model that only included a deliberation dummy as predictor and found a negative effect ($p = 0.053$). Finally, we conducted a Poisson regression that included the threshold condition, the deliberation dummy, and their interaction. In line with our expectations, the threshold × deliberation interaction term had a negative coefficient ($p = 0.039$). (A comparable ANOVA yielded similar results.) Running Poisson regressions in subgroups revealed that the threshold effect was more pronounced when deliberation was absent ($p = 0.000$) and noticeably weaker when groups deliberated prior to their vote ($p = 0.099$). Online Appendix H visualizes the condition means. The figure demonstrates that the threshold effect matters for groups both without and with pre-vote deliberation, but it is comparatively stronger among the former, in line with a negative interaction effect.

Discussion. The findings from Study 1F offer greater insight into the role of pre-vote deliberation. While deliberation does attenuate the effect of the voting threshold (as demonstrated by the interaction term), it does

[13] https://osf.io/btvd2/

not erase it. Taken together, the first six studies demonstrate considerable robustness of the proposed threshold effect across a wide variety of settings. The studies that follow next further extend the investigation and proceed to offer evidence for a key mechanism underlying the threshold effect.

MEDIATION EFFECT STUDIES

Study 2A

Sample. We obtained data from 410 online seminar participants. Of these participants, 282 (or 68.78%) were male. The mean age of participants was 36.64 (*SD* = 9.23), and the mean work experience was 13.54 years (*SD* = 8.99). A total of 113 (or 27.56%) of the participants indicated they had first-hand experience with venture investing. Most of the participants (91.22%) were not enrolled as full-time students, and among them, many worked in finance (17.91%) or operations (13.90%) and reported a job position as manager (16.58%) or analyst/associate (15.51%).

Procedures. In this one-factorial between-subjects design, participants were randomly assigned to either a one-yes-vote (*n* = 200) or a three-yes-votes condition (*n* = 210). The procedures followed those outlined for Study 1B while adding survey questions regarding the way participants interpret the voting context in-between the presentation of the venture descriptions and the casting of votes. Specifically, to measure the mediator of inferred information, we included the following two items (anchored on a five-point answer scale ranging from 1 = strongly disagree to 5 = strongly agree): "My 'yes' vote only matters if my teammates had a negative opinion of the venture" and "If all of my teammates vote 'no,' my 'yes' vote can hurt my team's performance." To facilitate interpretation of the results, we reverse-coded both items before computing their average. The pre-registration, instruments, data, and log files for Study 2A can be accessed on OSF.[14]

Results. To further probe the main effect, we started by estimating a Poisson regression to predict the number of yes votes on the basis of the study condition (threshold of 1 or 3 yes votes). Just like before, we found evidence for the proposed threshold effect (p=0.000). Participants in the threshold of three condition greenlighted 1.28 (95% *CI*, 1.16 to 1.41) times as many investments (*M* = 4.48; *SD* = 1.25) as participants in the threshold of one condition (*M* = 3.49; *SD* = 1.52), for a medium-to-large effect size of *d* = 0.71; 95% bootstrapped *CI*, 0.50 to 0.93. We began testing H2 by running mediation bootstrapping tests (Preacher and Hayes 2004; the mediation tests were run with 5,000 bootstrap samples using the PROCESS macro in SPSS; Hayes, 2017, Model 4).

[14] https://osf.io/982ws/

Employing bootstrap mediation analysis to calculate the indirect effect has the advantage of greater statistical power without making the assumption of multivariate normality in the sampling distribution (Hayes 2017). If the 95% bias-corrected *CI* for the parameter estimate does not contain zero, then the respective indirect effect is evident, and mediation is established. Figure 1 shows that the *CI* for the indirect effect of the threshold condition on project support through inferred information does not contain zero. A higher voting threshold increases information inference, and information inference in turn increases project support. We also ran this model for each of the two mediator items separately, and results were very similar. Finally, we estimated a mediation model using the paramed routine in Stata (Emsley and Liu 2013) and again found evidence for an indirect effect.

---Insert Figure 1 about here---

Discussion. The results from Study 2A offer initial empirical support for the mediating role of inferred information in the voting threshold–project support link, while supplementary analyses suggest that threshold-dependent information inference shapes quality perceptions, which in turn explain voting behavior. In this traditional measurement-of-mediation design, we captured the focal mediator of inferred information via survey. However, since we measured (rather than manipulated) this variable, our ability to establish its causal effect is somewhat limited based on this design. Hence, consistent with recommendations by Spencer et al. (2005) and others, we complemented Study 2A's measurement-of-mediation approach with an experimental-causal-chain design in Study 2B, which examined a causal link from inferred information to project support, as predicted by H2.

Study 2B

Sample. We obtained data from a total of 401 online seminar attendees. Participant characteristics are comparable to those of the previously reported online studies, with 285 participants (71.07%) being male and a mean age of 34.69 years (*SD* = 8.32). On average, participants had 11.81 years (*SD* = 7.86) of work experience, and 98 of them (or 24.44%) had first-hand experience investing in ventures. Full-time student status was reported by 56 participants; many of the remaining 345 non-student participants worked in operations (17.39%) or finance (14.20%) and held job titles of manager (16.81%), senior manager (14.78%), owner (14.20%), or director (14.20%).

Procedures. Participants were randomly assigned to either a control (n = 205) or an information inference priming condition (n = 196) in this one-factor between-subjects design that held the voting threshold constant at a level of one yes vote. At this low threshold, we expected that greater information inference would

lead to fewer yes votes, consistent with our theoretical logic of participants inferring that no other team member must have voted "yes" for their own vote to become pivotal.

We followed the general procedures of Study 1B but added an essay-writing assignment right after participants read the venture descriptions and before they cast their votes. Employing an essay priming approach similar to Galinsky et al. (2003), we asked participants in the information inference condition to envision the situation in which the team invests in a venture only because of their yes vote. Specifically, we asked participants to elaborate on three issues related to this hypothetical situation: "(1) How many team members (including yourself) must have voted 'yes' for this situation to occur (…)? (2) What can you infer from these votes regarding the likely quality of the venture (…)? (3) Retrospectively, do you think it was a good choice to vote 'yes' in this situation?" Consistent with priming logic, the goal of this manipulation was to make information inference more salient among participants in this condition. Participants in the control condition were instead asked to write on a topic unrelated to the task. These participants elaborated their opinion about the merits of teaching entrepreneurship in business school. Online Appendix I shows the differences in instructions across priming conditions. The pre-registration, instruments, data, and log files for Study 2B have been uploaded to OSF.[15]

Results. To examine the effect of the essay manipulation, we ran a Poisson regression to predict the number of yes votes based on the priming condition (information inference vs. control) and identified an effect that was statistically significant at $p = 0.04$. Individuals in the information inference condition supported 0.90 (95% confidence interval [*CI*], 0.81 to 0.99) times as many ventures ($M = 3.45$; $SD = 1.36$) as participants in the control condition ($M = 3.84$; $SD = 1.41$); $d = 0.29$; 95% bootstrapped *CI*, 0.10 to 0.47.

Discussion. Study 2B showed that when the voting threshold is fixed at a low magnitude, pronounced information inference reduces the support for uncertain projects relative to a control condition, thus providing evidence for the causal effect of the information inference mechanism on voting behavior. In combination with Study 2B, these findings provide empirical support for H2. Although it is beyond the scope of this article to examine all possible mechanisms that could help explain the proposed threshold effect, one potential alternative explanation might have to do with perceptions of vote pivotality. In particular, different voting thresholds may lead to different perceived probabilities of casting the pivotal vote, and such variations in perceived pivotality may in turn affect

[15] https://osf.io/c7b3s/

voting behavior.[16] Study 2C was designed to explore this alternative mechanism.

Study 2C

Sample. Data collection concluded after data from 355 participants were obtained in three seminar sessions. A total of 247 (or 69.58%) of these participants were male. On average, participants were 34.69 years old (*SD* = 10.56) and had 12.06 years of work experience (*SD* = 10.84). A little less than a quarter (21.97%) of these participants had prior experience investing in venture. Only 18.03% of the participants were students. Many worked in finance (17.18%), operations (13.06%), or research & development (13.06%) and held job titles of analyst/associate (19.59%) or manager (14.78%).

Procedures. Participants were randomly assigned to either a one-yes-vote (*n* = 193) or a two-yes-votes (*n* = 162) threshold condition in this one-factorial between-subjects design. Procedures were identical to those of our mediation Study 2A, except that we replaced questions about information inference with an item capturing perceived pivotality. In particular, we built on the belief elicitation approach of Duffy and Tavits (2008) (also see Dittmann et al. 2014) and included the following question (anchored on a nine-point semantic differential scale with "Very unlikely" and "Very likely" as anchors): "Given this voting rule, how likely is it you will find yourself in a constellation in which your individual vote is going to make a difference for the joint team decision?" The pre-registration, instruments, data, and log files for Study 2C can be accessed on OSF.[17]

Results. As usual, we started by estimating the main effect of voting threshold on the number of yes votes in a Poisson regression and found further evidence for this effect ($p = 0.015$). Participants in the threshold of two condition supported 1.13 (95% *CI*, 1.02 to 1 1.25) times as many venture investments (*M* = 4.58; *SD* = 1.22) as participants in the threshold of one condition (*M* = 4.04; *SD* = 1.53), resulting in a medium effect size of *d* = 0.39; 95% bootstrapped *CI*, 0.19 to 0.58. To explore the potential mediating effect of perceived pivotality, we ran a mediation bootstrapping test with 5,000 bootstrap samples using the PROCESS macro in SPSS (Hayes 2017). Online Appendix J shows that the *CI* for the indirect effect in this model does contain zero. Neither did the threshold condition predict pivotality perceptions, nor did those perceptions explain voting behavior. A mediation model using the paramed routine in Stata (Emsley and Liu 2013) similarly failed to show support

[16] We thank one of our anonymous reviewers for sharing this intriguing idea.
[17] https://osf.io/p5ncz/

for a mediating effect of perceived pivotality.

Discussion. Results of Study 2C did not point to perceived pivotality playing a substantial role in explaining the effect of voting thresholds on voting behavior in our setting. Combined with the results of Studies 2A and 2B, this should raise confidence that it is indeed inferred information that is driving this effect. To further bolster this evidence, the last study was designed to investigate a boundary condition consistent with our strategic voting account and proposed in H3: the level of private information.

MODERATION EFFECT STUDY

Study 3

Sample. Participants in Study 3 were enrolled in an Executive MBA program and attended an entrepreneurship class on the creation of new business ventures. In total, 99 people took part in the paper-and-pencil study, of whom 73 (73.74% percent) were male. Two participants left individual investment decisions unanswered; we retained these participants under the assumption that they did not want to invest in the respective ventures (but results are essentially unchanged when assuming positive investment or dropping these participants). Participants were an average of 37.85 years old (SD = 3.95) and had an average of 15.14 years of full-time work experience (SD = 4.08), with 18.18% indicating they had some work experience related to venture investments.

Procedures. Study procedures resembled those of Study 1A, with the key difference being that Study 3 used a 2 (threshold: one versus two yes votes) × 2 (level of information: low versus high) between-by within-subjects design.[18] As in Study 1A, the voting threshold was manipulated by assigning participants to a threshold of either one yes vote (n = 50) or two yes votes (n = 49). Except for the details of how individual votes were aggregated at the team level, study materials were identical across the two threshold-level conditions. In addition, we also manipulated level of information as a within-subjects factor. Specifically, four randomly selected company profiles in each participant's handout contained 5–6 lines of blurred-out, unreadable text (low level of information), whereas five contained 5–6 lines of bold and underlined text (high level of information). Additional instructions provided with each low-level-of-information company profile informed participants: "Some of your team members have done additional research and therefore have more information on this company than you do. The

[18] The measures collected from participants were also identical across studies, with the only exception being that we also included a risk attitude measure in Study 3 (as explained further below) and obtained sex and age from student records (rather than through the post-task questionnaire).

information that is blurred is information that some of your teammates have, but you do not have." In contrast, each high-level-of-information company profile was preceded by the instructions: "You have done additional research on this company and therefore have more information on the company than your team members. Specifically, the information that is highlighted in the text (bold and underlined) is information that you have exclusively and that your teammates lack." Online Appendix K shows examples of the blurred and highlighted text used to manipulate the level of information. Except for one four-person team, all teams had five members (M = 4.96 participants, SD = 0.20), and all but four participants gave a correct response to the study comprehension question.[19] Instruments, data, and log files for Study 3 are publicly available on OSF.[20]

Results. Because individual company profiles differed in terms of information levels (the within-subjects variable), we moved to a trial-level analysis as our default analytic method and coded the dependent variable of investment as a dummy variable for each company. To account for the fact that individual trials were nested within participants, we used random-intercept logistic regression with participant as clustering variable (Rabe-Hesketh and Skrondal 2012). The model included voting threshold, level of information, and their interaction as predictors.

Results provided additional support for the proposed main effect (H1): the voting threshold had a positive impact on the propensity to invest (b = 1.51; SE = 0.33; z = 4.51; p = 0.00). Further, we found that the level of information positively affected the dependent variable (b = 2.74; SE = 0.31; z = 8.98; p = 0.00). Most importantly, the negative coefficient of the interaction term was consistent with the moderation effect proposed in H3 (b = -1.37; SE = 0.37; z = -3.71; p = 0.00). Figure 2 graphically illustrates the percentages of positive investment decisions averaged by condition. Subsample regression analyses revealed that the effect of the voting threshold was no longer present when focusing only on the five trials with high levels of information (b = 0.13; SE = 0.18; z = 0.70; p = 0.48), whereas this effect was positive when focusing on the four low-level-of-information trials (b = 1.79; SE = 0.47; z = 3.79; p = 0.00). Consequently, the level of information constitutes a critical boundary condition to the threshold-level effect.

---Insert Figure 2 about here---

[19] The comprehension question was the same as in Study 1A, and results remained virtually unchanged when dropping the four respondents who provided an incorrect response to this question. Likewise, results are very similar to those reported in the paper when including a control for team size in the analyses.
[20] https://osf.io/tjxqw/

Finally, to explore the alternative explanation that the observed voting-threshold effect might be driven by changes in risk attitude, Study 3 also captured participants' risk attitude using Holt and Laury's (2002) hypothetical lottery-choice instrument. A Poisson regression revealed that the count of risky lottery choices is virtually unaffected by the threshold condition ($b = 0.08$; $SE = 0.08$; $z = 1.02$; $p = 0.31$), thus alleviating concerns about out this alternative explanation.

Discussion. The results of Study 3 both replicate and extend our findings. Using a sample of Executive MBA participants, we again find a positive main effect of the voting threshold on investment, illustrating the robustness of this effect and generalizing our findings across various different populations (Bettis et al. 2016). In addition, Study 3 provides novel evidence on the role of the level of information as a moderator of the voting threshold–investment effect. In situations with low levels of information, the effect of the voting threshold on investment likelihood is more pronounced than in situations with high levels of information.

POST-HOC GROUP-LEVEL ANALYSES

Although the paper's hypotheses pertain to effects on individuals' voting behavior, we were curious whether the voting-threshold manipulation might also have downstream effects on decisions at the level of the organization. We thus pooled our data from Studies 1A and 3 (resulting in 239 participants and 50 groups), applied the respective voting rule to compute team-level decisions, and compared them across conditions.[21] Interestingly, a Poisson regression revealed that the number of team-level investment decisions was not statistically different across conditions at the conventional 5% level ($z = -1.68$; $p = 0.09$. This implies that redesigning an organization's aggregation structure in an effort to influence organizational decisions may not create the anticipated downstream effect, as the mechanics of the aggregation rule appear to be substantially countervailed by changes in individual voting behavior. We return to this insight in our General Discussion section.

To dig a bit deeper, we also computed "statistical counterfactuals" (e.g., Csaszar and Laureiro-Martínez 2018), applying the two-vote aggregation mechanism to the decisions of teams exposed to the one-vote-threshold (and vice versa) and thus allowing for a comparison between actual voting behavior and voting behavior unaffected by the threshold condition. The ensuing analyses show that the threshold effect appears to play out asymmetrically

[21] One participant was mistakenly provided with materials for the other study condition than his/her teammates. While this did not affect the individual-level analyses reported above, it precluded us from team-level aggregation, so we omitted this particular team from the post-hoc analyses.

at the team level. As we uniformly apply the two-vote-threshold aggregation mechanism to all teams, we see a notable difference in the resulting team decisions across experimental conditions, with more investments among teams in the two- than in the one-vote-threshold condition ($z = 2.63$; $p = 0.01$). That is, even when holding constant the aggregation mechanism itself, individual voting differences produce differences in outcomes, such that teams exposed to the higher threshold will engage in more investments. However, when we uniformly apply the one-vote-threshold aggregation mechanism to all teams, a difference between conditions does not materialize ($z = 1.34$; $p = 0.18$). These results suggest that the endogenous changes in individual behavior are particularly consequential at the team level when voters anticipate a low threshold. And more broadly, they underline our position that it is not tenable to treat aggregation rules and voting behavior as independent phenomena. Data and log files for these post-hoc group-level analyses are available on OSF.[22]

GENERAL DISCUSSION

To examine whether organizational structure has the hypothesized dual function of not just aggregating but also shaping individuals' voting behavior, we conducted a series of experiments among student, managerial, and executive populations. Results reveal that voting thresholds indeed influence individuals' votes, and substantially so. Specifically, a lower threshold renders people significantly more conservative, whereas a higher threshold causes them to be more liberal in their voting. In support of the robustness of this voting-threshold effect, we show that it holds across different threshold magnitudes, different group sizes, and in the presence of pre-vote deliberation. We also illuminate the theoretical mechanism and offer empirical support for information inference underlying the observed threshold effect, in line with our strategic voting account. Finally, we show that the effect is contingent on individuals' level of private information about the underlying decision, with increasing levels of information "turning off" this effect. In sum, our article highlights the dual function of thresholds, it helps isolate the theoretical mechanism underlying the threshold effect while ruling out an alternative explanation, and it establishes an important bridge between research on jury decisions in political science and on information aggregation in organization theory.

Organizational Structure: Aggregating and Shaping

Our study adds to the reinvigorated line of research on the effects of organizational structure (Bernstein et al. 2016, Burton and Obel 2018, Joseph and Gaba 2020, Lee 2022, Puranam 2018). The specific stream we

[22] https://osf.io/td42z/

contribute to is scholarship on the aggregation of information from organizational members (Christensen et al. forthcoming, Christensen and Knudsen 2010, Csaszar 2013, Csaszar and Eggers 2013, Knudsen and Levinthal 2007, Piezunka et al. 2022). Our goal in this manuscript is to address repeated calls for "a theory of aggregation that explains how individuals combine their behavior to produce collective outcomes" (Freeman 1999, p. 175).

Our first and primary contribution is to draw attention to the *dual* role of organizational structure, which both (1) *aggregates* and (2) *shapes* individual votes. We elaborate and provide empirical evidence for the second—so far largely neglected—function of organizational aggregation rules: their shaping of individual votes. This notion resonates with recent research insisting that individuals' behavior is fundamentally affected by the organizational structures surrounding them. For instance, Piezunka et al. (2022) illustrate in a simulation that organizational structure influences individuals' votes, as it co-determines what kind of feedback people receive and thus what they learn. In this study, we illustrate that even in the absence of learning (and thus even in the short term), organizational structure shapes individual votes. The underlying mechanism is that organizational members take organizational structure as an important clue—and use it to adjust their reservation level.

By illustrating how organizational structure shapes individual voting behavior, we relax a key assumption in prior research: the exogeneity of individual voting behavior. Earlier work on organizational aggregation, often deploying formal modeling, tended to treat individual voting behavior as exogenous and independent of structure. Our article refutes the exogeneity assumption and emphasizes that neither decision-making structures nor individual voting behavior should be studied in isolation, as they are deeply intertwined. Our experimental approach allows us to endogenize individuals' votes and to demonstrate that these votes are a function of organizational structure.

The effect that aggregation rules have in shaping voting behavior is particularly remarkable given that it *countervails* their effect in combining votes into an organizational decision. While a smaller threshold lowers the bar for a project to be greenlit at the organizational level, it also reduces organizational members' tendency to support that project. The two functions of combining and shaping are thus in direct opposition. As a result, organizations that deliberately adopt a new voting threshold may not achieve the desired effect, as the change in individual voting behavior may undermine the structural change.

This insight—that is, that aggregation rules have two countervailing functions—is of high relevance for research on organizational aggregation. Studying the effect of aggregation rules on organizational decision making

without taking into account how aggregation rules shape individuals' voting behavior may result in inaccurate estimates of the effect of aggregation rules on organizational decision making. It is thus critical to account for the macro-to-micro implications of organizational structures to capture their true effects (Gavetti et al. 2007, Greve 2013, Keum and See 2017, Lee and Csaszar 2020). Consider an investigation examining strategic decision making while only studying individual choices and ignoring aggregation structures. Such a study would be unable to account for the fact that actors in organizations commonly make decisions in committees and, in doing so, engage in strategic behavior that can differ markedly from how they would behave in isolation. Conversely, a study focusing on aggregation structures while bracketing individual behavior may overestimate how these structures will affect organizational decisions. As our findings show, the micro-level effect can attenuate or even offset the macro-level effect. As such, our investigation directly supports the microfoundations perspective, to which we will turn below.

Organizational Selection: A Decision by Organizations—and Their Members

A rich body of work has examined organizations' selection of strategic projects (Bardolet et al. 2011, Boudreau et al. 2016, Criscuolo et al. 2017, Lane et al. 2022, Piezunka and Dahlander 2015, Sengul et al. 2019). While one key goal of many organizations may be to minimize the frequency of false positives and false negatives (Sharapov and Dahlander 2022), the number of projects being greenlit is a meaningful outcome variable in of itself, as organizations find themselves either too liberal or too conservative in their project selection (Boudreau et al. 2016, Piezunka and Dahlander 2015). Organizational structure appears like a plausible lever for adjusting an organization's selectivity. The introductory example illustrates how the venture capital firm DFJ considered itself too conservative and therefore adapted its structure in an attempt to correct for this problem. Our study, however, shows that such a structural change may be in vain if it is overturned by changes in individuals' decisions.

Microfoundations: From the Organization to Individuals and Back

The study of microfoundations has recently witnessed unprecedented interest and continued growth in organizational theory and strategy alike, to the extent that we can speak of a microfoundations movement that is fundamentally changing the landscape of these formerly purely macro-level fields (Felin et al. 2015, Zucker and Schilke 2020). Microfoundational research seeks to explain how relations between macro variables (such as organizations' structures and decisions) are mediated by micro cognitions and actions (such as agents' inferred information and voting behavior) (Barney and Felin 2013, Cowen et al. 2022).

At the core of the microfoundational approach is the question of what it means for organizations to act, given that they are rarely represented by only one individual (Bromley and Sharkey 2017, Halgin et al. 2018, King et al. 2010, Schilke and Lumineau 2022, Steele and King 2011). That is, how does collective organizational action emerge and take on forms that cannot be explained by taking the simple average among the organization's members? Our article provides one important answer to this question. We show that aggregation structures transform how organizational members act, in a way that deviates from how they would act either in isolation or under different aggregation structures. As such, on the one hand, our article offers support for the notion that there is something fundamentally supraindividual and uniquely organizational that drives the decision making of organizations (i.e., voting structures), demonstrating that organizations are indeed much more than the simple sum of their parts. On the other hand, our findings qualify the extreme view that individuals do not matter, as we show that their behavior may in fact go against and counteract tendencies inherent in the organizational structure. Overall, then, we contribute to elaborating a more balanced middle-ground perspective (Schilke 2018) that transcends the conventional dichotomy of structure vs. agency and unpacks the mechanisms through which the two interact. Our approach highlights the importance of organizations' voting rules as unique properties that structure the decision process (as briefly alluded to by Coleman 1974, pp. 39-41) while doing justice to the important role of individual behavior in organizational decision making. Whereas earlier research on organizational actorhood primarily traced an organization's character back to its identity and goals (e.g., King et al. 2010, Schilke 2018), we suggest that organizational structures—and in particular aggregation structures—should be added to the list of uniquely organizational features that explain the nature of the actions taken by an organization.

In addition to speaking to the issue of organizational actorhood, our research may also contribute specifically to elucidating the microfoundations of dynamic capabilities—that is, firms' capacity to purposefully create, extend, or modify their resource base (Helfat et al. 2007, Schilke 2014). Organizational design has been recognized as a critical enabler of dynamic capabilities (Felin and Powell 2016), and more specifically, the decision rules that organizations employ have been linked to the degree to which firms are able to seize opportunities (Teece 2007). Nevertheless, more fine-grained theorizing about specific types of decision rules and empirical research directly linking such rules to concrete organizational investment decisions have been largely absent in this literature (also see Felin et al. 2015, p. 616, Schilke et al. 2018, p. 415). As such, dynamic capabilities scholars

should find it insightful that voting thresholds significantly impact investment decisions and, by implication, the extent to which firms routinely change their resource base. Put differently, voting rules may qualify as a central (albeit underappreciated) microfoundation of firms' dynamic capabilities (Teece 2007), a notion that contributes to a more refined picture of how specifically the complex concept of dynamic capabilities (which has been criticized as being vague and elusive; Arend and Bromiley 2009) manifests inside firms.

Strategic Voting: in Political Science and Organization Theory

Another key contribution of our investigation lies in bringing the concept of strategic voting from political science to organization theory. Our investigation thus serves as a bridge between the political science literature on strategic voting and research on organization design and information aggregation in strategic management. Beyond merely "importing" the concept of strategic voting, our research also helps advance knowledge of strategic voting. There is a heated ongoing debate in political science on whether strategic or sincere voting offers a better approximation of voters' actual behavior (e.g., Esponda and Vespa 2014, Kawai and Watanabe 2013). Our investigation adds to this issue in several respects.

First, our article provides new evidence that strategic voting indeed provides a good approximation of voters' actual behavior. While political science has primarily focused its analysis on jury and electoral votes and collected experimental data from a student population (e.g., Guarnaschelli et al. 2000), our investigation establishes the generalizability to and robustness of the threshold effect across a variety of organizational settings and a broader set of participants. The effect holds across different threshold magnitudes, different group sizes, different threshold frames, when voters have the option to abstain, and in the presence of pre-vote deliberation. Therefore, we demonstrate that the threshold effect is anything but narrow (as surmised by Margolis 2001) and applies to different organizational voting contexts.

Second, our investigation endeavors to isolate the information-inference mechanism that is central to the process by which strategic votes are cast. While political scientists have made much progress in examining different voting rules (Palfrey 2016), they have paid relatively little empirical attention to the precise cognitive mechanisms that explain these rules' effects (see Duffy and Tavits 2008). Our examination of H2 (Studies 2A-2C) and H3 (Study 3) provides concrete evidence for the information-inference mechanism that is at the heart of the strategic voting account but has previously been merely assumed rather than demonstrated. Beyond their theoretical value, these

insights also have important practical implications that go beyond prior work. For example, our research helps to illuminate when or by whom strategic voting should most likely be expected. The degree to which actors engage in strategic voting and update their beliefs depends on their own information (with more private information leading to less engagement in strategic voting) as well as the information of their peers (with better informed peers leading to more engagement in strategic voting). Taken together, our article contributes to an improved understanding of how inferred information serves as a key process explaining divergent outcomes across voting regimes, thus adding important insights to the burgeoning voting literature in organization theory (e.g., Aadland et al. 2019, Andrei et al. forthcoming, Krause et al. 2014, Rao and Sivakumar 1999).

Managerial Implications

While drawing managerial implications from experimental research should be treated with caution (Bitektine et al. 2018), the results reported here speak to the roles of aggregation structures as organizational design tools that are relatively easy for firms to change but whose effects are not as straightforward as they may seem. For instance, changing the voting threshold may not have much effect at the organizational level unless committee members possess relatively high levels of information about the underlying investments. However, especially in settings where managers face significant information asymmetries and are thus uncertain about the prospects of an investment, they may vote strategically, in the hope that their colleagues might be better informed. As such, one implication of our findings is that firms trying to make strategic changes by adjusting voting thresholds may be well advised to also implement deliberation routines through which each voting member's level of information is made transparent to the group and the better-informed individuals are given the opportunity to share their insights before votes are collected. Another implication is that the transparency of the voting rule constitutes a relevant design lever in of itself. Our theory suggests that knowledge of the voting threshold induces strategic voting. If strategic voting is not desired, it may be decided not to disclose the voting threshold to the committee members. More generally, an organization may consider adjusting its structure without communicating the change to its members. If an organization strives to change its selection policy, and a change of the organizational structure will have the two countervailing effects demonstrated in this article (i.e., the mechanical aggregation and the shaping of votes), the organization may experience the first effect but not the second if it manages to conceal the change.

Limitations and Future Research

This investigation is subject to several limitations that result from our methodological and conceptual choices. Clearly, our study constitutes only an initial step in making voting a central issue in organization theory, and future research is urgently needed to continue this project and elucidate organizational voting in the various forms it can take. Methodologically, we relied on experiments, making it important to acknowledge that findings are generalizable only via theory rather than being directly transferable to the field (Zelditch 1980). Several simplifying assumptions are necessary to bring organizational decision making to the laboratory, and it is important to be transparent about the resulting boundary conditions. Nonetheless, there is a central place for experiments in organization theory (Bitektine et al. 2022), and more and more scholars are making use of experimental methods in order to shed light on the cross-level dynamics in organizational decision making (e.g., Billinger et al. 2022, Di Stefano and Micheli forthcoming, Klingebiel 2022, Molina et al. forthcoming, Raveendran et al. 2016, Schilke 2018), a movement that will only continue to grow.

It is also crucial to note that the voting structure we study in this article characterizes only one particular form of information aggregation (see, for instance, the taxonomy of information aggregation structures proposed by Piezunka et al. (2022)). Organizations may adopt other kind of structures; for example, they may weigh individual members' votes differently and/or give certain members veto rights (Phadnis et al. 2015). Future research may explore how other organizational structures (e.g., structures that are more hierarchical or that allow individuals to share more granular information; Lee and Csaszar 2020) affect organizational decision making.

Our study builds on a set of additional conceptual assumptions that point to fruitful avenues for further generalization and assessment in future research. Most crucially, for our findings to hold, decision makers must be aware of the aggregation structure (which we accomplished in our experiments by prominently highlighting the voting threshold before the participants made their decisions). However, it is possible that members of an organization may not be fully aware of the voting regime and thus may not take it into account when voting. Organizations may even gather their members' votes without having established an a priori voting regime at all. In such cases, the effects observed here may be absent or attenuated—an issue to be explored in future research. Moreover, it is worth highlighting that our experiments randomly assigned voting thresholds, and results might differ if teams self-select into different structures (Gibbons et al. forthcoming) or if participants were given specific reasons why the organization uses a particular structure. Future work may endogenize structure itself, and examine

if and how the effect of voting thresholds changes when organizational members themselves choose a voting threshold for their organization rather than being assigned one. Also, the individuals in our setting were incentivized to be concerned about the overall organizational outcome, consistent with our assumption of homogeneous preferences for optimal selection. The degree to which managers care about the organizational implications of their actions may vary across managers and organizations (Eisenhardt 1989, Garg 2013), and representatives from different departments may have different preferences when it comes to strategy making. Future research should thus examine organizational voting dynamics while allowing for divergent utility functions among voters. Such an approach would open the door to considering the possibility of misaligned voting (Kawai and Watanabe 2013), whereby actors deliberately vote for an option that they don't view as the optimal choice for the organization. Another assumption we made pertains to votes being cast simultaneously in a one-shot election. Voters' considerations and behavior might differ if votes were cast over several consecutive stages (Coughlan 2000). Our account also assumed that the focal decision maker engages in strategic voting while believing that others vote sincerely—that is, in direct accordance with their private information regarding project quality. This standard assumption in the strategic voting literature is highly aligned with the cognitive realities of voting (Ladha et al. 1999). While mounting empirical evidence suggests that decision makers can indeed be expected to conceive of the pivotal vote constellation and condition their vote accordingly (Palfrey 2016), it is less realistic to assume they will also go through the cognitively daunting task of envisioning other voters' possibly going against their private information, which would require a highly sophisticated understanding of Bayesian probabilities that is rather uncharacteristic of human thought processes in risky choice (Camerer et al. 2015, Ladha et al. 1999). Nonetheless, future research should investigate the effect of making actors aware of the fact that others might be voting strategically (Myatt 2007), which may in turn reduce the informativeness of the voting threshold. Motivated by the real-world case of the venture capital firm DFJ, we contrasted regimes with thresholds of one vs. two vs. three yes votes, but organizations may use higher thresholds than that. It is clear that we need further investigations to generalize our findings to other voting thresholds and different voting rules. Moreover, although our investigation moves beyond the common practice of using either undergraduates or crowdsourced participants for our experiments, MBA and Executive MBA students and online seminar participants may not be a representative sample of the population of venture capitalists, potentially limiting the generalizability of our research.

Our research has zoomed in on the role of information inference as an underlying *mechanism* explaining the effect of voting thresholds on voting behavior. While our article offers considerable evidence in support of this mechanism, there could certainly be others at play in addition. We explored one alternative explanation--perceived pivotality— in Study 2C and found no support for it, but the lack of a statistically significant effect of course does not establish the absence of an effect, so future research might use other approaches to further examine perceived pivotality while also broadening their scope and consider others. Perceived accountability might be one plausible candidate, considering that a higher threshold may translate into a diffusion of responsibility (O'Connor 1997), which may make actors feel more comfortable voting in favor of a risky project when the threshold is high.

We also urge future research to tease out additional theoretical *contingencies* that condition the link between voting thresholds and voting behavior. Our investigation highlights the important role of the level of private information, but future research should venture out to establish additional moderators. For instance, even though we found no evidence for perceived pivotality as a mediating mechanism (Study 2C), this factor could plausibly play a relevant role as a moderator: actors may invest more cognitive resources in information inference when they believe it is likely they will affect the vote, which should strengthen the proposed threshold effect. In addition, future research may explore how characteristics of the group of organizational members (e.g., the diversity of the group, fault lines between organizational members, interaction routines) may affect the linkage between voting structure and behavior.

We also encourage future research to examine additional *outcomes* of alternative voting structures. A particularly promising avenue is to examine the effect of alternative voting thresholds on selection accuracy—that is, the frequency of errors of omission and commission at both the individual and organizational level (Csaszar 2013, Keum and See 2017). Ideally, actors will want to select those alternatives that will help the organization succeed while staying away from those that are likely to fail, thus avoiding under- and overinvestment. Supplementary analyses (which are available upon request) indicate that higher voting thresholds reduce individuals' tendencies to make errors of omission while increasing their tendency to make errors of commission. As a result, voting thresholds appear to have important implications for an organization's level of under- and overinvestment. Ideally, future work will track the causal chain from the adopted voting threshold via individuals'

tendency to vote yes or no to organizational errors of omission and commission—and ultimately to organizational performance. Along similar lines, we see great value in exploring the relationship between voting thresholds and aspirations, adding to our knowledge of the antecedents to organizational aspirations (e.g., Keum and Eggers 2018).

For all of these reasons, we hope that our study will spark further research that expands on our findings. While we see significant opportunity for further experimental research, there is also a clear need for studies using complementary methods in order to shed more light on the complex and dynamic process through which voting unfolds in organizations. Most notably, we see much promise in accessing new archival data sources that enable investigations of aggregation structures' cross-level effects. For example, to coordinate their voting, organizations rely on professional service firms such as Qualtrics or Slack. Gaining access to these firms' data would allow a study of cross-level effects in a wide population of organizations. Other research may be able to leverage variance in voting thresholds across decisions within the same organization.

CONCLUDING REMARKS

This article offers valuable new insights into how aggregation structures shape information inference and voting behavior—and more broadly, decision making—in organizations. Most prior research on organizational aggregation structures focused on the bottom-up process of transforming the votes of individual agents into an organizational decision while taking individual voting behavior as a given. By contrast, we point to the top-down process through which aggregation structures go far beyond merely combining the votes of individual agents and instead play an active role in molding them. Individual agents assume center stage in this line of inquiry that the current investigation aims to bolster, thus laying further groundwork for a microfoundational agenda of organizational design research that considers structural and individual dynamics simultaneously in order to account for their interactions.

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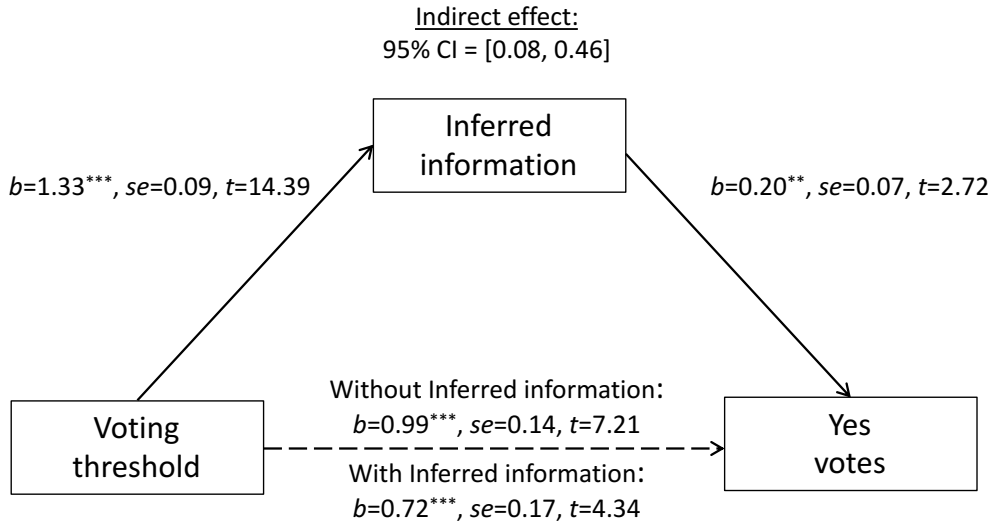
TABLES AND FIGURES

Table 1: Study Overview

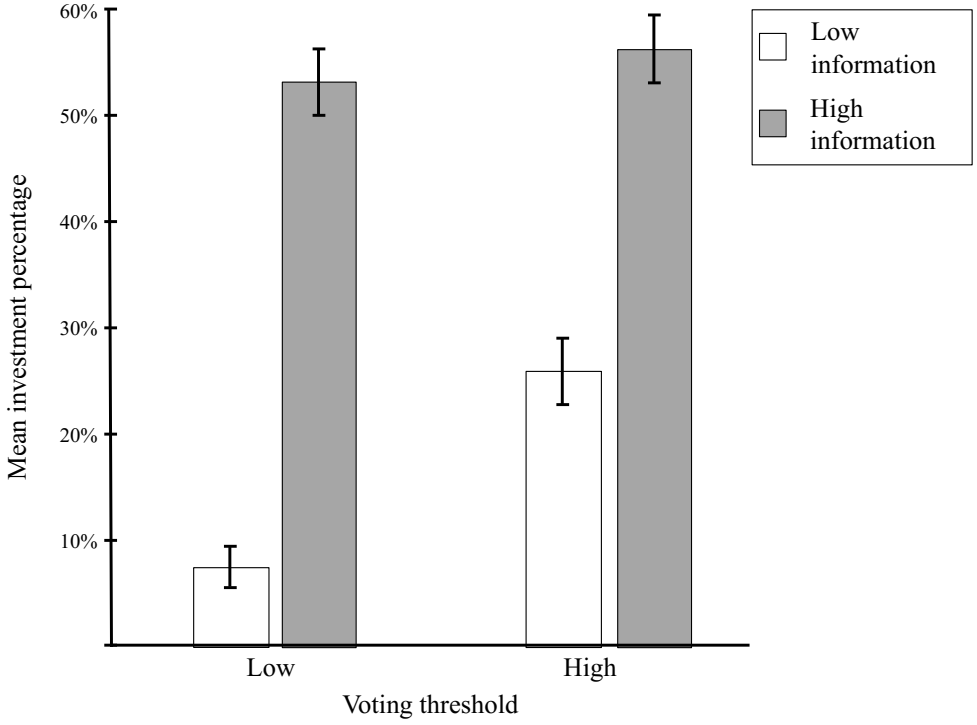
Study	Hypothesis addressed	Type of effect	Specific purpose	Experimental manipulation(s)	Sample	Key finding
1A	H1	Main effect	Show that voting threshold affects voting behavior	Voting threshold (1 vs. 2 yes votes)	140 MBA students	A higher voting threshold increases people's tendency to vote yes on a project
1B	H1	Main effect	Demonstrate robustness to other thresholds	Voting threshold (1 vs. 2 vs. 3 yes votes)	351 online seminar participants	Results of Study 1A generalize to a threshold of 3
1C	H1	Main effect	Demonstrate robustness to possibility to abstain	Voting threshold (1 vs. 2 yes votes)	328 online seminar participants	Results of Study 1A generalize to situations in which voters may abstain
1D	H1	Main effect	Demonstrate robustness to 'no' vote framing	Voting threshold (1 vs. 3 no votes)	232 online seminar participants	Results of Study 1A generalize to situations in which the threshold is framed in terms of 'no' votes
1E	H1	Main effect	Demonstrate robustness to other group sizes	Voting threshold (1 vs. 2 yes votes) × Group size (3 vs. 6 members)	520 online seminar participants	Results of Study 1A generalize to groups of 3 and 6 members
1F	H1	Main effect	Demonstrate robustness of mediation effect in a causal chain design	Voting threshold (1 vs. 3 yes votes) × Deliberation (absent vs. present)	623 online seminar participants	Pre-vote group deliberation attenuates the threshold effect
2A	H2	Mediating effect	Show evidence for mediation	Voting threshold (1 vs. 3 yes votes)	410 online seminar participants	Inferred information mediates the threshold effect
2B	H2	Mediating effect	Show evidence for mediation	Priming (information inference vs. control)	401 online seminar participants	Inferred information affects people's tendency to vote yes on a project
2C	H2	Mediating effect	Explore alternative mediating mechanism	Voting threshold (1 vs. 2 yes votes)	355 online seminar participants	Perceived pivotality does not appear to mediate the threshold effect
3	H3	Moderating effect	Show evidence for boundary condition	Voting threshold (1 vs. 2 yes votes) × Level of information (low vs. high)	99 Executive MBA students	The threshold effect goes away when people are highly informed about a project, but is amplified when they are poorly informed

Figure 1: Mediation Model, Study 2A



Notes: n = 410. Results from PROCESS model are shown. The statistics directly above the dashed arrow indicate the direct effect of voting threshold on yes votes, not accounting for the mediator. The statistics below the dashed arrow indicate the direct effect of voting threshold on yes votes, with the mediator included in the regression model. Asterisks denote coefficients at different significance levels ($^{\dagger}p < 0.10$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$; two-tailed tests).

Figure 2: Mean Investment Percentage by Condition, Study 3



Note: Error bars represent standard errors.

ONLINE APPENDICES

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Note: All instruments used for the studies are provided on OSF (links are included in the article). For face-to-face studies, we uploaded the materials handed out to study participants, and for online studies, we provided the Qualtrics surveys both as pdf and qsf files, the latter of which can be directly imported to Qualtrics to reproduce our study instruments. Our OSF depositories also include our data in xls format as well as Stata log files for all studies and SPSS outputs for the PROCESS analyses in Studies 2A and 2C.

Online Appendix A: Nine Company Dossiers Used in the Experiments and Company Outcomes

ForeignDigital is a platform that enables international money transfers via Bitcoin exchanges.[1] The model involves opening a local Bitcoin exchange in a new country where ForeignDigital will partner with local money transfer businesses. The latter are not involved in the Bitcoin-to-local-currency transactions, but they receive a 50% cut of ForeignDigital's fees in exchange for having the required licenses and relationships with regulators.

The company's co-founders previously sold an analytics-based company, filmed documentaries, founded a start-up house, and started a bitcoin mining operation called CoinHarvest. While working on the last of these ventures, the duo considered creating a legal platform for building Bitcoin exchanges and settled on the remittance market.

The company's goal is to open shop in India within the next three months and expand into six additional countries within nine months. The World Bank estimates that migrants will send approximately US$515 billion, ten times the amount of the US's budget for foreign aid, to relatives in developing countries by 2015. ForeignDigital aims to disrupt the remittance industry by allowing its user to send money quickly, inexpensively and legally via Bitcoin.

ForeignDigital's current main competitors are Coinbase, Bitpay and Bitnet. The company says it will take a commission of less than 1%. The average fee on remittances is 9% of the transaction cost, and some banks charge additional "lifting" fees of up to 5% when someone wants to turn the remittance into cash.

[1] Bitcoin is a new currency created in 2009 by an unknown person using the alias Satoshi Nakamoto. Transactions are made with no middlemen (i.e., no banks). There are no transaction fees and no need to give your real name.

LogistiX was founded in September 2012 by a UC Berkley graduate who had previously helped source and vet VC/early stage venture funds and start-ups for the investment firm, RAA Ventures, together with an ex-Google employee.

LogistiX is an application programming interface (API) that connects app developers to shipping carriers to automate shipping processes for e-commerce. In the past integrating with carriers has been a lengthy process for developers and required substantial effort since carriers use different standards which tend to change frequently. LogistiX works with all major US shipping companies, including USPS, UPS and FedEx. It gives businesses access to commercial USPS rates. It accommodates international shipments and automatically completes customs forms. The API requires no additional development and offers helper libraries in Python, PHP, Ruby, Node.js, Java and other languages. LogistiX's clients include Many Labs and Automatic. In the US, the shipping industry is estimated to be worth US$ 26 billion per year.

The company makes money by charging 5 US-cent for each shipping label created via its API. LogistiX first launched beta in September 2012 attracting approximately 500 sign-ups instantly. Over its first seven months of operations, LogistiX's revenue grew 179% month on month with 70,000 shipments made. In June 2013 it had 1,000 customers, with revenue and the number of packages sent doubling every month.

LogistiX's competitors include Postmaster and ShipHawk. LogistiX's goal is to differentiate itself and gain market share by specialising in first-rate customer support with response times of less than 10 minutes.

FashionStar

A former Partner and Associate at the private technology investment bank, Ridgecrest Partners co-founded FashionStar in May 2012.

The company aims to be the Netflix or Airbnb for fast fashion. Users sign up for a subscription with FashionStar for US$49 per month and complete a style profile. The company's stylists (algorithms) select looks and fashions based on the user's profile information. Each month, three garments and two accessories are sent to the customer. They can keep the outfit for an unlimited time until they tire of it and then return it in exchange for a new outfit.
Customers can also buy clothing items permanently at discounted prices.

The "FashionComets" (the bags sent in the mail) can be returned free of charge and as often as desired. Special requests can be made to customise the bags – just tops, just accessories, etc. The fashion items are produced under FashionStar's own label that is sourced in Asia, the Middle East and Latin America. The reused items are laundered and quality-checked after each use.
Customers can also purchase an insurance plan that will cover any stains or tears on returned clothing.

FashionStar is the only start-up to tackle this space in the USA – JewelMint and RockBox offer a similar subscription model but focus only on jewellery.
FashionStar had US$70,000 in revenues during its first 15 months, with 30% month on month growth and a 93% retention rate. Industry sales of women's clothing in the US were estimated at more than US$110 billion in 2011.

CaseBox

CaseBox was founded in June 2010. The founding team is comprised of a serial entrepreneur, a senior industrial designer and a former product manager at a mobile app development company. CaseBox creates a variety of accessories - most notably, their flagship product "Lever" an iPhone case that accommodates interchangeable back plates for the phone. It is inspired by watch clasps with a metal latch that allows the user to fit the frame around the phone which is then clicked into place, thus avoiding damage from snapping or pulling the case.
Lever is available with a plain back plate or with a card holder that fits five credit cards.

Since its creation in August 2011 CaseBox has enjoyed sales of US$2.5 million. The company is looking to sell US$7.5 million in 2013 with its Lever product.
The idea is for the case to be personalized to the customer's needs and to replace wallets. The company is targeting a market of 750 million potential customers with a price of US$40 per case. Worldwide smartphone accessory sales amounted to US$20 billion in 2012.

Other players in the accessory space include OtterBox, Case-Mate and Cynett. Whereas these companies' main concern with cases is protection, the Lever product also focuses on personal customization. CaseBox has partnered with other companies, including Zazzle and COLOURlovers, to facilitate such customization. CaseBox also offer

files for 3D printing back plates that work as kickstands and bicycle mounts. In addition, the CaseBox team is working on back plates with a headphone cord wrap and battery pack.

DeliverIt was founded in February 2013 by four Stanford students who had previously worked for Facebook, Square, Vevo and eBay.

It is an on-demand food delivery service. DeliverIt differs from its competitors such as Seamless and GrubHub who only offer an app to order food from restaurants that have their own delivery services. In contrast, DeliverIt employs its own drivers to make the delivery, thus widening the scope of restaurants where food can be ordered from. DeliverIt's business model caters largely for suburbs and college towns. When an order is placed through the DeliverIt system it is processed through an iPad that is installed in the partnering restaurant. The delivery drivers use a logistics system created by DeliverIt.

DeliverIt charges the customer US$6 per delivery regardless of order size and takes a cut of restaurant revenues but it does not inflate order prices. As of today, DeliverIt operates for lunch from 11.45am to 1.30pm and dinner from 5.30 to 9.00pm, seven days a week. It delivers food from 50 restaurants in the Palo Alto, Stanford, Menlo Park, Los Altos, and the Mountain View area. By August 2013 the company had delivered 3,500 orders with an average waiting time of 44 minutes. The company's weekly orders have increased by 31%, with US$1.5 million generated in annualised restaurant sales. In 2012 US-Americans spent a total of US$119.7 billion on takeaway meals, US$61 billion on drive- through, and approximately US$14 billion on delivered meals.

CoordinateMe is an app designed to streamline communication and task management during events. Prior to an event, users download CoordinateMe and create a login for the system. After approval from the administrator, each user can input basic contact information, including phone number and email, for one-to-one contact. The app uses a passive news feed, so people can check in to see what is happening but only receive notifications when they are tagged in a post. Once in CoordinateMe, the user can view posts, tasks, and participants and upload documents. Key features include the ability to track and manage tasks, as well as to assign tasks to specific people. Others who view the task can select "Grab Task," and then update the status to either "In Progress" or "Completed."

CoordinateMe targets events with thousands of attendees in the USA. They are also testing the app in other industries, such as hotel resorts, construction, security forces and airlines. The market size of the global event management software market in 2012 was US$29 billion and is expected to grow between 2.9% and 4.5% annually in coming years. The company's competition includes Yammer, Chatter.com and Convo. The main element that differentiates CoordinateMe from its competitors is the mobile focus of its coordination tools.

The company's two co-founders formerly worked as manager at the White House Executive Office and as co-founder and director of the trade federation for commercial spaceflight companies.

To date, CoordinateMe has gone from invitation-only to public beta. According to the founders, the company is

already profitable, having signed one large, US$150,000 per year contract.

GlassSpace produces augmented reality glasses that allow users to interact with virtual objects in real life. The GlassSpace software creates virtual 3D holograms that users can see through the GlassSpace glasses. These holograms are overlaid on and interact with objects in the real world; thus, users can construct and manipulate virtual objects with their hands. The virtual objects users create can be printed with 3D printers such as Makerbot. As such, the product removes the spatial constraints associated with computing and has potential applications in a wide variety of fields, including commerce, education, healthcare, architecture, product design, entertainment, and gaming. The glasses currently support Chess, Laser Tag, and Voxel Editor and feature a MineCraft simulator.

When the company launched GlassSpace's augmented reality glasses on Kickstarter in May, they almost doubled their original campaign goal of US$100,000. Since an early version of the glasses began pre-orders recently for US$667, they have racked up sales of US$500,000. The market for immersive augmented reality devices is expected to grow at a rate of 15.18% per year from 2013 to 2018, reaching US$1.06 billion in 2018.

GlassSpace was founded in December 2012 by a former Columbia University student and a PhD graduate in Computer Engineering from Toronto University who specialized in 3D sensing and 3D video processing. A renowned industry expert later joined the duo as their lead scientist. The company recruited several advisors, including a well-known augmented reality pioneer. In January 2013, GlassSpace announced a partnership with Epson. GlassSpace's competitors in the augmented reality wearables space are companies such as Vuzix and Recon Instruments.

EduAnalytics's goal is to bring data analytics to every school in the US by powering polls of students, teachers, and parents to improve school performance. EduAnalytics's products currently range from surveys and data analysis to online platforms that teachers can customize for their classes. They also offer a comprehensive toolkit of teaching tips and best practices.

EduAnalytics is using proprietary technology optimized for delivering and analysing surveys in K12-school environments. However, according to its founders, the most valuable service that EduAnalytics offers is supporting schools in creating surveys that provide actionable insights.

The three co-founders started the venture in their junior year at Yale, while helping school districts in the New Haven region with data analytics. Their long- term plan is to compile a national dataset of education data and use big data tools to help schools improve their performance.

By August 2013, 3,600 schools were signed up and are paying annual contracts to the venture at a rate of US$10 per student per month. Organisations such as Teach America are participating. The company currently has US$500,000 in annual revenues. In the US, 41 states have declared long-term mandates regarding the data that EduAnalytics can use. Currently the company is present in 225 districts and estimates that by September 2013 it will bypass US$1 million in recurring revenues. The total enrolment in US elementary and secondary schools in 2012 was 54.842

million, and it is expected to rise to 57.023 million in 2023. EduAnalytics's competitors in the K-12 data analytics market include Learnsprout, Clever, Kickboard, Teachboost, Alma, and junyo.

CustomLaundry was founded by two computer sciences graduates from Stanford with the idea of creating an Uber for laundry services. CustomLaundry allows scheduling laundry pick-ups from the customers' houses via a mobile app. Clients choose one of two daily pick-up and drop-off windows and wait for CustomLaundry's driver to pick up the laundry. The laundry is then washed and folded at a Laundromat before it is returned to the client. The Laundromat staff carries out washing and folding while CustomLaundry handles the logistics. CustomLaundry operates in San Francisco, Mountain View, Palo Alto, and Menlo Park. The cost is US$25 for the first bag of each pickup with US$15 for additional loads. CustomLaundry receives discounts from the laundromats so the price stays reasonable. If anything is lost or damaged, CustomLaundry will refund its customers 100% of the cost of the clothes. This is covered by a special insurance. It is estimated that in the United States, US$15 billion was spent on laundry in 2012.

As CustomLaundry grows, it intends to use economies of scale to improve its margins and lower its costs. Rather than expanding into Southern California where Wash.io operates or competing with specialized players in New York City, CustomLaundry is looking at Seattle, Boston, and other dense cities. What differentiates CustomLaundry from the existing companies is simplicity and the flat rate it charges.

The company's customer retention rate is 65%. Within two months, CustomLaundry grew to US$7,000 in recurring monthly revenue. An average subscriber pays the company US$80 per month. The founders intend to move into dry cleaning in the near future.

Disguised Venture Name	Actual Venture Name	Link to Crunchbase Profile	Link to Company Website	Indicators of Success/Failure (as of 2016)	Venture Outcome (as of 2016)
CaseBox	Graft Concepts	https://www.crunchbase.com/organization/graft-concepts	http://www.graftconcepts.com	Company is shut down, never managed to gain mainstream adoption; Amazon reviews suggest poor product quality.	Failure
CoordinateMe	Gocomm	https://www.crunchbase.com/organization/gocomm	http://www.gocomm.com	Claimed they had a first customer contract worth USD150,000. However, doesn't seem to have picked up traction. There are many communication solutions.	Failure
CustomLaundry	Prim	https://www.crunchbase.com/organization/prim	http://getprim.com	Company is shut down. Unprofitable business model that the founders didn't manage to turn into a profitable business.	Failure
DeliverIt	Doordash	https://www.crunchbase.com/organization/doordash	https://www.doordash.com	$59.7M in 4 Rounds from 16 Investors	Success
EduAnalytics	Panorama	https://www.crunchbase.com/organization/panorama-education	http://www.panoramaed.com	$16M in 4 Rounds from 26 Investors	Success
FashionStar	Le Tote	https://www.crunchbase.com/organization/le-tote	https://letote.com	$27.5M in 3 Rounds from 23 Investors; In 2015, Le Tote is on track to deliver more than two million items to customers, achieve 500 percent year-over-year revenue growth for the second year running and quadruple its workforce.	Success
ForeignDigital	Buttercoin	https://www.crunchbase.com/organization/buttercoin	https://buttercoin.com	Company is shut down; $1.25M in 2 Rounds from 9 Investors; Failed despite high-profile investment from YC and Google Ventures; Founder says: VC investment left them unflexible to adapt. Founder said: Should have bootstraped the company and not enter the growth stage too quickly before the product was ready. Also: Decline of bitcoin price (1000 to about 220 at time of closure) made investors more reluctant to invest in the industry.	Failure
GlassSpace	Meta	https://www.crunchbase.com/organization/meta-view	https://www.getameta.com	$23M in 3 Rounds from 11 Investors	Success
LogistiX	Easypost	https://www.crunchbase.com/organization/easypost	https://www.easypost.com	$3.45M in 3 Rounds from 25 Investors	Success

Online Appendix B: The Vote

Now it's time for you to vote. But first go back to Page 2 to reread the detailed instructions. Make sure to take another good look at the exemplary email on that page (you and your teammates will get such an email) to better understand how your vote gets aggregated and how you will be graded as a team.

Remember, if just ONE member of your team votes yes, your team will invest in the specific venture.

	Your Vote [Invest: Yes or No]	Vote Team member 2	Vote Team member 3	Vote Team member 4	Vote Team member 5	Team decision	Was the venture actually a success?	As a team your decision was ….	Points for **Team #**
ForeignDigital									
LogistiX									
FashionStar									
CaseBox									
DeliverIt									
CoordinateMe									
GlassSpace									
EduAnalytics									
CustomLaundry									

The grey portion of the table is for illustration purposes only. Do not fill in the grey cells.

Record your answers in this column ("Yes" or "No")

Online Appendix C: Power Analyses

To ensure our studies were sufficiently powered, we conducted a series of post hoc (Studies 1A and 3) and a priori (Studies 1B-2C) power analyses reported below.

Study 1A
A post hoc power analysis in G*Power (Faul et al. 2007) indicated that the study's sample (n = 140) is sufficiently powered (1 - β = 0.82) to detect medium effects (d = 0.5) at $p < 0.05$.

Study 1B
An a priori power analysis in G*Power (Faul et al. 2007) indicated that in order to detect a small to medium effect size (d = 0.4) at $p < 0.05$ with 80% power, a sample size of 104 for each study condition is required, yielding a target sample size of 312 across the three study conditions.

Study 1C
An a priori power analysis in G*Power (Faul et al. 2007) indicated that in order to detect a small to medium effect size (d = 0.4) at $p < 0.05$ with 80% power, a sample size of 104 for each study condition is required, yielding a target sample size of 208 across the two study conditions.

Study 1D
An a priori power analysis in G*Power (Faul et al. 2007) indicated that in order to detect a low to medium effect size (d = 0.4) at $p < 0.05$ with 80% power, a sample size of 104 for each study condition is required, yielding a target sample size of 208 across the two study conditions.

Study 1E
An a priori power analysis in G*Power (Faul et al. 2007) indicated that in order to detect a medium effect size (d = 0.5) at $p < 0.05$ with 80% power, a sample size of 67 for each study condition is required, yielding a target sample size of 268 across the four study conditions. This target sample size is similar to Kenny's (2018) recommended sample size (n = 316) for detecting a small to medium interaction term (f^2 = 0.025) based on Cohen's (1988) thresholds for interpreting f^2 effect sizes.

Study 1F
Using the power-analysis approach for interaction effects recommended by Kenny (2018) and anticipating a small effect size for the interaction term (f^2 = 0.02) based on Cohen's (1988) thresholds for interpreting f^2 effect sizes, an a priori power analysis in G*Power (Faul et al. 2007) pointed to a target sample size of 395 in order to have 80 percent power to detect an effect at $p < 0.05$.

Study 2A
Based on Fritz and MacKinnon's (2007, Table 3) recommendations regarding statistical power for mediation models with bias-corrected bootstrapping and assuming a small-to-medium effect of the independent variable on the mediator (a = 0.26) and a small effect of the mediator on the dependent variable (β = 0.14), an a priori power analysis pointed to a minimum sample size of 368 participants.

Study 2B
An a priori power analysis in G*Power (Faul et al. 2007) indicated that in order to detect a small to medium effect size (d = 0.35) at $p < 0.05$ with 80% power, a sample size of 136 for each study condition is required, yielding a target sample size of 272 across the two study conditions.

Study 2C
Based on Fritz and MacKinnon's (2007, Table 3) recommendations regarding statistical power for mediation models with bias-corrected bootstrapping and assuming a small-to-medium effect of the independent variable on the mediator (a = 0.26) and a small effect of the mediator on the dependent variable (β = 0.14), an a priori power analysis pointed to a minimum sample size of 368 participants.

Study 3

A post hoc power analysis in G*Power (Faul et al. 2007) indicated that the study's sample (n = 99) is sufficiently powered (1 - β = 0.82) to detect large effects (d = 0.80) at $p < 0.05$.

Online Appendix D: Descriptive Statistics by Condition

Study	Variables	Between-subjects condition 1	Between-subjects condition 2	Between-subjects condition 3	Between-subjects condition 4
1A		*Voting threshold: 1*	*Voting threshold: 2*		
	n	70	70		
	Female	0.37 (0.49)	0.19 (0.39)		
	Age	29.10 (2.14)	29.86 (2.27)		
	Work years	5.85 (1.83)	6.79 (2.62)		
	Venture experience	0.14 (0.35)	0.16 (0.37)		
	Yes votes	2.86 (1.21)	3.89 (1.31)		
1B		*Voting threshold: 1*	*Voting threshold: 2*	*Voting threshold: 3*	
	n	115	126	110	
	Female	0.36 (0.48)	0.32 (0.47)	0.26 (0.44)	
	Age	35.78 (8.55)	36.71 (9.18)	37.08 (9.43)	
	Work years	12.44 (8.18)	13.33 (8.53)	13.81 (9.11)	
	Venture experience	0.36 (0.48)	0.26 (0.44)	0.27 (0.45)	
	Student status	0.16 (0.36)	0.17 (0.37)	0.08 (0.28)	
	Yes votes	3.74 (1.53)	4.22 (1.50)	4.65 (1.18)	
1C		*Voting threshold: 1*	*Voting threshold: 2*		
	n	150	178		
	Female	0.33 (0.47)	0.34 (0.48)		
	Age	36.36 (11.36)	35.26 (12.57)		
	Work years	13.37 (10.21)	12.75 (11.64)		
	Venture experience	0.29 (0.46)	0.29 (0.46)		
	Student status	0.17 (0.37)	0.25 (0.43)		
	Yes votes	3.67 (1.44)	4.16 (1.34)		
1D		*Voting threshold: 3 no votes*	*Voting threshold: 1 no vote*		
	n	111	121		
	Female	0.31 (0.46)	0.26 (0.44)		
	Age	35.45 (10.12)	35.85 (12.01)		
	Work years	12.78 (9.24)	12.38 (10.06)		
	Venture experience	0.27 (0.45)	0.23 (0.42)		
	Student status	0.14 (0.35)	0.22 (0.42)		
	Yes votes	4.66 (1.59)	5.26 (1.55)		
1E		*Voting threshold: 1 and group size: 3*	*Voting threshold: 1 and group size: 6*	*Voting threshold: 2 and group size: 3*	*Voting threshold: 2 and group size: 6*
	n	134	142	121	123
	Female	0.32 (0.47)	0.31 (0.46)	0.34 (0.48)	0.33 (0.47)
	Age	34.08 (8.02)	35.88 (11.36)	36.12 (9.50)	35.20 (8.96)
	Work years	11.19 (7.41)	13.16 (9.91)	13.08 (8.97)	12.23 (8.49)
	Venture experience	0.26 (0.44)	0.27 (0.45)	0.31 (0.46)	0.30 (0.46)
	Student status	0.13 (0.33)	0.14 (0.35)	0.10 (0.30)	0.12 (0.33)
	Yes votes	3.94 (1.46)	3.68 (1.36)	4.38 (1.30)	4.24 (1.31)
1F		*Voting threshold: 1 and deliberation: absent*	*Voting threshold: 1 and deliberation: present*	*Voting threshold: 3 and deliberation: absent*	*Voting threshold: 3 and deliberation: present*
	n	139	178	142	164
	Female	0.29 (0.45)	0.37 (0.48)	0.32 (0.47)	0.30 (0.46)
	Age	34.93 (11.06)	36.40 (11.69)	37.18 (11.79)	38.70 (12.80)
	Work years	12.36 (9.56)	14.08 (10.53)	13.87 (9.88)	16.30 (12.42)
	Venture experience	0.21 (0.41)	0.31 (0.47)	0.28 (0.45)	0.35 (0.48)
	Student status	0.20 (0.40)	0.17 (0.38)	0.12 (0.33)	0.16 (0.37)
	Yes votes	3.76 (1.43)	3.81 (1.42)	4.84 (1.45)	4.17 (1.15)

2A		*Voting threshold: 1*	*Voting threshold: 3*
	n	200	210
	Female	0.31 (0.46)	0.32 (0.47)
	Age	37.36 (9.64)	35.96 (8.79)
	Work years	14.31 (9.45)	12.80 (8.48)
	Venture experience	0.32 (0.47)	0.24 (0.43)
	Student status	0.07 (0.26)	0.10 (0.31)
	Yes votes	3.49 (1.52)	4.48 (1.25)
2B		*Priming: control*	*Priming: information inference*
	n	205	196
	Female	0.27 (0.45)	0.31 (0.46)
	Age	35.19 (8.64)	34.16 (7.96)
	Work years	12.31 (8.26)	11.29 (7.40)
	Venture experience	0.26 (0.44)	0.23 (0.42)
	Student status	0.10 (0.30)	0.18 (0.39)
	Yes votes	3.84 (1.41)	3.45 (1.36)
2C		*Voting threshold: 1*	*Voting threshold: 2*
	n	193	162
	Female	0.30 (0.46)	0.31 (0.47)
	Age	34.08 (9.58)	35.42 (11.62)
	Work years	11.22 (9.82)	13.06 (11.90)
	Venture experience	0.20 (0.40)	0.24 (0.43)
	Student status	0.19 (0.39)	0.17 (0.37)
	Yes votes	4.04 (1.53)	4.58 (1.22)
3		*Voting threshold: 1*	*Voting threshold: 2*
	n	50	49
	Female	0.32 (0.47)	0.20 (0.41)
	Age	37.66 (3.71)	38.04 (4.22)
	Work years	14.98 (3.85)	15.31 (4.34)
	Venture experience	0.18 (0.39)	0.18 (0.39)
	Yes votes	2.96 (1.28)	3.86 (1.80)

Note: Standard deviations in parentheses.

Online Appendix E: Robustness Checks

In the paper, our primary statistical technique are Poisson regressions of yes votes on experimental manipulation(s). In this Appendix, we demonstrate that results are robust to using other types of statistical tests. Specifically, we show the results of (1) Poisson regressions with control variables included, (2) Wilcoxon-Mann-Whitney tests, and (3) random-intercept logistic regressions with participant as clustering variable.

(1) Poisson regressions with control variables included

Table F-1: Poisson Regression Results, Study 1A

Variables	Estimate	95% confidence interval
Constant	2.72 (2.28)	[0.53; 14.05]
Voting threshold	1.38 (0.14)	[1.13; 1.67]
Female	1.05 (0.12)	[0.84; 1.31]
Age	1.00 (0.03)	[0.94; 1.07]
Work years	1.01 (0.03)	[0.95; 1.07]
Siblings	1.01 (0.05)	[0.92; 1.11]
Self-monitoring	0.96 (0.08)	[0.82; 1.12]
Likelihood ratio chi square	11.84	

Notes: n = 136; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table F-2: Poisson Regression Results, Study 1B

Variables	Estimate	95% confidence interval
Constant	2.65 (0.75)	[1.52; 4.62]
Voting threshold	1.13 (0.04)	[1.06; 1.21]
Female	1.10 (0.06)	[0.98; 1.23]
Age	1.00 (0.01)	[0.98; 1.02]
Work years	1.00 (0.01)	[0.97; 1.02]
Siblings	1.07 (0.03)	[1.01; 1.12]
Self-monitoring	0.99 (0.04)	[0.92; 1.07]
Likelihood ratio chi square	20.54	

Notes: n = 351; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table F-3: Poisson Regression Results, Study 1C

Variables	Estimate	95% confidence interval
Constant	2.57 (0.56)	[1.68; 3.92]
Voting threshold	1.13 (0.06)	[1.02; 1.27]
Female	1.09 (0.06)	[0.97; 1.22]
Age	1.00 (0.01)	[0.99; 1.02]
Work years	1.00 (0.01)	[0.98; 1.01]
Siblings	1.03 (0.03)	[0.98; 1.10]
Self-monitoring	1.06 (0.05)	[0.97; 1.15]
Likelihood ratio chi square	10.36	

Notes: n = 328; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table F-4: Poisson Regression Results, Study 1D

Variables	Estimate	95% confidence interval
Constant	4.56 (0.99)	[2.97; 6.99]
Voting threshold	1.15 (0.07)	[1.02; 1.29]
Female	1.15 (0.07)	[1.02; 1.31]
Age	0.99 (0.01)	[0.98; 1.01]
Work years	1.01 (0.01)	[1.00; 1.03]
Siblings	1.03 (0.03)	[0.97; 1.09]
Self-monitoring	1.00 (0.04)	[0.92; 1.09]
Likelihood ratio chi square	12.74	

Notes: n = 232; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table F-5: Poisson Regression Results, Study 1E

Variables	Estimate	95% confidence interval
Constant	4.18 (0.66)	[3.06; 5.69]
Voting threshold	1.13 (0.05)	[1.04; 1.23]
Female	0.99 (0.05)	[0.91; 1.09]
Age	1.00 (0.00)	[0.99; 1.01]
Work years	1.00 (0.01)	[0.99; 1.01]
Siblings	1.03 (0.02)	[0.98; 1.07]
Self-monitoring	0.99 (0.03)	[0.93; 1.06]
Likelihood ratio chi square	11.00	

Notes: n = 517; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table F-6: Poisson Regression Results, Study 1F

Variables	Estimate	95% confidence interval
Constant	3.77 (0.51)	[2.90; 4.90]
Voting threshold	1.29 (0.08)	[1.15; 1.45]
Deliberation	1.02 (0.06)	[0.91; 1.14]
Voting threshold × Deliberation	0.85 (0.07)	[0.73; 1.00]
Female	1.03 (0.04)	[0.95; 1.12]
Age	1.00 (0.00)	[0.99; 1.01]
Work years	1.00 (0.00)	[0.99; 1.01]
Siblings	1.02 (0.02)	[0.98; 1.07]
Self-monitoring	1.01 (0.03)	[0.95; 1.07]
Likelihood ratio chi square	31.37	

Notes: n = 623; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table F-7: Poisson Regression Results, Study 2A

Variables	Estimate	95% confidence interval
Constant	3.34 (0.92)	[1.95; 5.72]
Voting threshold	1.28 (0.06)	[1.16; 1.41]
Female	1.06 (0.06)	[0.96; 1.18]
Age	1.00 (0.01)	[0.98; 1.02]
Work years	0.99 (0.01)	[0.97; 1.02]
Siblings	1.04 (0.03)	[0.99; 1.10]
Self-monitoring	0.97 (0.04)	[0.91; 1.05]
Likelihood ratio chi square	30.07	

Notes: n = 410; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table F-8: Poisson Regression Results, Study 2B

Variables	Estimate	95% confidence interval
Constant	3.06 (0.75)	[1.90; 4.93]
Information inference prime	0.90 (0.05)	[0.81; 1.00]
Female	1.01 (0.06)	[0.90; 1.13]
Age	1.00 (0.01)	[0.99; 1.02]
Work years	1.01 (0.01)	[0.99; 1.02]
Siblings	1.02 (0.03)	[0.96; 1.08]
Self-monitoring	1.01 (0.04)	[0.94; 1.09]
Likelihood ratio chi square	10.40	

Notes: n = 401; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table F-9: Poisson Regression Results, Study 2C

Variables	Estimate	95% confidence interval
Constant	4.08 (0.80)	[2.77; 5.99]
Voting threshold	1.13 (0.06)	[1.02; 1.25]
Female	1.04 (0.06)	[0.94; 1.16]
Age	1.00 (0.01)	[0.99; 1.01]
Work years	1.00 (0.01)	[0.99; 1.01]
Siblings	1.00 (0.03)	[0.95; 1.05]
Self-monitoring	0.98 (0.04)	[0.91; 1.06]
Likelihood ratio chi square	8.08	

Notes: n = 355; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table F-10: Poisson Regression Results, Study 3

Variables	Estimate	95% confidence interval
Constant	3.20 (2.08)	[0.90; 11.41]
Voting threshold	1.33 (0.15)	[1.06; 1.65]
Female	1.08 (0.14)	[0.85; 1.39]
Age	0.99 (0.02)	[0.95; 1.03]
Work years	1.01 (0.02)	[0.97; 1.05]
Siblings	1.07 (0.06)	[0.96; 1.19]
Self-monitoring	0.99 (0.07)	[0.86; 1.15]
Likelihood ratio chi square	7.90	

Notes: n = 96; incidence rate ratios (IRR) are shown; standard errors in parentheses.

(2) Wilcoxon-Mann-Whitney tests

Table F-11: Wilcoxon-Mann-Whitney Tests, Studies 1A-3

Study			Condition 1	Condition 2
1A			*Voting threshold: 1*	*Voting threshold: 2*
	n		70	70
	Rank sum		3915	5955
	Expected		4935	4935
	Adjusted variance	54135.18		
	z	-4.38		
1B			*Voting threshold: 1*	*Voting threshold: 2*
	n		115	126
	Rank sum		12717.5	16443.5
	Expected		13915	15246
	Adjusted variance	280646.80		
	z	-2.26		
			Voting threshold: 2	*Voting threshold: 3*
	n		126	110
	Rank sum		13761.5	14204.5
	Expected		14931	13035
	Adjusted variance	259732.93		
	z	-2.30		
			Voting threshold: 1	*Voting threshold: 3*
	n		115	110
	Rank sum		10870.5	14554.5
	Expected		12995	12430
	Adjusted variance	225959.75		
	z	-4.47		
1C			*Voting threshold: 1*	*Voting threshold: 2*
	n		150	178
	Rank sum		21956	32000
	Expected		24675	29281
	Adjusted variance	697446.39		
	z	-3.26		

1D			*Voting threshold: 3 no votes*	*Voting threshold: 1 no vote*
	n		111	121
	Rank sum		11545	15483
	Expected		12931.5	14096.5
	Adjusted variance	249802.89		
	z	-2.77		
1E			*Voting threshold: 1*	*Voting threshold: 2*
	n		276	244
	Rank sum		65065.5	70394.5
	Expected		71898	63562
	Adjusted variance	2781639.33		
	z	-4.10		
1F			*Voting threshold: 1*	*Voting threshold: 3*
	n		317	306
	Rank sum		85544	108832
	Expected		98904	95472
	Adjusted variance	4812725.14		
	z	-6.09		
2A			*Voting threshold: 1*	*Voting threshold: 3*
	n		200	210
	Rank sum		33070	51185
	Expected		41100	43155
	Adjusted variance	1373312.46		
	z	-6.85		
2B			*Priming: control*	*Priming: information inference*
	n		205	196
	Rank sum		44329	36272
	Expected		41205	39396
	Adjusted variance	1283904.93		
	z	2.76		
2C			*Voting threshold: 1*	*Voting threshold: 2*
	n		193	162
	Rank sum		31118	32072
	Expected		34354	28836
	Adjusted variance	883889.97		
	z	-3.44		
3			*Voting threshold: 1*	*Voting threshold: 2*
	n		50	49
	Rank sum		2134.5	2815.5
	Expected		2500	2450
	Adjusted variance	19275.38		
	z	-2.63		

(3) Random-intercept logistic regressions with participant as clustering variable

Table F-12: Random-Intercept Logistic Regression Results, Study 1A

Variables	**Estimate**	**z-value**	**95% confidence interval**
Constant	-0.77 (0.09)	-8.94	[-0.93; -0.60]
Voting threshold	0.49 (0.12)	4.18	[0.26; 0.72]
Wald chi square	17.46		

Notes: Participant is the clustering variable; 1,260 observations; 140 groups; standard errors in parentheses.

Table F-13: Random-Intercept Logistic Regression Results, Study 1B

Variables	Estimate	z-value	95% confidence interval
Constant	-0.54 (0.10)	-5.63	[-0.73; -0.35]
Voting threshold	0.21 (0.04)	4.58	[0.12; 0.29]
Wald chi square	20.98		

Notes: Participant is the clustering variable; 3,159 observations; 351 groups; standard errors in parentheses.

Table F-14: Random-Intercept Logistic Regression Results, Study 1C

Variables	Estimate	z-value	95% confidence interval
Constant	0.20 (0.06)	3.20	[0.08; 0.32]
Voting threshold	0.21 (0.08)	2.47	[0.04; 0.37]
Wald chi square	6.10		

Notes: Participant is the clustering variable; 2,952 observations; 328 groups; standard errors in parentheses.

Table F-15: Random-Intercept Logistic Regression Results, Study 1D

Variables	Estimate	z-value	95% confidence interval
Constant	0.07 (0.07)	1.05	[-0.06; 0.20]
Voting threshold	0.28 (0.09)	2.94	[0.09; 0.46]
Wald chi square	8.66		

Notes: Participant is the clustering variable; 2,088 observations; 232 groups; standard errors in parentheses.

Table F-16: Random-Intercept Logistic Regression Results, Study 1E

Variables	Estimate	z-value	95% confidence interval
Constant	-0.31 (0.04)	-7.63	[-0.39; -0.23]
Voting threshold	0.23 (0.06)	3.84	[0.11; 0.34]
Wald chi square	14.73		

Notes: Participant is the clustering variable; 4,680 observations; 520 groups; standard errors in parentheses.

Table F-17: Random-Intercept Logistic Regression Results, Study 1F

Variables	Estimate	z-value	95% confidence interval
Constant	-0.32 (0.04)	-8.41	[-0.39; -0.24]
Voting threshold	0.31 (0.05)	5.77	[0.20; 0.42]
Wald chi square	33.27		

Notes: Participant is the clustering variable; 5,607 observations; 623 groups; standard errors in parentheses.

Table F-18: Random-Intercept Logistic Regression Results, Study 2A

Variables	Estimate	z-value	95% confidence interval
Constant	-0.46 (0.05)	-9.44	[-0.55; -0.36]
Voting threshold	0.45 (0.07)	6.68	[0.32; 0.58]
Wald chi square	44.65		

Notes: Participant is the clustering variable; 3,690 observations; 410 groups; standard errors in parentheses.

Table F-19: Random-Intercept Logistic Regression Results, Study 2B

Variables	Estimate	z-value	95% confidence interval
Constant	-0.29 (0.05)	-6.24	[-0.39; -0.20]
Information inference prime	-0.18 (0.07)	-2.68	[-0.32; -0.05]
Wald chi square	7.20		

Notes: Participant is the clustering variable; 3,609 observations; 401 groups; standard errors in parentheses.

Table F-20: Random-Intercept Logistic Regression Results, Study 2C

Variables	Estimate	z-value	95% confidence interval
Constant	-0.20 (0.05)	-4.24	[-0.30; -0.11]
Voting threshold	0.24 (0.07)	3.37	[0.10; 0.38]
Wald chi square	11.39		

Notes: Participant is the clustering variable; 3,195 observations; 355groups; standard errors in parentheses.

Table F-21: Random-Intercept Logistic Regression Results, Study 3

Variables	Estimate	z-value	95% confidence interval
Constant	-0.73 (0.11)	-6.66	[-0.94; -0.51]
Voting threshold	0.43 (0.15)	2.87	[0.14; 0.73]
Wald chi square	8.24		

Notes: Participant is the clustering variable; 891 observations; 99 groups; standard errors in parentheses.

Online Appendix F: Regression Tables

Table G-1: Poisson Regression Results, Study 1A

Variables	Estimate	95% confidence interval
Constant	2.86 (0.20)	[2.49; 3.28]
Voting threshold	1.36 (0.13)	[1.13; 1.63]
Likelihood ratio chi square	11.03	

Notes: n = 140; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-2: Poisson Regression Results, Study 1B

Variables	Estimate	95% confidence interval
Constant	3.37 (0.24)	[2.93; 3.88]
Voting threshold	1.12 (0.04)	[1.05; 1.19]
Likelihood ratio chi square	11.23	

Notes: n = 351; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-3: Poisson Regression Results, Study 1C

Variables	Estimate	95% confidence interval
Constant	3.67 (0.16)	[3.37; 3.99]
Voting threshold	1.14 (0.06)	[1.02; 1.27]
Likelihood ratio chi square	5.12	

Notes: n = 328; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-4: Poisson Regression Results, Study 1D

Variables	Estimate	95% confidence interval
Constant	4.66 (0.20)	[4.27; 5.08]
Voting threshold	1.13 (0.07)	[1.01; 1.27]
Likelihood ratio chi square	4.30	

Notes: n = 232; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-5a: Poisson Regression Results, Study 1E

Variables	Model 1	95% confidence interval	Model 2	95% confidence interval	Model 3	95% confidence interval
Constant	3.81 (0.12)	[3.58; 4.05]	4.15 (0.13)	[3.91; 4.41]	3.94 (0.17)	[3.62; 4.29]
Voting threshold	1.13 (0.05)	[1.04; 1.23]			1.11 (0.07)	[0.99; 1.25]
Group size			0.95 (0.04)	[0.87; 1.04]	0.93 (0.06)	[0.83; 1.05]
Voting threshold × Group size					1.04 (0.09)	[0.87; 1.23]
Likelihood ratio chi square	8.10		1.36		9.56	

Notes: n = 520; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-5b: Subgroup Analyses, Study 1E

Variables	Group size of 6	95% confidence interval	Group size of 3	95% confidence interval
Constant	3.68 (0.16)	[3.38; 4.01]	3.94 (0.17)	[3.62; 4.29]
Voting threshold	1.15 (0.07)	[1.02; 1.30]	1.11 (0.07)	[0.99; 1.25]
Likelihood ratio chi square	5.24		2.96	
n	265		255	

Notes: Incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-6a: Poisson Regression Results, Study 1F

Variables	Model 1	95% confidence interval	Model 2	95% confidence interval	Model 3	95% confidence interval
Constant	3.79 (0.11)	[3.58; 4.01]	4.30 (0.12)	[4.07; 4.55]	3.76 (0.16)	[3.45; 4.09]
Voting threshold	1.18 (0.05)	[1.09; 1.28]			1.29 (0.07)	[1.15; 1.44]
Deliberation			0.93 (0.04)	[0.86; 1.00]	1.02 (0.06)	[0.91; 1.14]
Voting threshold × Deliberation					0.85 (0.07)	[0.73; 0.99]
Likelihood ratio chi square	18.05		3.75		25.67	

Notes: n = 623; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-6b Subgroup Analyses, Study 1F

Variables	Deliberation absent	95% confidence interval	Deliberation present	95% confidence interval
Constant	3.76 (0.16)	[3.45; 4.09]	3.81 (0.15)	[3.54; 4.11]
Voting threshold	1.29 (0.07)	[1.15; 1.44]	1.09 (0.06)	[0.98; 1.22]
Likelihood ratio chi square	19.20		2.71	
n	281		342	

Notes: Incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-7: Poisson Regression Results, Study 2A

Variables	Estimate	95% confidence interval
Constant	3.49 (0.13)	[3.24; 3.76]
Voting threshold	1.28 (0.06)	[1.16; 1.41]
Likelihood ratio chi square	25.05	

Notes: n = 410; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-8: Poisson Regression Results, Study 2B

Variables	Estimate	95% confidence interval
Constant	3.84 (0.14)	[3.58; 4.12]
Information inference prime	0.90 (0.05)	[0.81; 0.99]
Likelihood ratio chi square	4.29	

Notes: n = 401; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-9: Poisson Regression Results, Study 2C

Variables	Estimate	95% confidence interval
Constant	4.04 (0.14)	[3.77; 4.34]
Voting threshold	1.13 (0.06)	[1.02; 1.25]
Likelihood ratio chi square	5.95	

Notes: n = 355; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-10: Random-Intercept Logistic Regression Results, Study 3

Variables	Estimate	z-value	95% confidence interval
Constant	-2.61 (0.28)	-9.19	[-3.16; -2.05]
Voting threshold	1.51 (0.33)	4.51	[0.85; 2.16]
Level of information	2.74 (0.31)	8.98	[2.14; 3.34]
Voting threshold × Level of information	-1.37 (0.37)	-3.71	[-2.09; -0.65]
Wald chi square	118.24		

Notes: Participant is the clustering variable; 891 observations; 99 groups; standard errors in parentheses.

Table G-11a: Poisson Regression Results, Post-hoc Analysis

Variables	Estimate	95% confidence interval
Constant	6.60 (0.51)	[5.67; 7.69]
Voting threshold inference prime	0.82 (0.10)	[0.65; 1.03]
Likelihood ratio chi square	2.86	

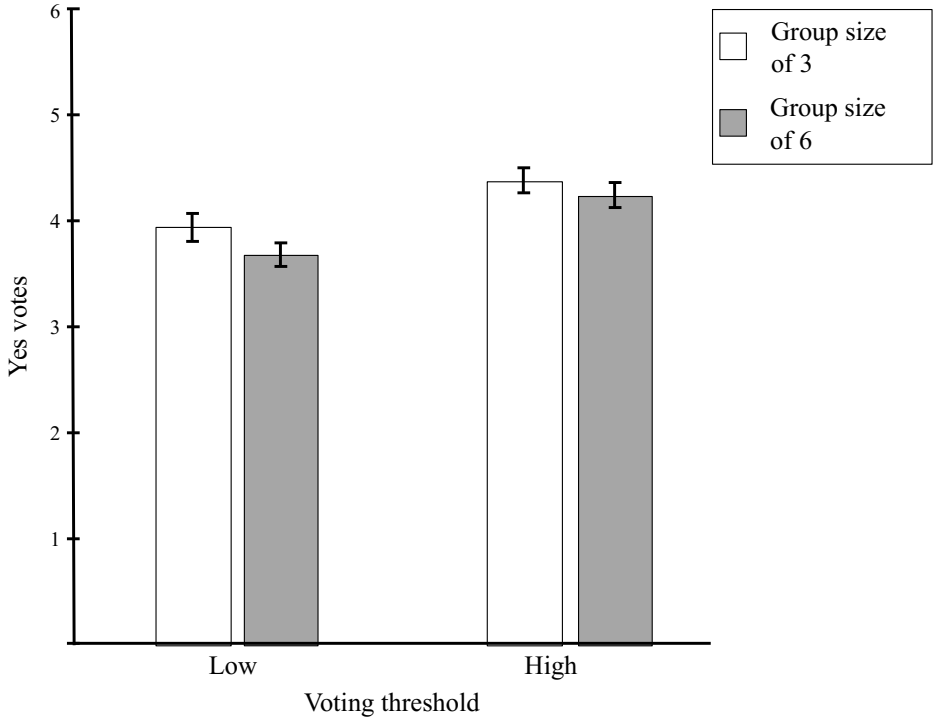
Notes: n = 49 groups; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Table G-11b: Poisson Regression Results, Counterfactual Analyses

Variables	Uniform 2-vote counterfactual	95% confidence interval	Uniform 1-vote counterfactual	95% confidence interval
Constant	3.80 (0.39)	[3.11; 4.65]	6.60 (0.51)	[5.67; 7.69]
Voting threshold	1.43 (0.19)	[1.09; 1.86]	1.16 (0.12)	[0.94; 1.43]
Likelihood ratio chi square	6.99		1.81	

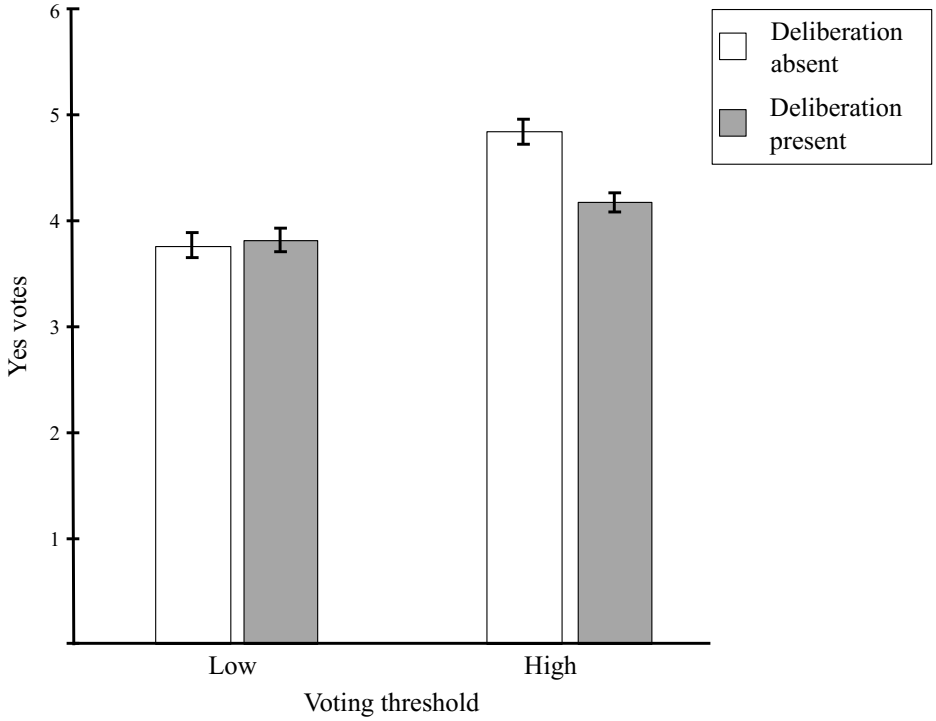
Notes: n = 49 groups; incidence rate ratios (IRR) are shown; standard errors in parentheses.

Online Appendix G: Mean Yes Votes by Condition, Study 1E



Note: Error bars represent standard errors.

Online Appendix H: Mean Yes Votes by Condition, Study 1F

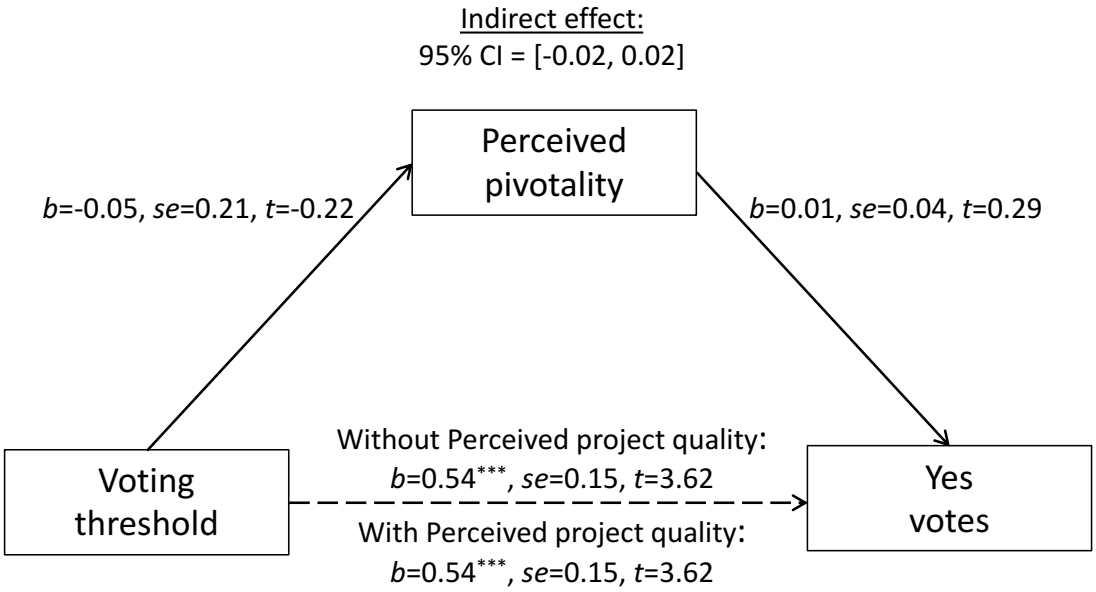


Note: Error bars represent standard errors.

Online Appendix I: Differences Across Essay Conditions, Study 2B

Information Inference Condition	Control Condition
Before casting your votes, we want to be sure you understand how the eventual investment decision is made on the level of the group. **Instructions** Please take approximately 5 minutes and write 7-8 sentences in the text box below. Bear in mind your team's voting rule: your team will invest in a given venture if just 1 team member votes "yes" on it. Now imagine the situation in which your team invests in a venture only because of your "yes" vote. That is, without your "yes" vote, your team would not have invested, but given that you voted "yes," it DID invest in the venture. In your essay, describe this hypothetical situation in greater detail, making sure to address the following three questions: (1) How many team members (including yourself) must have voted "yes" for this situation to occur (this question should be easy - but please write down your answer so I know you understood the rule)? (2) What can you infer from these votes regarding the likely quality of the venture (low, medium, high)? (3) Retrospectively, do you think it was a good choice to vote "yes" in this situation?	Before casting your votes, we want you to clear your mind and tell us about your opinion regarding a hotly debated issue surrounding entrepreneurship education. **Instructions** Please take approximately 5 minutes and write 7-8 sentences in the text box below. I would like you to elaborate your opinion about the merits of teaching entrepreneurship in business school. Try to be very specific, also making sure to address the following three questions: (1) Do you think entrepreneurship can be taught, or is this rather a skill that people are born with? (2) What do you think should be the role of business schools in teaching entrepreneurship? (3) How should business schools teach entrepreneurship?

Online Appendix J: Mediation Model, Study 2C



Notes: n = 355. Results from PROCESS model are shown. The statistics directly above the dashed arrow indicate the direct effect of voting threshold on yes votes, not accounting for the mediator. The statistics below the dashed arrow indicate the direct effect of voting threshold on yes votes, with the mediator included in the regression model. Asterisks denote coefficients at different significance levels ($^{\dagger}p < 0.10$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$; two-tailed tests).

Online Appendix K: Differences Across Level of Information (Within-Subjects) Conditions, Study 3

Low Level of Information Condition (Example)	High Level of Information Condition (Example)
 FashionStar **Some of your team members have done additional research and therefore have more information on this company than you do. The information that is blurred is information that some of your teammates have, but you do <u>not</u> have.** A former Partner and Associate at the private technology investment bank, Ridgecrest Partners co-founded FashionStar in May 2012. [redacted] The company aims to be the Netflix or Airbnb for fast fashion. [redacted] Each month, three garments and two accessories are sent to the customer. They can keep the outfit for an unlimited time until they tire of it and then return it in exchange for a new outfit. The "FashionComets" (the bags sent in the mail) can be returned free of charge and as often as desired. Special requests can be made to customise the bags – just tops, just accessories, etc. Customers can also purchase an insurance plan that will cover any stains or tears on returned clothing. [redacted] [redacted] Industry sales of women's clothing in the US were estimated at more than US$110 billion in 2011.	 ForeignDigital **You have done additional research on this company and therefore have more information on the company than your team members. Specifically, the information that is highlighted in the text (bold and underlined) is information that you have exclusively and that your teammates lack.** ForeignDigital is a platform that enables international m oney transfers via Bitcoin exchanges.[1] The model involves opening a local Bitcoin exchange in a new country where ForeignDigital will partner with local money transfer businesses. The latter are not involved in the Bitcoin-to-local-currency transactions, but they receive a 50% cut of ForeignDigital's fees in exchange for having the required licenses and relationships with regulators. The company's co-founders previously sold an analytics-based company, filmed documentaries, founded a start-up house, and started a bitcoin mining operation called CoinHarvest. **<u>While working on the last of these ventures, the duo considered creating a legal platform for building Bitcoin exchanges and settled on the remittance market.</u>** The company's goal is to open shop in India within the next three months and expand into six additional countries within nine months. **<u>The World Bank estimates that migrants will send approximately US$515 billion, ten times the amount of the US's budget for foreign aid, to relatives in developing countries by 2015.</u>** ForeignDigital aims to disrupt the remittance industry by allowing its user to send money quickly, inexpensively and legally via Bitcoin. ForeignDigital's current main competitors are Coinbase, Bitpay and Bitnet. The company says it will take a commission of less than 1%. The average fee on remittances is 9% of the transaction cost, and some banks charge additional "lifting" fees of up to 5% when someone wants to turn the remittance into cash.