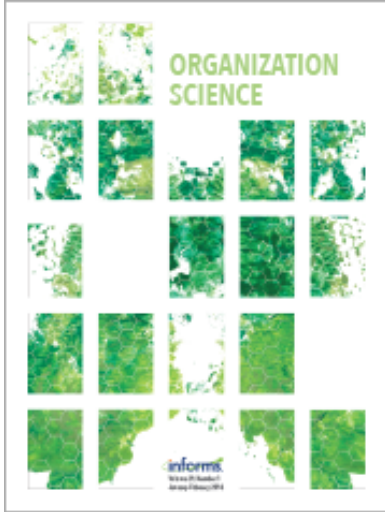




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The Six Dimensions of Strong Theory

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Abstract. One of the most important features on which to judge the merit of any academic paper is the strength of its theory. Although commentary about what constitutes strong theory is widespread, there is no holistic account of the full range of existing perspectives. To address this oversight, I construct a typology composed of six dimensions of strong theory: importance, interestingness, actionability, generality, simplicity, and accuracy. This typology provides a lens to examine a vexing problem: the effort to increase the strength of a theory on one dimension will usually compromise its strength on others. Despite this reality, authors experience pressure in the review process to optimize theories on all six dimensions. I explain how the expectation to build theories that cannot possibly be built (what I call the fruitless search for unicorn theories) is driven by historical forces. The study of organizations emerged not from one source but from a half-dozen distinct traditions, including applied scientists who study time-sensitive problems, disciplinary scholars who identify universal laws of human behavior, and empiricists who prefer a tight link between concepts and measures. Review teams are often composed of referees who hail from different traditions and possess divergent views on theory. Even when each reviewer has concerns that are reasonable in isolation, authors often confront unrealistic expectations when reviewers' preferences are aggregated. To mitigate this problem, I recommend that authors, reviewers, and editors (1) prioritize fewer dimensions in any single theory and (2) emphasize distinct dimensions in different theories on the same topic.

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Keywords: theory • philosophy of science • science of science • metascience • theoretical contribution

1. Introduction

Theory lies at the heart of the organizational sciences. Indeed, organizational scholars maintain that one of the most important features on which to judge the merit of any academic paper is its theoretical contribution (Daft and Lewin 2008, Shapira 2011, Ashford 2013). Even scholars who do not want their claims to stray far from observable phenomena—such as applied researchers who scrutinize data on how family businesses innovate (Duran et al. 2016), ethnographers who study how film directors handle surprises on set (Bechky and Okhuysen 2011), or empiricists interested in how chief executive officers (CEOs) benefit from media appearances (Kang and Han Kim 2017)—still build theory insofar as they describe how distinct concepts are related beyond any single time and place (Sandberg and Alvesson 2021). The reality that theory is relevant to almost all research invites a broad range of perspectives with respect to what constitutes strong theory (Gavetti and Levinthal 2004, Cronin et al. 2021). For example, scholars pronounce

that strong theories are those that address important problems (Tihanyi 2020), captivate the interest of readers (Davis 1971), translate to practical insights (Beckman 2022), explain broad swaths of phenomena (Shapira 2011), strive for simplicity (Bacharach 1989), and precisely depict causality (Fiss 2011).

Reflecting this diverse range of perspectives, many commentators acknowledge that theory can be strengthened on multiple dimensions rather than a single one (Dubois and Gadde 2014, Farjoun et al. 2015, George 2016, Antonakis 2017, Aguinis and Cronin 2022). However, each commentary emphasizes a different subset of dimensions. One editorial may argue that the most valuable theories are both simple and precisely calibrated (Ashforth 2021), another may extol theories that examine important problems and translate to actionable takeaways (Colquitt and George 2011), and yet another may prescribe theories that upend assumptions while explaining a wide range of phenomena (David and Hatchuel 2007). It is understandable that each

commentary would focus on only one portion of such a wide range of perspectives. However, readers will be left with an incomplete understanding of strong theory after engaging with any single view. They are then likely to be confused after being exposed to multiple commentaries as they grapple with how different perspectives are related.

To address the need for a holistic account of the core dimensions of theoretical strength, I synthesize 284 commentaries on the nature of strong theorizing and organize them within a typology composed of six dimensions: importance, interestingness, actionability, generality, simplicity, and accuracy. After validating this typology by comparing it against a series of alternatives, I explain how this six-dimensional framework can help scholars compare different interpretations of strong theory. Toward that end, I use the typology as a starting point to illuminate a vexing problem: the effort to increase the strength of a theory on one dimension will usually compromise its strength on others. For example, a theory's importance typically relates to its ability to stand the test of time; however, timeless truths tend to be widely assumed (thus lacking the element of surprise that often causes an idea to be interesting) and so wide ranging that they are difficult to translate into specific behaviors (thus lacking actionability).

The presence of trade-offs means that it is not possible to optimize a single theory on all dimensions. Despite this constraint, authors experience pressure to develop theories that are superlative in every way (what I call unicorn theories because they cannot possibly be built). I explain how the fruitless search for unicorn theories is driven by the unique history of organizational research. The study of organizations emerged not from one source but from a half-dozen distinct traditions, including applied scientists motivated to solve urgent problems, disciplinary scholars striving to identify universal laws of human behavior, and empiricists focused on reducing the gap between the constructs in their hypotheses and the measures in their data. Because these traditions have markedly distinct objectives, they differ sharply as to what constitutes strong theory.

Authors are most likely to experience pressure from disparate traditions when the stakes are the highest: in the review process. Compared with other groups involved in knowledge generation (e.g., dissertation committees), review teams are more likely to be composed of members who belong to different institutions, networks, and subdisciplines. Thus, reviewers evaluating the same paper frequently have different views on theoretical strength. Even when individual referees have preferences that are reasonable in isolation, the aggregation of their viewpoints may seem like a patchwork of contradictory concerns, thereby putting pressure on authors to craft theories that excel on most or all dimensions at once (Gans and Shepherd 1994, Starbuck

2003). Altogether, the fruitless search for unicorn theories makes the process of generating theory seem like a baffling combination of savvy, arbitrage, and guesswork. This drives inefficiency in how scholars receive feedback, adjust to it, and learn from it (Pfeffer 1993).

I recommend two key changes to the knowledge generation process: authors, reviewers, and editors should prioritize (1) fewer dimensions in any single theory and (2) all six dimensions over the long term by focusing on distinct dimensions in different theories on the same topic (e.g., a general and important theory in one paper followed by a simple and actionable theory in another). The first change addresses a downside of a discipline shaped by traditions with conflicting standards, and the second harnesses a benefit of a field composed of diverse viewpoints: the ability to publish work with broader reach. Altogether, my “theory of theory” can free authors from much of the hand-wringing common to publishing and help referees demand bolder contributions without putting authors in an unwinnable position. More broadly, scholars can use the six dimensions of strong theory to build a more sustainable model of research.

2. A Typology of the Dimensions of Strong Theory

I searched the literature for commentaries on theory, wherein theory is defined as arguments that describe how distinct concepts are causally, conceptually, or temporally related (Bacharach 1989, Sutton and Staw 1995, Sandberg and Alvesson 2021).¹ By this definition, theory is nearly ubiquitous. Almost every paper in our discipline features concepts rather than only observations and cases—that is, ideas that abstract away from a specific time, organization, or sample rather than providing a journalistic account of one set of individuals in a single location at a single point in time (Cronbach and Meehl 1955, Eden and Huxham 1996). Even papers with a strong empirical focus typically include arguments that (often by implication) extend well beyond the specific sample and time period featured in the empirics. The *strength* of a theory is the degree to which it advances the knowledge of direct stakeholders (organizational scholars) and other beneficiaries, including students and practitioners (Shapira 2011). A stronger theory is one that contributes more to the literature, especially for the author's primary audience. Using these definitions, I employed the search strategy detailed in the appendix to identify commentaries related to every stage of a theory's life cycle.² Given that strength pertains to the content of theory, I did not consider articles that examined the process of building theory (Colquitt and Zapata-Phelan 2007), the distinction between a theory's strength and validity, the distinction between what theory is and is not (Sutton and Staw 1995), or whether organizational

research should have theory at all (Hambrick 1994) (see the appendix for details).

I identified 284 editorials that met these criteria and 14 themes that emerged across them (see Table 1). I distilled these 14 themes into higher-order dimensions, culminating in a typology with six dimensions of strong theory: importance, interestingness, actionability, generality, simplicity, and accuracy (Table 1 shows how the 14 themes were grouped). To test whether this typology possessed sufficient variation between versus within dimensions, I employed Wordnet, a linguistic dictionary that maps the similarity between about 117,000 distinct concepts. I reduced each of the 14 themes to a single keyword, which is a requirement in Wordnet (see Table 1 for the keywords for each theme). For each pair of words, I calculated the Wu–Palmer similarity score ranging from zero (unrelated) to one (identical). This enabled me to compare the similarity scores for word pairs I assigned to the same dimension (e.g., “actionable” and “applicable”) versus different dimensions (e.g., “actionable” and “general”). A regression analysis confirmed that there was significantly more variation between word pairs that belong to different dimensions than those that belong to the same dimensions ($b = 0.10$, standard error = 0.033, $t = 2.99$, $p = 0.003$; see the Open Science Framework (OSF) link for the results).³

I also tested whether the six-dimensional framework possessed more between versus within variation than alternative configurations. There are about 63.4 million distinct six-dimensional groups using the 14 themes listed in Table 1. Given that testing all of them is not

feasible, I used Python to calculate the between versus within variation of 1,000 of these configurations (the code is provided on OSF). The cluster that emerged from the 1% of configurations with the greatest between versus within variation was the same as the original configuration shown in Table 1. Given that there are more than 60 million configurations, the odds that the top-ranked cluster would replicate the initial framework by chance are low. Altogether, these analyses provide some confirmatory evidence for the six-dimensional typology. The appendix classifies all 284 editorials according to the six dimensions of strong theory.⁴

3. Summary of the Six Dimensions of Strong Theory

Importance relates to whether a theory provides insight into a significant problem (Daft and Lewin 2008, Ashford 2013). Theories are more important if they identify patterns that are longstanding and cut across contexts. A theory’s importance is also fueled by its robustness. The theory of Flynn and Lide (2023) on leader communication is a notable example. Their prediction that followers would criticize leaders more for undercommunication than overcommunication was not only directionally correct but large in magnitude; leaders were maligned 10 times more often for undercommunication than overcommunication. Because theoretical importance is determined by readers, I focus on when consensus emerges among scholars with respect to the inherent significance of an idea (Hollenbeck 2008). Scholars may consider a

Table 1. How 14 Themes from the Literature Map onto the Six Dimensions of Strong Theory

Dimension of strong theory	Importance	Interestingness	Actionability	Generality	Simplicity	Accuracy
Description of the most common themes in literature (Wordnet keyword for each theme is in parentheses)	Establish an important contribution (keyword: <i>important</i>)	Draw the reader’s interest (keyword: <i>interesting</i>)	Translate to action steps (keyword: <i>actionable</i>)	Pertain to a general class of ideas (keyword: <i>general</i>)	Possess a limited number of concepts and causes (keyword: <i>simple</i>)	Portray reality accurately (keyword: <i>accurate</i>)
	Significantly advance knowledge (keyword: <i>significant</i>)	Convey an engrossing narrative (keyword: <i>engrossing</i>)	Be practically applicable (keyword: <i>applicable</i>)	Explain a broad range of phenomena (keyword: <i>broad</i>)	Reduce complexity to increase clarity (keyword: <i>clear</i>)	Precisely specify concepts and causes (keyword: <i>precise</i>)
	Substantially contribute to thought (keyword: <i>substantial</i>)	Reveal assumption-challenging insights (keyword: <i>revelatory</i>)				

Notes. As described in the text, it is possible to test whether the six-dimensional typology possesses sufficient variation between vs. within dimensions via Wordnet, a linguistic dictionary that maps the similarity between concepts. As a first step, I reduced each of the 14 themes displayed in this table to a single keyword, which is a requirement in Wordnet. The keywords are in parentheses below the description of each theme. For each pair of keywords, I calculated the Wu–Palmer similarity score ranging from zero (the words are unrelated) to one (the words are identical). This enabled me to compare the similarity scores for the themes that I assigned to the same dimension (e.g., “actionable” and “applicable”) vs. the themes that I assigned to different dimensions (e.g., “actionable” and “general”). Please see the text for the results of this analysis.

theory more important if, for example, it explains a widely studied outcome (e.g., a typology of the strongest predictors of isomorphism) or sheds new light on a key social phenomenon (e.g., a theory that establishes how local hiring practices contribute to broader patterns of inequality).

Interestingness represents the capacity of ideas to stir curiosity. A theory may be interesting because it challenges established conventions or reveals an unexpected insight—an “aha” moment (Davis 1971, Barley 2006). In this respect, the element of surprise or revelation can spark curiosity and drive the desire to share insights with others. Given that valence (whether something is bad or good) is the primary factor that determines how people remember information (Gilbert 2006), one way to elicit interest is to establish when a positive effect becomes negative or vice versa. For example, although the person-job fit literature established that employees benefit when their values are aligned with their organizations, employees who are “misfits” can prompt innovation because managers often adjust the tasks that they assign to misfits to help them thrive (Sun and Billsberry 2024).

Actionability relates to whether a theory’s insights solve real-world problems—a dimension that is especially central in an applied field (Lewin 1946, Rousseau 2006, Gioia 2022). A theory is more actionable if its constructs are levers that can be “pulled” because they are cost effective and easy to implement (Thomas and Tymon 1982, Berg 2014). In micro research, one-time behaviors (e.g., recognizing high performers) and inexpensive tweaks (e.g., allowing employees to work behind a privacy curtain) (Bernstein 2012) are more actionable than constructs related to cultural values, ingrained habits, or immutable identities. At the macro level, actionable ideas include policy changes that can be implemented without creating more bureaucracy. This may include adopting specific monetary targets or publishing company-wide compensation data (Castilla 2015). In short, an actionable theory is a useful and usable one.

Generality pertains to a theory’s relevance to a broad array of phenomena. Related to the concept of bandwidth (Cronbach and Gleser 1957), this criterion speaks to a theory’s reach and inclusiveness of a wide array of circumstances. The most general theories are unified paradigms, typologies composed of collectively exhaustive constructs, and contingency models that map out an entire domain (Seyed 2012). Examples of general theories include job demands theory, institutional theory, expectancy theory, dynamic capabilities, and the behavioral theory of the firm. Another example is the knowledge-based theory of the firm, which helps explain a wide range of ways that managers support knowledge formation (Nickerson and Zenger 2004).

Simplicity reflects whether theories are clear, digestible, and easy to understand (von Nordenflycht 2023). A theory is simpler when it has fewer constructs and

causal linkages. Theories are also simpler when they are focused around a coherent theme. Whereas simplicity clarifies, complexity obscures; although theories can have thousands of variables and paths (as is the case in some research on systems dynamics and machine learning), readers can only retain five to nine discrete bits of information in mind at once (Miller 1956). As such, many scholars prefer theories with only a handful of constructs and causes (Ashforth 2021). Indeed, theorists often lament that a large number of constructs reflects “theory piling on”—sacrificing parsimony by superficially incorporating a bevy of ideas (King and Lepak 2011, p. 208).

Accuracy relates to whether a theory is closely tethered to the phenomena of interest (George 2016). One way to increase accuracy is to define constructs less abstractly. It is more concrete to examine the effect of a CEO’s speaking tone on employee positive mood than the effect of CEO core self-evaluations on firm culture. Speaking tone is still a theoretical construct insofar as it has no single observable referent and can be operationalized in many ways (e.g., the volume or cadence of speech) (Cronbach and Gleser 1957). However, it closely mirrors reality.⁵ Another example is the theory of Haas (2010) on self-managing teams engaged in knowledge-intensive work in multinational organizations. Not only is this paper’s empirical setting rich, but its theory is bound to one type of team doing a particular type of work in a specific class of organizations.

4. The Stage of a Theory’s Consumption vs. the Structure of Its Logic

The first three dimensions (importance, interestingness, and actionability) represent distinct phases of knowledge consumption. Although a theory’s importance represents its potential to redirect discourse, an important theory will not be impactful if it is not consumed in the first place. Scholars have significant discretion over what to read, and true to the root word “interest,” it is interestingness that often determines which papers get on their radar. Finally, a common view is that an important theory may not be socially valuable if it cannot help solve real-world problems (actionability). In short, the first three dimensions reflect an input-process-output model of knowledge consumption; interestingness often determines if a theory is consumed, importance represents a theory’s potential to substantially alter how readers think once they engage with it, and actionability relates to whether scholars use a theory to solve applied problems after reading it. By extension, the dimensions of importance, interestingness, and actionability are useful for comparing how distinct theories will be consumed and used. A theory pertaining to how CEO narcissism increases employee creativity may be more surprising but less robust than a theory suggesting that CEO turnover increases after a decline in firm performance.

Whereas the first three dimensions are useful for comparing different ideas, the second three (generality, simplicity, and accuracy) pertain to different ways of understanding the *same* idea. Consider a theory positing that team conflict decreases team performance. This theory is simple (featuring two constructs and one causal relationship) and relatively general such that each construct is inclusive of a wide range of ideas. Now, consider a theory positing that status conflict decreases team creativity. This theory is equally simple, but it is more accurate and less general because it conceptualizes both the predictor and the outcome at a more granular level of abstraction. Finally, consider a theory that maps the link between four types of team conflict (task conflict, relational conflict, process conflict, and status conflict) and two types of team performance (team creativity and team productivity). This is as general as the first theory (it covers all subdimensions of team conflict and team performance) and as accurate as the second theory (it features subtypes rather than parent constructs), but it is less simple than both (possessing eight constructs rather than two). In each theory, the topic remains the same—team conflict drives team performance—yet the properties of the theory's constructs vary.

In sum, the first three dimensions (importance, interestingness, and actionability) involve the *stages of a theory's consumption* (input, process, or output), and the latter three dimensions (generality, simplicity, and accuracy) involve the *structure of a theory's logic*. This demarcation will be helpful for understanding interdependence among the dimensions, which I turn to now.

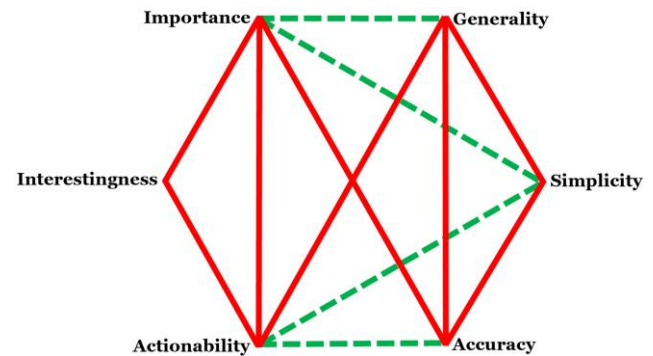
5. Interdependence Among the Six Dimensions of Strong Theory

The six dimensions of strong theory can be related to each other in two key ways: positive covariation (strengthening one dimension usually bolsters another) and negative covariation (improving one dimension usually undermines another). I visually depict which dimensions are positively and negatively related in the “theory hexagon” shown in Figure 1.

5.1. Positive Covariation: When Enhancing One Dimension Bolsters Another

The existence of positive spillovers hinges on the following tenet: generality, simplicity, and accuracy serve as logical parameters that establish the conditions necessary to optimize importance, interestingness, and actionability. First, generality increases importance because it involves timeless ideas that appeal to many audiences (Schilling and Green 2011, Leahey 2018). A theory linking *justice* and *well-being* possesses concepts relevant to many literatures, whereas a theory tying *email salutation style* to *colleague responsiveness* is narrow and less meaningful. Second, simple ideas (especially general ones) are more

Figure 1. (Color online) The Theory Hexagon: Positive and Negative Covariation Among the Six Dimensions



Note. The dashed green lines indicate positive covariation, and the solid red lines indicate negative covariation (trade-offs).

likely to be considered important because of the exaltation of parsimony, which involves distilling many claims into law-like principles (Rhodes et al. 2004). As examples, transaction cost economics explains a variety of outcomes, including search costs, asset specificity, and organizational structure (Williamson 1989); a single dimension (tightness-looseness) explains many cultural differences (Gelfand et al. 2006); and more than 90% of social perception is explained by only two judgments—whether a person appears benevolent and competent (Martin and Slepian 2021). Third, simplicity is inherent to actionability because an actionable concept is a well-defined lever. Finally, concepts are more actionable when they are more concrete (accurate) because they are more closely tied to real life. For instance, making people opt out of being an organ donor rather than go out of their way to opt in increases donor registration from 12% to 99% (Johnson and Goldstein 2003). It is worth emphasizing that accurate concepts are not automatically correct or valid; rather, they concretely depict reality.

5.2. Negative Covariation: The Specter of Trade-offs

Some scholars have touched on the risk of trade-offs, but they typically focus on one trade-off at a time. An examination of all six dimensions within the same framework illuminates that (1) these trade-offs are more widespread and extreme than any single commentator has previously suggested and that (2) a number of trade-offs have not yet been uncovered.

5.2.1. Trade-offs Among Importance, Interestingness, and Actionability. Perhaps the most vexing trade-off is between importance and interestingness. For most of the twentieth century, scholars emphasized the virtues of robust, time-tested ideas (i.e., important theories). Then, the treatise “That’s interesting! Towards a phenomenology of sociology and a sociology of phenomenology”

by Davis (1971) became a staple in doctoral seminars, increasing the priority placed on capturing the attention of readers. The focus on interestingness persisted throughout the rest of the twentieth century (Mowday and Sutton 1993). For example, Staw (1995, p. 89) argued that “as research matures on any given topic, there should be a willingness to make more rash predictions.” The fondness for interestingness increased even further in the 2000s (Bartunek et al. 2006, Grant and Pollock 2011). During this period, researchers in literatures such as behavioral ethics and embodied cognition captured widespread attention by making surprising claims (e.g., standing outside a box leads to out of the box thinking).

Then, beginning in 2011, four sets of scholars argued that authors were overindexing on interestingness, thereby compromising importance (and other qualities, such as empirical rigor, that are outside the scope of this paper). The first argument relates to how the importance of a theory is often driven by the strength of its causal relationships. The fact that strong relationships (e.g., motivation predicts performance) are robust makes them feel pedestrian and thus unlikely to elicit the elusive “aha” response that scholars associate with epiphanic interestingness (Napier et al. 2009). Yet, critics argued that interesting ideas—such as double-edged sword theories featuring one pathway that opposes an established effect—often lack predictive power because they possess assertions that are tenuous, contingent, and sometimes absurd (Tsang 2022). An established pattern is established for a reason; when abstracted from the thousands of contingencies that modify and occasionally even reverse it, there remains a robust regularity. Any condition that attenuates it is, by definition, an exception that proves the rule. It may be more eye opening to argue that job satisfaction increases deviance than performance, but the impact of job satisfaction on performance is direct, whereas the link between job satisfaction and deviance is circuitous. In this way, critics lament that scholars who focus on fascinating exceptions to the rule ignore the importance of the rules themselves.

A second related argument is that interesting ideas are more likely to be incremental (Tihanyi 2020). Whereas important theories are time tested, interesting theories are often fleeting. As such, they cannot address large-scale and long-standing theoretical or societal problems, such as social inequality or “grand challenges” (George et al. 2016, p. 1880). By focusing on surprising insights, scholars overlook the importance of the often mundane details that underlie complex problems.

A third contingent of scholars bemoaned how the focus on clever ideas came at the cost of high-level theorizing (Pillutla and Thau 2013). Clever patterns often translate to sound bites that capture attention, whereas abstract ideation in the tradition of March and Weick

has the potential to be more abstruse. However, abstraction is essential for accumulating knowledge because it helps readers identify crosscutting patterns that unify ongoing research with prior work, thereby more reliably advancing the body of knowledge (Cronin et al. 2021).

A fourth group of researchers argued that the field was overindexing on a different source of interestingness: novelty. Alvesson and Sandberg (2011) argued that scholars often justify research by spotlighting a phenomenon that had never been studied before because of the assumption that revealing equates to revelatory (novel arguments are compelling because they are new). Yet, gap spotting fails to outline how a theory addresses a worthwhile problem (Locke and Golden-Biddle 1997). Further, theories are more likely to be novel when they depart from time-tested assertions. This is at odds with a theory’s ability to speak to stable patterns, thereby impairing its importance. Toward that end, a study of 17.9 million scientific articles revealed that the most influential ideas are conventional rather than novel (Uzzi et al. 2013).

Despite recent trends, the push away from interestingness has not continued inexorably (Thatcher and Fisher 2022). A lack of interestingness poses an existential threat even for scientists who attest that importance should be our preeminent consideration; if we do important research, but no one reads it, then we march toward obscurity. Accordingly, one contingent of scholars argues that it may not be worth sacrificing interestingness, even if it sometimes comes at the cost of other features of theorizing (Hidi 1990, Das and Long 2010).

There is also a trade-off between importance and actionability. An idea is more theoretically important when it is universal rather than context dependent (Bacharach 1989). But, this makes it harder to enact at any given moment (Gulati 2007). To paraphrase McGregor and Little (1998), important problems are so grand that they are far removed from schedulable actions. Important theories often change how people think but not how they behave. As such, many scholars call for theories that solve urgent problems (Gioia 2022). Conversely, some prominent voices—although not inherently opposed to practitioners using their theories—fear that overemphasizing application undermines their ability to identify the most central features of organizations (Daft and Lewin 2008, March 2013). This view assumes that actionable theories are often so narrow that they lack the evergreen quality that defines the most important paradigms. It is challenging to build ideas that are timeless and have widespread appeal when focusing on application (Kilduff and Kelemen 2001, Mathieu 2016). This dovetails with the argument that the goal of science should be description, not prescription, especially given that efforts to improve the world are often offset by unintended consequences (Merton 1936).

Finally, interestingness and actionability are at odds. Whereas interesting ideas often involve covariation between positive and negative concepts (e.g., a positive concept increasing a negative one), actionable ideas usually involve covariation among concepts of the same valence (e.g., a positive effect on a positive construct). What is provocative is often not what is practical.

Altogether, there is an “input-process-output” tension; the different phases of consuming an idea— noticing it, internalizing it, and taking action on it—are not only distinct but partly antithetical. In particular, what makes a theory significant makes it less likely to be consumed by scholars and used by the people (managers) and systems (organizations) that we study.

5.2.2. Trade-offs Among Generality, Simplicity, and Accuracy. The trade-offs among generality, simplicity, and accuracy are driven by logical constraints (Thorngate 1976). It is impossible to build a theory that is simultaneously general, simple, and accurate (Weick 1995). If a theory is general and simple, then it is parsimonious (it “explains a lot with a little”) but fails to capture the nuance of any single organization, industry, demographic, or era. This means that it lacks accuracy, reflecting the adage that “all theories are wrong” (at least over extended time horizons). For example, the main theory in research on personality asserts that many outcomes (e.g., well-being and job performance) can be predicted by only five personality traits; however, it glosses over the 17,953 ways that people differ from one another (Allport and Odbert 1936).

Alternatively, if a theory is general and accurate, then it will be so complex that it is difficult to digest (lacking in simplicity). Some agent-based models, systems theories, “nk” landscapes, and contingency models forsake simple typologies in lieu of sprawling theories that possess dozens of variables (Davis et al. 2007). For example, a theory that details how diffusion varies across geopolitical locations may precisely plot how organizations adapt their strategies to dozens of contexts (Guler et al. 2002), yet its complexity resists a concise thesis (Anderson 1999).

Finally, if a theory is simple and accurate, then it is tractable and anchored closely to how a phenomenon manifests in a specific context but lacks the reach necessary to speak to a wide range of circumstances (lacking in generality). To illustrate, theory on “dirty work” provides a rich description of how employees find meaning in their jobs; however, its insights are limited to employees in stigmatized industries and thus, do not generalize to jobs in most sectors, such as education, engineering, retail, logistics, media, and technology (Ashforth and Kreiner 1999).

Authors often confront these trade-offs when arranging ideas hierarchically via a “pyramid” or “hourglass”

writing style that starts broad and then narrows. Examples abound. Many authors deploy the resource-based view to examine concepts, such as capabilities, contracts, outsourcing, and emerging economies (Guillen 2000, Jain and Thietart 2014). Two broad categories of moral instinct can be broken down into five foundations; “individualized” foundations are care and fairness, whereas “binding” foundations are loyalty, authority, and purity (Haidt et al. 2009). Psychological distance can be decomposed into four rudiments— physical, temporal, social, and hypothetical (Trope and Liberman 2010). Kahneman (2011, p. 86) explained how the tendency to pay attention only to information in view (“what you see is all there is”) can be deconstructed into dozens of biases and heuristics. These examples underscore a fact of cognition; because people remember information hierarchically (Rosch 1978), a tiered writing style enhances coherence by packaging discrete constructs under inclusive categories. This improves narrative continuity, helps the reader retain core ideas, and enhances the rigor of a theory’s inclusion criteria (see Hardin et al. 2024 for an excellent example). It also highlights how a paper’s ideas link to broader themes (generality). Yet, this writing style decreases the average accuracy of the article’s concepts. Further, although it improves coherence, it compromises a distinct source of simplicity; readers must consider more concepts—not only the theory’s primary constructs but also the high-order categories that subsume them—as well as the connections between them.

5.2.3. Trade-offs Between Stages of a Theory’s Consumption and the Structure of a Theory’s Logic. As noted earlier, generality, simplicity, and accuracy provide the logical parameters within which authors can construct theories that are important, interesting, and actionable. This interdependence creates new trade-offs. As one example, generality and actionability are often at odds. Because highly general theories are inclusive of a wide range of phenomena, they are important but also often too abstract or too complex to translate to on-the-ground action. As another example, accurate ideas often lack importance. Because a theory must be closely tethered to a narrowly defined event to be accurate, its connection to wide-ranging ideas may not be straightforward, and in turn, its implications may not be of interest to a large audience. Another trade-off involves simplicity. It is unlikely that the same argument will promote multiple dimensions of strong theory. Thus, authors interested in maximizing a theory on many dimensions often have to complexify their theory— perhaps devoting one section to unpacking a pattern as accurately as possible, another to outlining an interesting moderator, and another to explaining why the insights are actionable. This undermines simplicity.

6. How the (Uneasy) Coexistence of Distinct Organizational Traditions Fuels the Fruitless Search for Unicorn Theories

The trade-offs between the six dimensions hinder authors. Scholars who are not aware of them may inadvertently weaken their theorizing while believing that they are strengthening it. Those who are aware of trade-offs will struggle to thread the theoretical needle by building theories that are as strong as the limits of logic will allow. Additionally, even scholars who manage to sidestep trade-offs when they first build their theories will encounter challenges when they attempt to publish their ideas. To understand why, it is helpful to consider our field's unique history. Organizational research is distinct from other disciplines because it has not one point of origin but about a half dozen, each of which has historically approached theorizing in a different way. A *tradition* is a cluster of scholars who share a similar style of theorizing. Scholars may belong to the same tradition for many reasons—perhaps because they share the same discipline, were trained by the same advisor, or are affiliated with the same institution. For example, the Carnegie tradition is composed of current and former faculty at Carnegie Mellon (including Linda Argote, Wesley Cohen, Richard Cyert, David Krackhardt, Daniel Levinthal, James March, Richard Nelson, Denise Rousseau, Gerald Salancik, Laurie Weingart, and Oliver Williamson) who followed Herbert Simon in building decision- and learning-centered theories of organizations, units, and teams (Gavetti et al. 2007). Traditions differ from topics (scholars from different traditions often study the same topics) and disciplines (e.g., many disciplines have an applied tradition).

Although the boundaries between traditions are sometimes blurry, they reflect meaningful differences in training and socialization. Scholars from each tradition have contrasting objectives and thus different interpretations of strong theory. I will now explain how each of the six dimensions of strong theory is emphasized vocally by at least one tradition and then consider a problem that emerges when so many distinct currents of thought (uneasily) coexist: although none of the individual traditions value all six dimensions, authors are often expected to build theories that excel in all respects because they must satisfy the aggregate preferences of reviewers from multiple traditions.

6.1. Traditions That Prioritize Importance

Scholars from several traditions are especially vocal that every theory should significantly advance knowledge. The first tradition arose when scholars from psychology, sociology, economics, and political science converged in the midtwentieth century at institutions including Berkeley, Carnegie Mellon, Illinois, Michigan, Stanford,

and Yale to examine the large corporations that predominated in the wake of World War II (Palmer 2006). Although some business school faculty members are rooted in only one of these four disciplines (a contingent that has recently grown), the convergence of multiple disciplines in one place spawned a tradition of scholars who study organizational topics, such as inequality and acquisitions, from an interdisciplinary perspective (Litchfield 1956). Scholars who take this approach must articulate how their ideas contribute to audiences that lean toward one discipline or another. The topic of power is illustrative. Whereas a sociologist may focus on structural bases of power and a psychologist can isolate the intrapsychic experience of power, an interdisciplinary researcher must identify implications for both audiences (Staw 2016). Scholars who span audiences and levels of analysis produce theories of greater import (DeCelles et al. 2019). Further, those who publish interdisciplinary research favor our field's "big tent" outlets, which, reflecting their multidisciplinary roots, have missions that call for far-reaching theories that boldly challenge received wisdom. Accordingly, interdisciplinary scholars tend to reject the style of research—incrementalism—that is *de rigueur* in traditions rooted in some disciplines, especially those that use laboratories to study focused questions (e.g., some areas in psychology and the life sciences).

Another reason that organizational scholars emphasize the dimension of importance stems from how conventions in methodology shape what researchers expect from any single publication. Interdisciplinary researchers, qualitative researchers, and scholars in macro areas (neoclassical economics, sociology, and political science) often conduct multimethod research and employ methods that are time consuming (Chatman and Flynn 2005). By contrast, scholars from micro literatures (psychology, decision theory, and behavioral economics) collect data more efficiently. Data collection for a multiwave field study or archival analysis may take two years, whereas for an experiment it may take an hour. This affects what scholars expect from a single article. Because scholars from qualitative, interdisciplinary, and macro traditions publish far fewer papers (Staw 2016), they expect more from each element of any paper, including its theoretical contribution.

A final tradition that prioritizes the dimension of importance is composed of researchers who publish theory without data. Many scholars who view organizations as layers of interconnected systems embrace the opportunity to unpack the complexities and contradictions of management without the constraints imposed by study designs and statistical analyses. The most well-cited publications of many thought leaders lack data. These scholars include Mats Alvesson, Blake Ashforth, Jane Dutton, Kathleen Eisenhardt, Robert Gibbons, Richard Hackman, Donald Hambrick, William Ocasio,

Jeffrey Pfeffer, Barry Staw, Karl Weick, and Birger Wernerfelt. Although these scholars also produce empirical work, their theory-only publications are especially influential. Some prominent scholars view stand-alone theory as essential, a sentiment embodied by the story that Jim March once challenged doctoral students to name a single iconic paper that had data (Alvesson et al. 2017).⁶ The standard that a theory paper must meet is daunting; if a paper contains only ideas, then readers will set a high bar with respect to what constitutes a significant contribution. The emphasis on the importance of a theory's contribution has socialized many organizational scholars to expect *all* theories—including those in empirical papers—to significantly advance knowledge.

6.2. Traditions That Prioritize Interestingness

Although experts in any subject often differ from nonexperts regarding what constitutes an interesting research question, scholars more strongly emphasize interestingness in traditions that focus on human behavior (e.g., organizational psychology). More than a half-century ago, Davis (1971) and Gergen (1973) popularized the idea that research about people should challenge assumptions and disabuse misconceptions. Whereas most audiences rarely observe events that are very small (e.g., chemical elements interacting) or very large (e.g., galaxies forming), they have extensive personal experience with how people behave at work. As such, they enter the study of behavior in organizations with skepticism about what counts as insightful and, in turn, may ignore an idea if it confirms rather than subverts expectations (Short 2009, Lilienfeld 2012). This is a key consideration given the shift from information scarcity to information abundance because readers now have more discretion concerning what they consume. Authors are, therefore, rewarded when they challenge conventional thinking, such as the prediction of Tewfik (2022) that a negative self-view (e.g., the belief that one is overestimated) can trigger positive outcomes by increasing one's motivation to work effectively with colleagues.

Although readers have lay theories about most features of the social world, their preconceptions are weaker for phenomena that they rarely encounter. Whereas a reader may have a clear intuition about whether emotional intelligence is helpful when leading a team, they are less likely to have strong prior beliefs about the impact of board interlocks on the diffusion of controversial healthcare practices. When audiences have little personal exposure to the topic of scientific investigation, authors feel less pressure to challenge deeply held assumptions. The upshot is that pressure to be interesting is stronger in micro than macro traditions (Oxley et al. 2010).

6.3. Traditions That Prioritize Actionability

Many organizational scholars begin with a real-world problem rather than a purely theoretical one and then

use scientific methods to solve it and study it at the same time. Applied traditions include industrial psychology, scientific management, human factors, labor relations, and government policy. These areas of research attained legitimacy in the midtwentieth century in part from faculty who helped outside stakeholders (e.g., practitioners and government officials) solve on-the-ground problems (Kozlowski 2017). For example, industrial psychologists at schools such as Maryland, Michigan, and Ohio State have received funding from institutions, including the military, that rely on management faculty for solutions to urgent problems, such as how to coordinate battalion teams in high-stakes situations. These external stakeholders exert palpable pressure in applied traditions. To illustrate, at least half of the attendees at the major annual conference for applied psychologists (Society for Industrial/Organizational Psychology) are practitioners (Latham 2019). As a result, scholars from applied traditions want research to solve problems in well-defined contexts, such as manufacturing and retail. This is especially the case for field experimentalists—a contingent that rose to prominence in the midtwentieth century (List 2007, Eden 2017). If a randomized design featuring a cost-effective managerial tactic—such as helping employees see the final version of a product that they worked on—yields reliable results in real organizational settings, then research can be seamlessly imported into the classroom or boardroom without sacrificing rigor.

Scholars with applied backgrounds are likely to self-classify not only as organizational scholars but as scholars of “management”—a term that is often paired with “organization” but has more prescriptive connotations. It speaks to the notion that our field is “a profession (academia) studying another profession (management)” (Corley and Gioia 2011, p. 22), wherein researchers can help the agents (managers) responsible for solving problems in organizations rather than studying organizations in a purely descriptive manner. Scholars who prioritize relevance lament that neighboring disciplines, such as behavioral economics, have more impact than organizational scholars on the actual practice of management because they focus less on high-level contributions and more on how individuals can proactively improve their circumstances (Fox and Sitkin 2015; see also Pfeffer and Sutton 1999, Boudreau 2004, Markides 2011). Addressing this limitation, a recent crop of organizational scientists has focused on identifying specific cost-effective behaviors—especially those that counteract problematic issues, such as deviance, poverty, and discrimination (Martin 2016, Arnett 2023, Preston et al. 2024).

By contrast, researchers from nonapplied traditions are often less concerned with actionability. These include scholars who extend theories related to complex social systems, such as institutional theory, population ecology, resource dependence, role theory, and

structural embeddedness (Pfeffer and Salancik 1978, Whetten et al. 2009, Aksom and Firsova 2021). Although these topics often translate to actionable insights, those who study them usually do not consider actionability a primary objective, at least at first (March 2013). Indeed, an effort to establish actionability can sometimes even detract from the ability to understand the multifaceted nature of certain phenomena (Montgomery et al. 1989, Goldberg 1996). Relatedly, some scholars prioritize description over prescription because interventions and policies can have unintended consequences or be rooted in inaccurate evidence (Gorwa and Guilbeault 2020).

6.4. Traditions That Prioritize Generality

At first, it may appear that traditions that prioritize actionability will not be amenable to general theories. To be sure, general theories, such as those that feature universal laws or exhaustive typologies, are concentrated in highly paradigmatic fields, such as economics, genetics, physics, and cognitive science (Bosco 2022). But, although most applied fields (e.g., addiction, exercise science, and social policy analysis) favor circumscribed theories tailored to real-world problems, some also feature general theories. Three examples are pathology, clinical psychology, and organizational research. Similar to the unified frameworks in pathology (e.g., the germ theory of disease) and clinical psychology (beginning with Freud's psychoanalytic theory), many organizational scholars build theories about features common to most or all organizations, including jobs, functions, routines, reporting channels, product development, information systems, and institutional memory (Stokes 2011). This signifies that the distinction between basic and applied disciplines is a false dichotomy; although organizational research solves applied problems, it qualifies as a discipline in its own right (Daft and Lewin 1990, Augier et al. 2005, Corley and Gioia 2011, Gooday 2012). Unlike fields that import knowledge from "parent" disciplines (e.g., developmental economics), organizational research pertains to a phenomenon—organizing—that is widespread and exists independent of science and scientists. By extension, it behooves scholars to catalog universal features of organizing rather than repurpose ideas from other fields to solve applied problems (Heath and Sitkin 2001).⁷

The approach of scholars who study universal phenomena is reinforced by the close link between organizational research and neighboring disciplines. Some of the most influential organizational scholars received their doctorates in other fields, including political science (e.g., James March and Herbert Simon), sociology (e.g., Paul DiMaggio and Michael Hannan), economics (e.g., Ronald Coase and Oliver Williamson), and psychology (e.g., Teresa Amabile, Richard Hackman, Gerald Salancik, and Karl Weick).⁸ Rather than viewing organizations as a context to apply concepts from their home disciplines, they

imported their disciplinary mindset and tool kit to refine organizational science as its own field (Katz and Kahn 1978, Weick 1979, Fligstein 1996). Whereas an applied scientist might focus on one location or industry (e.g., healthcare), these scholars identify tenets of organizational behavior, systems, and competition that are universal—that is, robust to context (Reinecke et al. 2024).⁹ Relatedly, many scholars trained in business schools examine how universal features of organizations are shaped by forces, such as environmental shocks or psychological biases, that were historically studied in other disciplines (Pfeffer 1981, Phillips and Loyd 2006, Rosette et al. 2008, Staw 2016, Klüppel et al. 2018).

Some universities have a concentration of faculty who build highly general theories. The University of Illinois is one example. Its current and former faculty members have produced integrative theories on topics relevant to almost all organizations, including power (Salancik and Pfeffer 1974), conflict (Murnighan and Roth 1977, Pondy 1992), organizational identity (Albert and Whetten 1985), managerial cognition (Staw 1995, Dane and Pratt 2007), work teams (McGrath 1984), job design (Hackman and Oldham 1976), group diversity (Joshi and Roh 2009), resource allocation (Northcraft and Neale 1986), and the economic foundations of strategy (Mahoney 2004). Although an increasing number of business school faculty publish in social psychology, sociology, and economics on topics unrelated to organizing, managing, and work, the legacy of scholars who were trained in other disciplines and then built seminal organizational theories continues to redound to the benefit of the field. These scholars not only helped establish organizational studies as a stand-alone discipline on both the micro and macro sides but trained students who populate the faculty of management departments all over the world (Staw 2016).¹⁰

6.5. Traditions That Prioritize Simplicity

Many organizational researchers highly value simplicity. This can be understood by considering a common theme in most of the social sciences. Whereas scholars in the natural sciences and computational sciences—especially machine learning and artificial intelligence—sometimes build models with millions or billions of causal relationships, most social science theories have fewer than seven concepts (Ashforth 2021). This distinction stems from a difference in objectives. The natural sciences and computational sciences value prediction, which can be improved with increasingly complex theories (Baum and Haveman 2020). As a result, simplicity is often a limitation (Hofman et al. 2021). By contrast, the social sciences focus on explanation, which involves helping audiences understand how, why, and when events occur (Whetten 1989). Although there are exceptions, such as the examination by McGahan and Porter (1997) of the impact of industry on firm profitability—

an analysis that does not directly speculate about how or why industry drives one fifth of firm profits—most organizational research unpacks the underlying mechanisms that explain key outcomes of interest. In some cases, scholars do not predict a new cause-effect relationship but instead unearth new mechanisms that explain a known relationship, such as the effort of Husain et al. (2024) to advance a novel reason why employees accept lower wages in social impact contexts. It is easier to explain a phenomenon via a simple theory because it abides by the limited amount of information that readers can keep in mind at any one time (Miller 1956).

Because it is impossible to build theories that are simultaneously general, simple, and accurate, scholars who value simplicity must decide how to balance accuracy and generality. Those who prioritize accuracy build theories that are grounded in specific regions (e.g., Japanese firms' investment strategies) (Henisz and Delios 2001), roles (e.g., union leaders, CEOs), industries (e.g., the semiconductor industry), and processes (e.g., internal promotions, socialization) (Bidwell 2011, Deng et al. 2025). Other researchers combine simplicity with generality. One approach to doing so is to examine concrete phenomena, such as helping, stereotyping, acquisitions, bargaining, and decision making, in a fashion that assumes that they are manifest in a similar way regardless of physical or social context (i.e., universal and concrete).¹¹ These scholars are often trained to publish in core disciplines, such as social psychology.

Other traditions strive for even more generality by building simple theories that are not only universal but abstract. Rather than focusing on phenomena that are concrete, they “lump” multiple concrete mechanisms within each individual construct and tie their theory to broader principles. Examples include Rothbard (2001), who integrated several discrete mechanisms within each construct in her theory on work engagement; Ouchi (1980), who aggregated all forms of organizational control into three categories (markets, bureaucracies, and clans); and the diversity typology of Harrison and Klein (2007), which distills all forms of diversity to three types (e.g., disparity subsumes all hierarchical differences, including power, status, class, and rank).

Combining simplicity and generality (i.e., parsimony) is more common among faculty who publish in management journals than those who publish in disciplinary outlets. Examples include research on hierarchy (Schouten et al. 2024), creativity (Berg 2016), trust (Mayer et al. 1995), workplace aggression (Ferris et al. 2016), and mergers and acquisitions (Feldman and Hernandez 2022).¹² Although parsimony is valued in organizational behavior, dominant theories in some macro literatures (e.g., agency theory and the resource-based view) are even more abstract, moving beyond “midrange” frameworks and toward paradigms that unify multiple literatures.

Scholars build these highly parsimonious theories in a variety of ways. To illustrate, Gibbons (2005) attributed the origins of firms to four formal theories (incentive systems, property rights, adaptation, and rent seeking), whereas Zenger et al. (2011) advanced a theory of the firm that while also integrating four perspectives (market failure, organizational virtues, market virtues, and organizational failure), focused on theories that are informal.

Not all organizational researchers venerate simplicity. Scholars who build contingency models and systems theories often deprioritize simple theories in favor of highly complex ones as do some formal theorists and researchers who conduct simulations. For instance, the model of Cowgill et al. (2025) of how leaders assign employees to jobs is far more nuanced than traditional theories on task allocation in organizational behavior. The literature on teams also provides informative contrasts. Whereas verbal theories of team processes typically have less than 20 variables (Gladstein 1984, Hackman 1987, Marks et al. 2001, Woolley 2009), computational models of teams often have an order of magnitude more. One example is a computational model that charts the way that four interpersonal mechanisms (e.g., frustration) affect how team members move through three stages of conflict in a crossed design with 181×181 conditions (Somaraju et al. 2024). This approach is common among current and former faculty at Michigan State, including Richard DeShon, Steven Kozlowski, and Frederick Morgeson. As former editors of top journals in the field, their preferences are influential.

6.6. Traditions That Prioritize Accuracy

Some researchers value accuracy as an outgrowth of their emphasis on actionability. Although actionability is orthogonal to accuracy, actionable solutions tend to be more accurate because they involve behaviors that can be visualized and implemented (Graebner et al. 2023). Accuracy is also emphasized by scholars who value ecology—that is, studying a phenomenon in its natural state (Zilber 2020). Researchers from this tradition typically build theories inductively. Even when they ratchet up their analysis from raw data to more coarse-grained theory, they often retain a significant degree of context. Examples include theories on the upward mobility of individuals from Brazilian favelas (Pongeluppe 2024) and the physical mobility of digital workers (Prengler et al. 2024). Given the upside of vivid contexts for enriching theorizing with more accuracy, some scholars have called for management theory to incorporate more of it (George 2016). The value of context is especially pronounced for researchers who study specific industries (healthcare or technology) (Kapoor and Argawal 2017, La Forgia 2023, Shen et al. 2023) and organizational forms (e.g., hybrid organizations, start-

ups, nonprofits) (Hsu 2006, Piekkari et al. 2022). To illustrate, Felin and Zenger (2009) explain how entrepreneurs develop beliefs differently than other businesspeople, often relying more on imagination and reason than first-hand experience.

Finally, traditions with an empirical focus place a strong emphasis on accuracy in their up-front theorizing. Paper introductions in traditions with a strong quantitative underpinning (e.g., behavioral science, applied psychology, and labor economics) are typically short as authors forsake high-level conceptualization in favor of constructs that are clearly delineated and closely tied to observable indicators. Examples include hiring, safety, divestments, sleep deprivation, and patent creation. To be clear, scholars who study these topics almost always build theories with constructs prior to introducing statistical models with measures. Even highly concrete constructs, like patent creation, are theoretical insofar as they have multiple observable indicators and vary in important ways across samples, industries, and time periods. However, the gap in abstraction between constructs and measures is relatively small.

7. When Traditions Collide: The Review Process as a Collective Action Problem

As shown in the appendix, the traditions that call for more accurate and actionable theories have recently grown in size and influence. For example, the most recent editors-in-chief of *Academy of Management Journal*, *Administrative Science Quarterly*, *Management Science*, and *Organization Science* have all explicitly called for theory with clear practical applications. This provides a counterweight to an extended period when editors focused primarily on importance, interestingness, generality, and simplicity.¹³ However, these latter four dimensions still have a large number of vocal advocates. The result is that the six dimensions are endorsed in a more balanced fashion now than in prior eras. An analysis of data in the appendix establishes that the total number of dimensions that authors of commentaries collectively endorse has increased over time ($r = 0.19$, $p = 0.002$).¹⁴ This may reflect movement toward an equilibrium; even if each tradition has matured at a different rate, they have all now attained a stable degree of clout. As such, a homeostatic solution may be one in which all six dimensions are championed by vocal subgroups with comparable power.

The balance of power among traditions has a critical implication for publishing: scholars now feel more pressure to generate theories that excel in every way. Among different groups involved in the knowledge generation process (e.g., author teams, review teams, and dissertation committees), scholars are most likely to

face these expectations from review teams. To be sure, some review teams are composed of scholars who overlap with respect to how they prioritize the six dimensions. However, because reviewers are not self-selected, review teams are characterized by less homophily than other types of groups (e.g., author teams). Reviewers with expertise in the same topic (e.g., inequality or knowledge production) may belong to different institutions, networks, or subdisciplines. As such, authors whose traditions do not value a particular dimension (e.g., actionability or interestingness) are often assigned a reviewer from a tradition that prizes it—especially in big tent journals. This is compounded by a growing number of researchers who straddle the boundary between more than one tradition. Although scholars who span boundaries may appreciate the specter of trade-offs, they also see value in a greater number of dimensions. A reviewer at a school with a strong applied tradition may expect actionable theories, yet if this same reviewer holds a doctorate from an institution where scholars publish in economics journals, then they may also be socialized to value generality and simplicity. When individual reviewers endorse more dimensions, the odds are greater that the aggregate preferences of multiple reviewers will force authors to confront difficult trade-offs.

These realities are likely to be compounded by other features of review teams. Whereas members of other groups (e.g., author teams) influence each other's views over many years, the short-term nature of review teams means that reviewers with different predilections are unlikely to converge. Additionally, review teams are characterized by relatively flat hierarchies. Even if some reviewers are higher in status than others, the anonymity of their identities mutes the review team's common knowledge of this status and in turn, the extent to which other reviewers defer to them. Further, it is inherent to the role of reviewing to voice one's viewpoint rather than defer. The lack of power distance extends to editors. Compared with reviewers, editors have more of one base of power (authority) but less of another (domain-specific expertise) (French and Raven 1959). They also know author identities and want to avoid bias. Thus, even though they serve as the final arbiter, editors often defer to reviewers. Latham (2019) observed that since the mid-1980s, editors have been less willing to instruct authors to ignore certain reviewers. Instead, they typically expect authors to respond to all reviewer comments.

If each reviewer's comments are treated as prerequisites, then authors will be thrust into an unwinnable position because the existence of the above-summarized trade-offs between the six dimensions means that authors cannot possibly build theories that are superlative on all (or even most) dimensions. Authors are led on a fruitless search for unicorn theories, forced to make

choices that will alienate some reviewers.¹⁵ This breeds inefficiency. It also fosters resentment among authors who invest years in their work, only to be denied publications based on contradictory criteria (King and Lepak 2011). In short, the review process constitutes a collective action problem; even if it is reasonable for each reviewer to request certain changes, authors are stifled by the expectation that they must satisfy every reviewer concern.¹⁶

8. Guidelines for a More Sustainable Model of Theorizing

Given that all six dimensions are valuable, yet it is not possible to optimize all of them at once, I argue that authors, reviewers, and editors should adopt two objectives. When building any single theory, the status quo is that the aggregate preferences of reviewers cause each dimension to be a necessary and insufficient criterion. I argue for a reversal; each dimension of theoretical strength should be treated as an unnecessary and potentially sufficient criterion. No single dimension is required, and although it is ideal to maximize as many as possible, a theory can be considered sufficiently strong if it maximizes a subset or in some cases, only one of the six dimensions. By contrast, when taking a big picture perspective by considering multiple theories on the same topic (i.e., a program of research), scholars should treat each of the six dimensions as necessary criteria. The best way to follow through on this strategy is to build a series of related theories that collectively satisfy all six dimensions, a step that will require scholars to take stock of existing theories on a topic and compensate for omissions (e.g., a series of highly applied theories can be followed by a parsimonious one that links them all together). When authors do this, each paper is less likely to be redundant with the author's existing work and more likely to address a need within the author's program of research and the broader literature. This twofold approach addresses the unique hazards and opportunities of a field with many traditions. Namely, it mitigates a key downside (unrealistic expectations for any single paper) and harnesses a benefit (scholars are incentivized to strengthen their research programs on a greater number of dimensions when compared with disciplines that have fewer traditions).

An analogy for this two-pronged strategy is validity. Although validity involves the link between theory and empirics, whereas the arguments in this paper relate only to theory, there is an important similarity; just as it is not possible to maximize all forms of validity in any single study (e.g., an experiment may establish internal but not external validity), it is impossible to maximize all forms of theoretical strength in any one theory. Additionally, in the same way that all forms of validity add unique value at a holistic level, all six dimensions add

unique value when judging a program of research. Thus, just as the strongest papers combine multiple methods to optimize all forms of validity, the strongest research programs combine various theories that balance all six dimensions. This can address the criticism that scholars underemphasize how individual papers contribute to broader programs of research (Cronin et al. 2022).

In sum, scholars should aim to do less within theories and more across theories. In the next two sections, I consider how these two recommendations can be enacted by authors, reviewers, and editors. The 12 guidelines summarized in Table 2 can help address the reality that scholars currently fall victim to diversity of thought when it hurts them (for each individual paper in the review process) and fail to exploit diversity of thought when it could help them (over the long term).

9. Emphasizing Fewer Dimensions Within Each Individual Theory

Authors should exploit instances when it is possible to maximize more than one dimension (Table 2, author guideline 1). For example, although generality is not required to boost importance, it can be a helpful contributing factor. One way to bolster importance via generality is to examine ideas that are often studied separately but have points of overlap, such as the possibility that promotive voice and idea generation both fall under the rubric of creativity. But, authors will quickly exhaust their ability to augment multiple dimensions at once. Further, trade-offs will be unavoidable. Accordingly, authors should be explicit about the costs associated with how they have chosen to handle trade-offs (Table 2, author guideline 2). To illustrate, authors who have chosen to build a parsimonious (simple and general) theory can explicitly acknowledge that they had to omit edge cases and exceptions (i.e., forgo accuracy).

Because of the constraints they face, the onus should not solely be on authors to navigate trade-offs. To that end, my six-dimensional framework can also help reviewers position authors for success. As a first step, reviewers can make requests related to fewer dimensions (Table 2, reviewer guideline 1). For example, rather than suggesting that authors reduce the number of hypotheses (increase simplicity) and tie their theory more closely to their context (increase accuracy), a reviewer can focus on just one of these issues. This reduces the likelihood that the aggregate effects of different reviewers' concerns will put authors in an unwinnable position. If a reviewer who expects a simple and accurate theory is paired with a reviewer who prefers a general theory, then the authors will have to resort to playing an unwinnable game of "theoretical whac-a-mole," in which each step that they take to improve one

Table 2. Recommendations for Authors, Reviewers, and Editors

Role	A single article (one theory)	Program of research (a collection of theories on the same topic)
Authors	Author Guideline 1: Exploit complementary dimensions (i.e., when strengthening a theory on one dimension strengthens it on others). <i>Examples:</i> Pair generality with importance; pair accuracy with actionability; pair simplicity with importance and interestingness. Author Guideline 2: Be explicit about the rationale for navigating a trade-off in a particular way. <i>Example:</i> When building a highly parsimonious theory (a theory that is extremely simple and general), acknowledge that edge cases must be omitted (reduced accuracy).	Author Guideline 3: From one article to the next, retain dimensions that are valued by your local tradition while strengthening at least one that is unconventional. <i>Examples:</i> Scholars trained in social psychology retain simplicity while increasing generality; qualitative researchers retain accuracy while increasing actionability. Author Guideline 4: Read journals and attend conferences in other disciplines to understand how your core topic is studied elsewhere. <i>Example:</i> A networks scholar trained in organizational behavior attends a session on networks at the annual conference of the Strategic Management Society.
Reviewers	Reviewer Guideline 1: Request fewer dimensions. <i>Example:</i> If a reviewer believes a theory would be stronger if the authors incorporated one industry into their theory (greater accuracy), then they should either refrain from asking the authors to also make the theory more general by tying it to a broader theoretical framework (greater generality) or acknowledge that they are comfortable with the authors building a more complex theory (reduced simplicity). Reviewer Guideline 2: Instead of viewing the six dimensions as necessary and insufficient, view each dimension as unnecessary and potentially sufficient. <i>Example:</i> If a theory integrates two literatures (generality), a reviewer should not require that the authors focus on practical implications (actionability), even if actionability is a highly valued dimension in their home tradition.	Reviewer Guideline 3: If an undervalued dimension is a possible complement (i.e., it will enhance other dimensions), then suggest that the authors enhance it, but do not require that they do so. <i>Example:</i> For a theory mapping the five key causes of firm profitability (an important theory), reviewers can suggest (but not require) that the authors also enhance the theory’s reach (generality) by couching arguments in a broader paradigm, such as the attention-based view of the firm. Reviewer Guideline 4: Avoid raising the same issue in different reviews if it consistently leads papers to become weak on the same dimensions. <i>Example:</i> A reviewer should avoid consistently requesting that authors increase interestingness because doing so may inadvertently cause most of the papers that they review to become less important or actionable.
Editors	Editor Guideline 1: Point out tensions between reviewer requests. <i>Example:</i> If reviewer 1 wants a smaller gap between constructs and measures (more accuracy), reviewer 2 asks the authors to broaden the paper’s contribution (more generality), and reviewer 3 requests fewer constructs and greater coherence (more simplicity), then the editor can acknowledge that the authors cannot satisfy all requests. Editor Guideline 2: Recommend specific paths. <i>Example:</i> If an editor’s understanding of the journal’s mission suggests that papers would benefit from more actionability, then they may recommend that the authors increase actionability, even if doing so comes at the expense of importance or interestingness.	Editor Guideline 3: When editors-in-chief call for authors to prioritize an overlooked dimension, they should reinforce the need to balance all six dimensions over the long term. <i>Example:</i> A new editor-in-chief calls for more important theories but reminds authors not to entirely abandon interestingness, actionability, and accuracy, which are often sacrificed when authors emphasize importance. Editor Guideline 4: Codify the six dimensions. <i>Example:</i> Editors-in-chief may consider presenting the six dimensions in a digestible format when onboarding reviewers, in part by tweaking the journal’s reviewer form so that reviewers can explicitly evaluate each paper’s theory on the six dimensions (see Table 3 for an example).

dimension compromises another, inevitably alienating at least one reviewer.

Even if each individual reviewer focuses on only a single dimension of strong theory, conflicts between reviewers will still occur. Each reviewer can only control how they react when their own requests clash with those of others. Because it is not possible to establish that one dimension is objectively superior to the others, reviewers can contribute substantial value by making concessions (Table 2, reviewer guideline 2). A reviewer who wants an author team to build a simpler and more accurate theory about the role of institutional logics in the semiconductor industry may consider conceding their preferences if, for example, feedback from other referees would make it easier for the author team to build a simple and general theory that focuses on

features of institutional logics that are universal (i.e., not anchored to one industry). Reviewers who adopt this approach are not lowering their standards. Rather, they are remaining open to the value of other criteria in a system that does not permit all criteria to be satisfied (consistent with the mantra that none of the six dimensions are necessary and that each dimension may be sufficient).

Like reviewers, editors can help authors avoid trade-offs (Table 2, editor guideline 1). If one reviewer wants a theory to be more accurate and another wants it to be more general, then an action editor may suggest that the authors trade off on some of the theory's simplicity to satisfy both reviewers. In other cases, editors can help authors and reviewers recognize the presence of conflicting reviewer requests and suggest how to navigate them. Although there are many good reasons for editors to defer to reviewers, it may be valuable for editors to exercise more power if they believe that one approach to navigating a trade-off is superior given their unique knowledge of the authors, reviewers, and the journal's mission. Currently, there is significant variation across journals with respect to how much latitude action editors possess to make editorial decisions. Editors-in-chief at all outlets—including those where policies and norms have historically given action editors less discretion to go against reviewers—can encourage action editors to point out when reviewer requests are in tension and then recommend ways that authors can navigate trade-offs (Table 2, editor guideline 2). This includes explicitly deprioritizing reviewer requests when doing so is the only frictionless path to publication for a worthy paper.

10. Emphasizing Distinct Dimensions Across Different Theories

Although it is not possible to optimize all six dimensions in a single theory, it is possible to do so when developing a program of research. Yet, this rarely occurs. The reason can be traced to the previously discussed distinctions between review teams and author teams. Whereas review teams consist of a relatively diverse group of scholars who do not interact, author teams are groups of self-selected scholars who share similar mental models of theory that are reinforced across years of interaction. This has important implications for how authors develop research over time; even if author teams accommodate reviewers when developing each individual theory, they are likely to resolve trade-offs in a similar way across publications. Thus, although the field overall is composed of traditions that collectively balance all six dimensions, author teams within each tradition tend to consistently focus on the same subset of dimensions.

The balance of power that has recently emerged among organizational traditions exacerbates this. When

different traditions are forced to be interdependent (e.g., in the review process at big tent journals), authors experience pressure from all sides as a function of dissensus. Yet, when the traditions are independent—as is the case with most scholarly interactions—authors can retreat to the safe enclave of their own tradition. Each scholar is surrounded by a critical mass of colleagues who reward and reinforce their preferred way of thinking. When authors only engage with other traditions during the review process, they do not deeply internalize different ways of theorizing. This problem is compounded when scholars publish in specialty and disciplinary journals rather than big tent management outlets. Although the review process at specialty and disciplinary journals is more predictable because authors are subject to a less diverse set of viewpoints, it exacerbates field-wide fragmentation. Authors may become so comfortable with how those in their orbit build theories that they misunderstand or even look down upon researchers who value different dimensions than they do (Staw 2016). This is a damning state of affairs given that scholars from different traditions often study the same topics (e.g., inequality), and as such, there is no *prima facie* reason for them to remain isolated in clusters that each approach theorizing in an isomorphic way. The upshot is that whereas review teams experience a collective action problem, the field as a whole is plagued by a different social dilemma—one that resembles a tragedy of the commons, in which knowledge does not accumulate over the long term to benefit the field (Cronin et al. 2022). To help redress this social dilemma, I again provide recommendations for authors, reviewers, and editors; however, in this case, I examine how they navigate programs of research rather than individual theories (see Table 2, column 3).

Authors face a challenging tension. By delimiting theories to their “home” tradition, they carve out a clear audience, exploit their expertise, distinguish between primary and secondary ideas, and enjoy predictable collaborations. But, authors who restrict themselves to one tradition will consistently prioritize the same subset of the six dimensions. This is limiting. I propose a countermeasure; when building each new theory, authors can emphasize a subset of dimensions that are valued within their local tradition while also including at least one dimension that is often overlooked (Table 2, author guideline 3). This enables them to adhere to the preferences of scholars in their core area while strengthening each new theory in a different way. Consider research on justice, which is studied differently in strategy (Gartenberg and Zenger 2023), organizational behavior (Colquitt and Zapata-Phelan 2007), and social psychology (Lind et al. 1997). Some justice scholars build theories that emphasize simplicity and interestingness—such as a theory predicting that procedural justice decreases affective commitment (an ostensibly “good” behavior

backfiring). An author team from this tradition can pivot by retaining simplicity while also prioritizing actionability and accuracy, even if it requires them to forsake some interestingness. For example, they may argue that leaders who speak last in meetings elicit more participation from direct reports, a prediction that is still simple but features more concrete and actionable ideas. Although authors may have to temporarily relinquish a dimension coveted by their local tradition, they support the sustained vitality of their topic. When each new theory excels on a different subset of dimensions, their program of research appeals to more audiences.

How can authors learn to build theories that excel on dimensions overlooked by their home traditions? Because organizational research resides at the nexus of multiple traditions, authors can locate many journals and conferences in areas adjacent to their own (Table 2, author guideline 4). For example, the topics of justice, diversity, conflict, competition, inequality, and groups are all studied by large contingents of scholars in the Academy of Management, the American Sociological Association, INFORMS, the Society for Industrial and Organizational Psychology, the Society for Personality and Social Psychology, and the Strategic Management Society. By reading journals and (contingent on available funding) attending conferences associated with more than one tradition, scholars learn that there is no single best way to theorize about a given topic. They can more readily experiment with new combinations of dimensions, such as one paper that features a simple and actionable theory and another that is general and accurate. Given the nature of specialization and categorization, most scholars will benefit by continuing to write for their home tradition rather than multiple traditions. However, exposure to other traditions can help scholars understand how to retain a narrow focus in terms of *what* they study while adopting a more expansive understanding of *how* to study it. Research on multiteam management (MTM) is an exemplar in this regard. MTM scholars have consistently varied the six dimensions since the literature's inception (DeChurch and Marks 2006, Lanaj et al. 2013, de Vries et al. 2016, Zaccaro et al. 2020). Many of them have built their capacity to think expansively about theory by frequenting conferences with scholars who study teams in various ways. For example, attendees of the annual conference for the Interdisciplinary Network of Groups Researchers span eight disciplines (ingroup.net).

Reviewers, especially board members, witness entire literatures evolve as papers move through the review process. Because of their longitudinal perspective, they will have a sense of what types of theories predominate in a given area (e.g., simple and interesting versus general and important). This can help them orient authors toward dimensions of strong theory that are often overlooked. One way that reviewers can do this is to

encourage authors to exploit complementary dimensions (Table 2, reviewer guideline 3). If, for instance, a literature is populated by accurate theories, then reviewers can encourage author teams to incorporate actionable predictors or moderators that allow a manager to “switch on” or “switch off” effects. (Of course, reviewers should not require that authors add a dimension as that would increase the likelihood of conflicts with other reviewers.) Another guideline pertains to how a reviewer will often raise the same concerns to different author teams without realizing the costs of doing so. If, for example, the same reviewer asks different author teams to expand the breadth of their theorizing, then they may reinforce the tendency for some dimensions (e.g., generality) to come at the expense of others (e.g., actionability). Reviewers should thus be leery of recurring concerns (Table 2, reviewer guideline 4). In this respect, they should disentangle features common to all strong theories (e.g., internally consistent logic) from those that may merely reflect their taste.

As the primary stewards of journals, editors are arguably the field's most influential thought leaders. Editors often urge scholars to rethink what constitutes strong theory, such as their recent calls for more actionable research. Efforts to effect change are pronounced during periods of succession when new editors arrive with a distinct long-term vision. Accordingly, editors are likely to shape how scholars generate not only their next paper but entire research programs. When an incoming editor recognizes that the field is out of equilibrium such that certain dimensions of strong theory have been unduly deprioritized, they are justified in prescribing those overlooked dimensions. But, when issuing calls to action, editors should acknowledge that moving toward one dimension of strong theory will necessitate moving away from others (Table 2, editor guideline 3). One example of an editorial that acknowledges these trade-offs is the imperative of Tihanyi (2020) to make readers say “that’s important” instead of “that’s interesting.” This type of approach can help attain—or restore—an equilibrium among the six dimensions if the field is ensnared in a period in which some dimensions are being reliably underemphasized. Yet, the pendulum must not swing too far in any direction. Continuing the above example, a wholesale deprioritization of interestingness may be unwise over the long haul because it could undermine readers’ willingness to consume new ideas. To avoid overcorrections, editors can reiterate the need to strike an even balance among the six dimensions over the long term.

This leads to a final question. How can editors help authors and reviewers balance all six dimensions over time? One way is to codify the six dimensions in the forms that reviewers complete when assessing manuscripts (Table 2, editor guideline 4). Editors-in-chief can provide a brief description of each dimension, examples

Table 3. An Example of Reviewer Guidelines for Evaluating the Six Dimensions of Strong Theory

Dimension	Definition	Examples (not an exhaustive list)
Importance	Provides insight into a significant problem or widely studied topic	Identifies robust causal connections between widely studied concepts; provides long-lasting solutions for unresolved questions; sheds new light on the underlying processes associated with key social phenomena; carries implications for many scholars
Interestingness	Stirs curiosity and a desire to share ideas with others	Contains a thought-provoking revelation that challenges well-established conventions; reveals a novel insight that readers do not expect—often by reversing an established pattern or identifying a novel exception to an otherwise robust pattern
Actionability	Can help solve urgent, real-world problems	The theory features behaviors or policies that managers can enact or adjust in a cost-effective manner to improve their organizations or the circumstances of organizational members; the theory addresses time-sensitive problems
Generality	Insights pertain to a broad array of topics, contexts, and phenomena	A theory that unifies a number of existing concepts within one paradigm; a typology that maps a wide range of phenomena; a contingency framework that comprehensively details a wide range of scenarios
Simplicity	Clear, digestible, and easy to understand	A theory with 4–6 constructs and 2–4 core predictions (i.e., causal relationships); a theory that is centered on a consistent theme and coherent set of ideas
Accuracy	Precisely captures the nuance of behaviors and events	Defines constructs at a low level of abstraction; richly details mediating processes (including parallel and serial mediating pathways); unpacks contingencies and edge cases

Notes. This table focuses on a paper’s theoretical contribution. Reviewer forms will also include items related to a paper’s empirical rigor, appropriateness for the journal, and other issues deemed important by the journal’s leadership. As with any framework designed to be simple and general, the examples of each dimension are intended to be illustrative rather than exhaustive. Possible instructions for reviewers are as follows. Please assess the article’s theory on the following six dimensions from one (weak) to seven (strong). Your scores should reflect the reality that a paper’s theory cannot excel on all six dimensions because of trade-offs, including but not limited to the following. (1) Because theories that are more important often feature robust and timeless patterns, they may be less surprising (interesting) and less likely to solve immediate problems (actionable). (2) Whereas interesting theories often involve countervailing effects (e.g., when a positively regarded action decreases a positive outcome), theories that are more actionable often involve covalent effects (e.g., when a positively regarded action increases a positive outcome). (3) Theories can only be maximized on two of the following dimensions: generality, simplicity, and accuracy.

of how to assess them, and a note about trade-offs.¹⁷ I provide an example in Table 3. This represents a trickle-down model of editorial leadership; if reviewers formally incorporate the six dimensions into their assessment of articles, then authors will be incentivized to consider all of the dimensions—and the hydraulic tensions among them—when embarking on new projects. Another advantage of this approach is that it would give editors hard data to determine whether some dimensions have been underemphasized over time.

11. The Link Between Theory and Empirics

The six dimensions can shed light on the interplay between theory and data. Because constructs that are general and simple (parsimonious) are so inclusive, the odds that any indicator fairly represents a construct (face validity) are higher (Astley and Zammuto 1992). But, it is more challenging to attain content validity (when indicators map onto the entire construct), and the gap between constructs and measures is larger, with more layers of intervening abstraction. Thus, even clear

statistical patterns may not lead to straightforward conclusions. It is also harder to collect enough data to test more general claims and, correspondingly, easier to omit key explanatory forces, increasing the possibility of confounds or spurious relationships.

As theories become more accurate (e.g., more closely tailored to specific industries, organizations, and groups), the gap between concepts and measures becomes smaller (Donaldson 2022). There are fewer observable referents available, but the process of identifying them is more straightforward. It is thus easier to build reliable measures and achieve content validity. This can help researchers make inferences when patterns from statistical tests are inconsistent (e.g., testing the same hypothesis in two separate studies and getting two different results). Although this approach increases confidence in the theoretical conclusions drawn from empirical data, it requires scholars to forsake either generality or simplicity. There are still other risks. When scholars build detailed theories after conducting analyses—even when doing so transparently—they are more likely to generate propositions that are tethered to the idiosyncrasies of their data and therefore unlikely to generalize (i.e., overfitting).

Thus, the descriptions of their data may be accurate; however, their broader theory may be inaccurate.

Finally, the six-dimensional framework can benefit qualitative research. Inductive scholars often ground their theorizing in a specific phenomenon (e.g., remote work during the COVID-19 pandemic) and strive to retain a plausible chain of inference by minimizing the distance between their data and theory (Rahman et al. 2024, Yang and DiBenigno 2025). As a result, their theories privilege accuracy in that they are likely to be rooted in a specific era, event, group, or industry. This can bolster interestingness because readers find nuanced, concrete portrayals of unfolding events gripping—similar to storytelling (Patient et al. 2003, Heath et al. 2007, Pollock and Bono 2013). But, an emphasis on accuracy can be limiting. It may sometimes be worthwhile to also increase either a theory’s simplicity (by keeping only the most novel and compelling concepts) or a theory’s generality (by increasing the range of time, industries, or populations that the theory covers) given that accuracy can coexist with one of these two dimensions. Additionally, qualitative scholars can prioritize concepts that are actionable given that actionable concepts are often accurate.

12. Conclusion

The six dimensions of strong theory can demystify and systematize the elusive concept of “theoretical

contribution.” My “theory of theory” can improve our collective understanding of the trade-offs that make it impossible to develop the unicorn theories that review teams often expect. In the same way that scholars are trained to understand that no single study can possess all forms of validity, I have argued that no single theory can possess all desirable qualities of theoretical strength. But, when we move beyond the development of a single theory and instead focus on a series of related publications, the unique pressure felt by organizational researchers changes from a burden to an opportunity. Whereas the pursuit of a single theory optimized on all six dimensions is akin to the search for a mystical unicorn, it is within reach to develop a research program composed of papers that collectively account for all facets of strong theory.

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Appendix. Classification of 284 Commentaries on Strong Theory

Author(s)	Year	Journal	Importance	Interestingness	Actionability	Generality	Simplicity	Accuracy
Abrahamson and Eisenman	2001	<i>Human Relations</i>			X			
Acemoglu	2010	<i>J. Econom. Perspect.</i>	X			X		
Aguinis	1993	<i>J. Appl. Behav. Sci.</i>						X
Aguinis and Cronin	2022	<i>Organ. Psych. Rev.</i>	X			X	X	
Aguinis and Vandenberg	2014	<i>Annual Rev. Organ. Psych. Organ. Behav.</i>	X		X			
Aguinis et al.	2014	<i>Acad. Management Learn. Ed.</i>	X		X			
Ahuja	2022	<i>Organ. Sci.</i>	X					X
Aksom and Firsova	2021	<i>Philos. Management</i>				X		
Alvesson and Sandberg	2011	<i>Acad. Management Rev.</i>	X	X				
Alvesson and Sandberg	2013	<i>J. Management Stud.</i>		X				
Anderson	1999	<i>Organ. Sci.</i>						X
Antonakis	2017	<i>Leadership Quart.</i>	X				X	X
Arend	2021	<i>Acad. Management Rev.</i>	X					X
Argyris	1996	<i>Organ. Sci.</i>			X			
Ashford	2013	<i>Acad. Management Learn. Ed.</i>	X					
Ashforth	2021	Book					X	X
Astley and Zammuto	1992	<i>Organ. Sci.</i>			X			

Appendix. (Continued)

Author(s)	Year	Journal	Importance	Interestingness	Actionability	Generality	Simplicity	Accuracy
Baba	2016	<i>Canadian J. Admin. Sci.</i>		X				
Bacharach	1989	<i>Acad. Management Rev.</i>				X		X
Bamberger	2017	<i>Acad. Management J.</i>				X		X
Bamberger and Pratt	2010	<i>Acad. Management J.</i>		X		X		X
Barley	2006	<i>Acad. Management J.</i>		X				
Barley	2016	<i>Admin. Sci. Quart.</i>			X	X		X
Barney	2018	<i>Acad. Management Rev.</i>	X	X			X	
Bartunek et al.	2006	<i>Acad. Management J.</i>		X				
Beckman	2022	<i>Admin. Sci. Quart.</i>			X			X
Beer	2001	<i>Reflections</i>			X			
Beer	2020	<i>J. Appl. Behav. Sci.</i>			X			
Bem	1995	<i>Psych. Bull.</i>		X				X
Bentler and Mooijart	1989	<i>Psych. Bull.</i>						X
Bergh	2003	<i>Acad. Management J.</i>	X					
Bettis	1991	<i>Organ. Sci.</i>			X			
Bettis	2012	<i>Strategic Management J.</i>	X					X
Bettis et al.	2014	<i>Strategic Management J.</i>	X			X	X	
Bettis et al.	2016	<i>Strategic Management J.</i>				X		
Boudreau	2004	<i>Management Sci.</i>			X	X		
Bouvier	2022	<i>Book</i>				X		
Boyer	1985	<i>Internat. J. Institute Management Higher Ed.</i>	X		X	X		
Boyd et al.	2005	<i>Strategic Management J.</i>				X		X
Brief and Dukerich	1991	<i>Res. Organ. Behav.</i>			X			
Cachon	2009	<i>Management Sci.</i>		X	X	X		X
Campbell and Wilmot	2018	<i>Book</i>					X	X
Cannella and Paetzold	1994	<i>Acad. Management Rev.</i>	X					X
Cascio	2007	<i>Acad. Management J.</i>			X			
Cassimatis et al.	2008	<i>Cognitive Sci.</i>				X	X	
Caza et al.	2024	<i>Acad. Management Learn. Ed.</i>	X					
Chatman and Flynn	2005	<i>Organ. Sci.</i>						X
Cheng	1994	<i>Management Sci.</i>				X		
Cloutier and Langley	2020	<i>Organ. Theory</i>						X
Colquitt and George	2011	<i>Acad. Management J.</i>	X	X				
Cooter	1995	<i>J. Law Soc.</i>				X	X	
Connel and Nord	1996	<i>J. Appl. Behav. Sci.</i>					X	X
Corley and Gioia	2011	<i>Acad. Management Rev.</i>		X	X			
Cornellison and Höllerer	2019	<i>Organ. Theory</i>	X		X			
Cortina	2016	<i>J. Organ. Behav.</i>	X				X	X
Craighead et al.	2016	<i>Transportation J.</i>					X	X
Cronin et al.	2021	<i>Acad. Management Rev.</i>	X		X	X		
Cronin et al.	2022	<i>Acad. Management Rev.</i>				X		
Cronin and George	2022	<i>Organ. Res. Methods</i>				X		
Chen et al.	2023	<i>Entrepreneurship Theory Practice</i>			X			
Cronin et al.	2024	<i>Acad. Management Discoveries</i>				X		
Daft et al.	1987	<i>Acad. Management J.</i>	X					
Daft and Lewin	1993	<i>Organ. Sci.</i>						X
Daft and Lewin	2008	<i>Organ. Sci.</i>	X					
Daniels	1991	<i>J. Internat. Bus. Stud.</i>	X		X			
Das and Long	2010	<i>J. Management Issues</i>		X				

Appendix. (Continued)

Author(s)	Year	Journal	Importance	Interestingness	Actionability	Generality	Simplicity	Accuracy
David and Hatchuel	2007	Book			X	X		
Davis, M.	1971	<i>Philos. Soc. Sci.</i>		X				
Davis, G.	2010	<i>Organ. Res. Methods</i>				X		
Davis, G.	2015	<i>J. Management Stud.</i>	X			X		X
Davis and Marquis	2005	<i>Organ. Sci.</i>						X
Dehler	1998	<i>Management Learn.</i>			X			X
Delbridge and Fiss	2013	<i>Acad. Management Rev.</i>				X	X	
DeNisi	1994	<i>J. Managerial Issues</i>			X			
Denrell et al.	2015	<i>Organ. Sci.</i>				X		
DiMaggio	1995	<i>Admin. Sci. Quart.</i>		X		X	X	X
Donaldson	2022	<i>J. Econom. Perspect.</i>						X
Dorobantu et al.	2024	<i>Acad. Management J.</i>	X					
Dubois and Gadde	2014	<i>J. Bus. Res.</i>				X	X	X
Duton and Dukerich	2006	<i>Acad. Management J.</i>		X				
Easterby-Smith	1997	<i>Human Relations</i>						X
Eby	2021	<i>J. Appl. Psych.</i>			X			X
Edwards	2002	<i>Organ. Behav. Human Decision Processes</i>	X			X		
Edwards	2010	<i>Organ. Res. Methods</i>						X
Edwards and Berry	2010	<i>Organ. Res. Methods</i>						X
Eisenhardt	1989	<i>Acad. Management Rev.</i>				X		X
Elsbach et al.	1999	<i>Acad. Management Rev.</i>						X
Endersby	2009	<i>Science</i>				X		X
Ethiraj et al.	2018	<i>Strategic Management J.</i>	X					
Farjoun et al.	2015	<i>Organ. Sci.</i>			X		X	X
Feldman	2004	<i>J. Management</i>	X			X	X	X
Ferraro et al.	2009	<i>Organ. Sci.</i>			X			
Ferris et al.	2012	<i>Organ. Psych. Rev.</i>				X		
Fiske	1992	<i>Psych. Rev.</i>				X	X	
Fiss	2012	<i>Acad. Management J.</i>						X
Fox and Sitkin	2015	<i>Behav. Sci.</i>			X			
Francken et al.	2022	<i>Neuroscience Consciousness</i>				X		
Gavetti and Levinthal	2004	<i>Management Sci.</i>				X	X	
George	2016	<i>Acad. Management J.</i>	X		X			
George	2015	<i>Management Organ. Rev.</i>						X
George	2014	<i>Acad. Management J.</i>	X		X			X
George et al.	2016	<i>Acad. Management J.</i>	X		X			
Gergen	1973	<i>J. Personality Soc. Psych.</i>		X				X
Ghoshal	2005	<i>Acad. Management Learn. Ed.</i>	X			X		X
Gioia	2022	<i>Acad. Management Discoveries</i>			X			
Glick et al.	2007	<i>J. Organ. Behav.</i>	X			X		
Goldberg	1996	<i>J. Management Inquiry</i>	X					
Gopinath and Hoffman	1995	<i>J. Management Stud.</i>			X			
Graebner et al.	2023	<i>Strategic Management J.</i>			X			X
Grant and Pollock	2011	<i>Acad. Management J.</i>		X				
Gray and Wegner	2013	<i>Perspect. Psych. Sci.</i>		X				
Greenburg	2009	<i>Indust. Organ. Psych.</i>	X					
Greve	2020	<i>Admin. Sci. Quart.</i>	X			X		
Gruber	2023	<i>Acad. Management J.</i>	X		X			X
Gulati	2007	<i>Acad. Management J.</i>	X		X	X		
Hakel et al.	1982	Book			X			X
Hakem-Zadeh and Baba	2016	<i>Management Decision</i>			X			
Hambrick	2004	<i>Strategic Organ.</i>				X		
Handfield and Melnyk	1998	<i>J. Oper. Management</i>		X	X		X	X

Appendix. (Continued)

Author(s)	Year	Journal	Importance	Interestingness	Actionability	Generality	Simplicity	Accuracy
Harre	1990	<i>Ann. Theoret. Psych.</i>					X	
Hatchuel	2001	<i>J. Management Governance</i>				X	X	
Hatchuel	2005	<i>Eur. Management Rev.</i>			X			
Haveman et al.	2019	<i>Acad. Management Rev.</i>		X				
Heath and Sitkin	2001	<i>J. Organ. Behav.</i>						X
Heinze and Jappe	2020	<i>Quant. Sci. Stud.</i>					X	X
Hempel	1965	Book					X	
Hidi	1990	<i>Rev. Ed. Res.</i>		X				
Higgins	1992	<i>Personality Soc. Psych. Bull.</i>		X			X	
Hirsch and Levin	1999	<i>Organ. Sci.</i>				X	X	X
Hitt	1995	<i>J. Management Inquiry</i>			X			
Hollenbeck	2008	Book chapter	X			X		
Hollenbeck et al.	2004	<i>Human Resource Management</i>			X			
Hoover	2016	<i>J. Econom. Literature</i>				X		
Ilgen	2016	<i>Organ. Behav. Human Decision Processes</i>	X		X	X		
Jasso	2008	<i>Eur. Sociol. Rev.</i>				X	X	
Judge	2022	<i>Annual Rev. Organ. Psych. Organ. Behav.</i>			X			
Judge et al.	2012	<i>J. Organ. Behav.</i>				X		X
Kaidesoja	2019	<i>J. Theory Soc. Behav.</i>					X	X
Kehoe and Chari	2006	<i>J. Econom. Perspect.</i>			X			
Kessler	2017	<i>Group Organ. Management</i>				X		
Khan	2023	<i>Internat. J. Hospitality Management</i>						X
Khurana	2007	Book			X			X
Khurana and Spender	2012	<i>J. Management Stud.</i>			X			
Kilduff	2006	<i>Acad. Management Rev.</i>	X	X				
Kilduff and Keleman	2001	<i>British J. Management</i>	X					
Kilmann et al.	1983	Book			X			
Kim et al.	2018	<i>Acad. Management Rev.</i>				X	X	X
King and Lepak	2011	<i>Acad. Management Rev.</i>	X	X				
Kozlowski	2009	<i>J. Appl. Psych.</i>	X		X			X
Kozlowski et al.	2017	<i>J. Appl. Psych.</i>	X			X		
Lawler et al.	1985	Book	X		X			
Latham	2019	<i>Annual Rev. Organ. Psych. Organ. Behav.</i>			X			X
Lazarsfeld	1967	Book				X		
Leahey	2018	<i>Eur. Rev.</i>	X			X		
Lee	1982	<i>Bus. Horizons</i>			X			
Levine et al.	2023	<i>Organ. Sci.</i>						X
Lewin	1946	Book			X			
Lewis and Grimes	1999	<i>Acad. Management Rev.</i>				X		
Lilienfeld	2012	<i>Amer. Psychologist</i>		X				
Litchfield	1956	<i>Admin. Sci. Quart.</i>			X	X		
Loch	2024	<i>Management Sci.</i>			X			
Locke and Golden-Biddle	1997	<i>Acad. Management J.</i>	X			X		
Lockwood et al.	2019	<i>J. Management</i>			X			
Mahoney and McGahan	2007	<i>Strategic Organ.</i>			X			
Makadok et al.	2018	<i>Strategic Management J.</i>	X			X	X	X
Mannix	2003	<i>Acad. Management Rev.</i>						X
March	2013	<i>Management</i>			X			
Markides	2011	<i>J. Appl. Behav. Sci.</i>			X			
Mathieu	2016	<i>J. Organ. Behav.</i>	X		X			X
Mayer and Sparrowe	2013	<i>Acad. Management J.</i>	X			X		

Appendix. (Continued)

Author(s)	Year	Journal	Importance	Interestingness	Actionability	Generality	Simplicity	Accuracy
McGahan	2021	Book chapter	X		X	X		
McGuire	1997	<i>Annual Rev. Psych.</i>		X				
McKelvey	1999	<i>Emergence</i>				X		X
McMahan and McFarland	2021	<i>Amer. Sociol. Rev.</i>				X		
Merton et al.	1959	Book				X		X
Miles	1996	<i>J. Management Inquiry</i>			X			
Miner	1984	<i>Acad. Management Rev.</i>	X		X			
Mintzberg	2005	Book chapter		X			X	X
Mitchell and James	2001	<i>Acad. Management Rev.</i>						X
Mohr	1982	Book						X
Montgomery et al.	1989	<i>Strategic Management J.</i>	X					X
Morgeson et al.	2015	<i>Acad. Management Rev.</i>						X
Mortensen and Cialdini	2010	<i>Soc. Personality Psych. Compass</i>						X
Morrison	2010	<i>Acad. Management J.</i>	X					X
Mowday	1997	<i>Acad. Management Rev.</i>			X			
Mowday and Sutton	1993	<i>Annual Rev. Psych.</i>						X
Napier et al.	2009	<i>J. Management Inquiry</i>		X				
Niederman and March	2017	<i>Philos. Management</i>	X					X
Oxley et al.	2010	<i>Strategic Organ.</i>	X					X
Palmer	2006	<i>Admin. Sci. Quart.</i>			X	X		X
Parsons	1951	Book				X		
Patton and Appelbaum	2003	<i>Management Res. News</i>						X
Pentland	1999	<i>Acad. Management Rev.</i>				X	X	X
Perrow	1994	<i>Acad. Management Rev.</i>						X
Peterson	1998	<i>Organ. Sci.</i>						X
Pierce	2023	<i>Organ. Sci.</i>	X		X		X	X
Pfeffer	1993	<i>Acad. Management Rev.</i>				X		
Pfeffer	1995	<i>Organ. Sci.</i>				X		
Pfeffer	1997	Book			X	X		
Pfeffer and Fong	2005	<i>Organ. Sci.</i>				X	X	
Pfeffer and Sutton	1999	<i>Calif. Management Rev.</i>			X			
Pillutla and Thau	2013	<i>Organ. Psych. Rev.</i>	X					
Pinder and Moore	1980	Book					X	X
Piekkari et al.	2022	<i>Organ. Theory</i>						X
Ployhart and Bartunek	2019	<i>Acad. Management Rev.</i>			X			X
Podsakoff et al.	2018	<i>Acad. Management Learn. Ed.</i>	X					
Porter and Schneider	2014	<i>Annual Rev. Organ. Psych. Organ. Behav.</i>			X			X
Ragins	2012	<i>Acad. Management Rev.</i>					X	X
Reinecke et al.	2022	<i>Organ. Theory</i>	X		X			
Rhodes et al.	2004	<i>Health Ed. Res.</i>						X
Roberts	1978	Book			X	X		
Rousseau	2006	<i>Acad. Management Rev.</i>			X			
Rousseau	2020	<i>Acad. Management Learn. Ed.</i>			X			
Rousseau and Fried	2001	<i>J. Organ. Behav.</i>			X			X
Rudolph and Peluchette	1992	<i>J. Appl. Bus. Res.</i>			X			
Rynes	2005	<i>Acad. Management J.</i>	X					
Rynes	2007	<i>Acad. Management J.</i>	X	X				
Rynes and Bartunek	2015	Book		X				
Sackett	2021	<i>Annual Rev. Organ. Psych. Organ. Behav.</i>			X		X	

Appendix. (Continued)

Author(s)	Year	Journal	Importance	Interestingness	Actionability	Generality	Simplicity	Accuracy
Salas et al.	2017	<i>J. Appl. Psych.</i>	X			X		X
Santangelo and Verbeke	2022	<i>J. Internat. Bus. Stud.</i>				X		X
Schafer et al.	2016	<i>Organ. Res. Methods</i>					X	
Schwab et al.	2011	<i>Organ. Sci.</i>	X				X	
Schad et al.	2016	<i>Acad. Management Ann.</i>						X
Schilling and Green	2011	<i>Res. Policy</i>	X			X		
Seybolt	1996	<i>J. Management Inquiry</i>	X		X			
Shah and Corley	2006	<i>J. Management Stud.</i>				X	X	X
Shepherd and Gruber	2021	<i>Entrepreneurship Theory Practice</i>			X			
Shepherd and Suddaby	2017	<i>J. Management</i>	X	X				
Short	2009	<i>J. Management</i>		X				
Shugan	2003	<i>Marketing Sci.</i>		X				
Simchi-Levi	2023	<i>Management Sci.</i>			X		X	
Simon	1990	<i>Annual Rev. Psych.</i>				X		
Siponen et al.	2024	<i>Eur. J. Inform. Systems</i>	X					X
Smith	2003	<i>J. Acad. Marketing Sci.</i>		X				
Spencer	1873	<i>Stud. Soc.</i>				X		
Stamenkov	2022	<i>Quality Quantity</i>						X
Starbuck	2003	<i>J. Management Inquiry</i>			X			
Staw	1985	Book chapter	X	X				X
Staw	2016	<i>Annual Rev. Organ. Psych. Organ. Behav.</i>				X		X
Sterman and Wittenberg	1999	<i>Organ. Sci.</i>	X			X	X	
Strang and Siler	2017	<i>Soc. Stud. Sci.</i>					X	X
Suchan	1993	<i>J. Bus. Comm.</i>			X			
Suddaby	2010	<i>Acad. Management Rev.</i>					X	X
Swedberg	2014	Book						X
Swedberg	2016	<i>British J. Soc.</i>		X				
Swedberg	2017	<i>Annual Rev. Soc.</i>						X
Sweeney	1991	<i>Internat. J. Oper. Production Management</i>				X	X	
Thatcher and Fisher	2022	<i>Acad. Management Rev.</i>		X				
Thomas and Tymon	1982	<i>Acad. Management Rev.</i>			X			
Thompson	1956	<i>Admin. Sci. Quart.</i>			X	X		
Thompson	2017	Book			X	X		
Thorngate	1976	Book				X	X	X
Tihanyi	2020	<i>Acad. Management J.</i>	X					
Tobias	1994	<i>Rev. Ed. Res.</i>		X				
Tourish	2020	<i>Acad. Management Learn. Ed.</i>	X					
Tsang	2022	<i>J. Management Inquiry</i>	X					X
Tsang and Kwan	1999	<i>Acad. Management Rev.</i>				X		X
Tsui	2022	<i>Annual Rev. Organ. Psych. Organ. Behav.</i>			X			
Turner et al.	2017	<i>Organ. Res. Methods</i>				X	X	X
Uzzi et al.	2013	<i>Science</i>	X					
Van Aken	2005	<i>British J. Management</i>	X			X		
Van de Ven	1989	<i>Acad. Management Rev.</i>			X			
van Knippenberg	2011	<i>Organ. Psych. Rev.</i>			X	X		X
Van Krogh	2012	<i>Long Range Planning</i>						X
Van Maanen	1995	<i>Organ. Sci.</i>		X				X
Vermeulen	2005	<i>Acad. Management J.</i>			X			
Vermeulen	2007	<i>Acad. Management J.</i>			X			
Vidal	1995	<i>J. Contingencies Crisis Management</i>				X	X	X
von Nordenflycht	2023	<i>Organ. Sci.</i>				X	X	X

Appendix. (Continued)

Author(s)	Year	Journal	Importance	Interestingness	Actionability	Generality	Simplicity	Accuracy
Voss	2003	<i>J. Acad. Marketing Sci.</i>		X				
Wagner and Berger	1985	<i>Amer. J. Sociol.</i>				X	X	X
Weick	1974	<i>Behav. Sci.</i>					X	X
Weick	1996	<i>Admin. Sci. Quart.</i>				X		
Weick	1999	<i>Acad. Management Rev.</i>				X	X	X
Whetten	1989	<i>Acad. Management Rev.</i>				X	X	X
Whetten et al.	2009	<i>J. Management</i>				X		
Wickert et al.	2021	<i>J. Management Stud.</i>	X		X			
Wolfe	1992	<i>Soc. Res.</i>				X		X
Woodside	2010	<i>Indust. Marketing Management</i>	X			X		X
Zahra and Newey	2009	<i>J. Management Stud.</i>	X					
Zahra et al.	2024	<i>Strategic Entrepreneurship J.</i>	X	X				X
Zald	1993	<i>Organ. Sci.</i>				X		X
Zald	1996	<i>Admin. Sci. Quart.</i>				X		

Notes. The references are available at https://osf.io/8fyqp/?view_only=e93b177e6d2d4dc5bed22d325e98f2ec. An “X” denotes that a commentary prescribes a dimension. I consider the role of proscription (when scholars discourage a dimension) in the section on trade-offs. My objective was to gain a comprehensive and representative array of editorials, commentaries, and prescriptive articles about strong theory. The main text includes the definition that I used for strong theory as well as inclusion and exclusion criteria. Given that it is difficult to obtain an exhaustive set of relevant articles, I used the following search strategies in order to ensure that the list is representative. I searched for commentaries, editorials, and prescriptive articles from top 50 Thompson Reuters journals in management. These include the most recent commentaries written by editors of journals such as *Academy of Management Journal*, *Academy of Management Review*, *Administrative Science Quarterly*, *Journal of Applied Psychology*, *Management Science*, *Organization Science*, *Organizational Behavior and Human Decision Processes*, and *Strategic Management Journal*. I also searched for the most recent editorials and commentaries written by editors at theory-only outlets, including *Organization Theory* and *Organizational Psychology Review*. I also searched journals that include review articles (e.g., *Academy of Management Annals*) because these journals can feature editorials about how to build stronger theory. Additionally, I scrutinized citation networks, research databases, search engines, and doctoral syllabi to identify articles, books, and book chapters that are not in the top 50 journals but have influenced how organizational scholars are trained or socialized to think about theory. This led me to include sources that come from less widely read outlets but have shaped thought or are otherwise representative of key traditions. I also considered memoirs and retrospectives written by prominent scholars at the end of their careers because they often include advice on how to build theory. I performed multiple searches over several months. To ensure that the literature review is up to date, I restricted several searches only to articles published in the past two years.

Endnotes

¹ Sandberg and Alvesson (2021) note that there are five types of theory. All five involve specifying causal relationships (how certain concepts cause others), conceptual relationships (how different concepts are semantically linked), or temporal relationships (how an event unfolds over time). Most theories are *explaining* theories, which detail causal relationships. However, there are many influential *ordering* theories (especially typologies), which detail conceptual relationships (e.g., different types of organizations) as well as theories that combine explaining and ordering. There are fewer examples of the final three types: *comprehending* theories (which unpack related meanings of a concept), *enacting* theories (which unpack how phenomena unfold over time), and *provoking* theories (which challenge our understanding of phenomena). Given that my goal is to provide a representative account of views on theoretical strength, I am agnostic to which type or types of theory are relevant in a given commentary.

² This includes generating an initial theory, evaluating theory in the review process, revising theory based on reviewer feedback, understanding which published theories scholars choose to read, and understanding which theories mature into time-tested canon (often denoted by a formal term, such as *agency theory* or *social identity theory*, that pertains to a set of assumptions, cause-effect patterns, and boundary conditions among an established set of constructs).

³ The OSF link is https://osf.io/8fyqp/?view_only=e93b177e6d2d4dc5bed22d325e98f2ec.

⁴ I classified an article as prescribing a dimension if it explicitly called for researchers to prioritize it; later, I will speak to when scholars argue for less of a dimension (proscription).

⁵ Although scholars can increase a theory’s accuracy by revising it after subjecting it to rigorous testing, it is important that they heed the risks of overfitting and post hoc theorizing.

⁶ Although stand-alone theory is widespread, scholars vary in their approach to building it, ranging from formal models to verbal theory. One notable style of verbal theory eschews “boxes and arrows” models in favor of “meta-theories” that often feature metaphors (Cornelissen 2025). Recent examples are the view of King et al. (2010) of the organization as an agent and the conception of Gartenberg and Zenger (2023) of firms as subsocieties.

⁷ In this vein, actionable concepts can be rooted in general frameworks. For example, managerial search has been linked to learning, evolutionary theory, organizational design, and the attention-based view (Rivkin and Siggelkow 2003, Rosenkopf and Almeida 2003, Piezunka and Dahlander 2015, Raveendran et al. 2023).

⁸ Other examples are Albert Bandura, Carol Dweck, and Kurt Lewin for psychology; Alfonso Gambardella, Joshua Gans, and Scott Stern for economics; and Howard Aldrich, Rosabeth Moss Kanter, Charles Perrow, Phillip Selznick, and Max Weber for sociology.

⁹ Scholars value generality for different reasons in each discipline. Economists have converged on common paradigms, such as agency theory. Sociology and political science have been defined by weaker paradigms since the 1990s (Davis and Marquis 2005); however, because they focus on the interdependence between subsystems, they find little value in isolating any single feature at once. This increases the reach of their theories. Psychology, like sociology, also lacks a strong core paradigm (one exception to this is personality psychology embodied by micro scholars, such as Timothy Judge,

and macro scholars, such as Donald Hambrick). Most psychologists focus on concrete features of affect, cognition, and behavior rather than general theories. Yet, they build general theories for a different reason; they presume that their theories hold across contexts, including different countries. Another distinctive feature of psychology is how it employs moderators. Whereas other disciplines use moderators to identify boundary conditions and interventions, psychologists often use moderators to establish that a pattern does *not* vary (i.e., it is universal). Management departments hired disciplinary scholars to gain legitimacy after the Carnegie and Ford Foundations argued that the historical emphasis in business schools on vocational training prevented faculty from conducting rigorous research on universal patterns (Gordon and Howell 1959, Agger 2007, Hambrick 2007, Khurana 2010).

¹⁰ Scholars trained in the disciplines have increasingly favored disciplinary (rather than organizational) outlets (Staw 2016). In universities that have historically placed an especially strong emphasis on economics, sociology, psychology, and political science, the faculty members of management departments publish in disciplinary outlets, such as the *Quarterly Journal of Economics*, the *American Journal of Sociology*, and *Psychological Science*, 10–20 times as frequently as they publish in journals affiliated with the Institute for Operations Research and the Management Sciences (INFORMS) and the Academy of Management (AOM).

¹¹ As scholars build theories that are more parsimonious, they move through stages. First, rather than building a theory tailored to a specific industry or role, they build a theory that cuts across them, and rather than building a theory tailored to a specific time or location, they build a theory that generalizes across them. Second, they lump observable events into abstract concepts (e.g., lumping diversity in function, geographic location, business unit membership, and reporting channel into a single construct rather than studying them separately) (Cummings 2004).

¹² Other examples include theory on environments (Jurkovich 1974), purpose (Besharov and Mitzinneck 2023), alliance governance mechanisms (Keller et al. 2021), learning (Miller 1996), networks (Borgatti and Foster 2003), conflict (Bendersky and Hays 2012), psychological safety (Edmondson and Lei 2014), adaptation (Lau and Shaffer 2023), multiplex relationships (Ertug et al. 2023), dimensions of teams (Hollenbeck et al. 2012), legitimacy judgments (Tost 2011), diversity (van Knippenberg et al. 2004), and work orientation (Wrzesniewski et al. 1997).

¹³ The recent emphasis on accuracy and actionability may be because of improved technology for collecting and analyzing data. It is now easier to test theories that are closer to measures (thereby promoting accuracy) and to run intervention studies online (thereby promoting actionability). The increased focus on actionability and accuracy may also reflect the evolution of a maturing discipline. First-movers legitimize the field with parsimonious theories that map the field's boundaries and the most critical universal phenomena (an emphasis on generality, simplicity, and importance). Scholars then build provocative theories that cut against conventional wisdom (interestingness) (Staw 1995) and move beyond parsimonious theories to account for contingencies, exceptions, and granularity (accuracy) (Hirsch and Levin 1999). Finally, scholars focus on translating the science to steps that improve real-life circumstances (actionability). At this point, authors occasionally cycle back to parsimonious theories to provide updated high-level sensemaking. This solidifies an equilibrium such that all six dimensions are prioritized.

¹⁴ I computed the total number of dimensions prescribed over the 10 most recently published commentaries for all $284 - 10 + 1 = 275$ sequences of 10 papers. I correlated this value with the order of publication (most recent = 284).

¹⁵ If three reviewers each prioritize only one of the six dimensions, then there is likely to be at least one trade-off. However, more than

half of the editorials in the appendix prescribe more than one dimension. If that is representative of the proportion of reviewers who expect more than one dimension, then authors will almost certainly face at least one trade-off in every review process.

¹⁶ This is similar to negotiation constraint problems, in which a set of agents have incompatible terms and nonaligned means, leading to nontransitive group preferences or Condorcet cycles—compounded by the reality that the group's simultaneous judgment of a deliverable (in this case, a manuscript that reviewers are assessing at the same time) makes it impossible for authors to satisfy one reviewer at a time (Gehrlein 1983, Starmer 1999).

¹⁷ Some editors may wish to further ground the dimensions with consistent indicators. For instance, the six dimensions can be proxied with different features of nomological networks. To illustrate, perhaps network centrality proxies importance, distality (the number of intervening edges) proxies interestingness, and size proxies generality.

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