

## INTRODUCTION TO SPECIAL TOPIC FORUM

### THE EVOLVING SCIENCE OF ORGANIZATION: THEORY MATTERS

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This special topic forum (STF) was conceived because we were concerned that recent trends in empirical work had led some management scholars to devalue theory. Scholars in management, like those in many other social-science fields, have paid increasing attention to empirical methods: using advanced statistical techniques or harnessing laboratory, natural, and survey experiments to identify causality; emphasizing transparency and reproducibility in order to avoid questionable research practices such as p-hacking, HARKing (hypothesizing after the results are known); and focusing exclusively on statistical significance to the neglect of substantive importance. We are tapping into ever-bigger digital-world datasets and beginning to use computational statistics and machine-learning techniques to induce novel patterns in numerical, textual, audio, and image data, and to examine variation across observations, rather than testing central-tendency theories on numerical data. Given these trends, you might be tempted to conclude that data are everything and theory is nothing, or nearly so. But you would be wrong, as we explain below and as the articles in this STF demonstrate.

We agree with the declarations that “nothing is as practical as a good theory” (Lewin, 1945: 129), and “good theory is practical precisely because it advances knowledge in a scientific discipline, guides research toward crucial questions, and enlightens the profession of management” (Van de Ven, 1989: 486). We also acknowledge that all data are theory-laden (Hanson, 1958), and we recognize that theory can be built on practice because “nothing is [as] theoretical as a good practice” (Ployhart & Bartunek, 2019: 493). Therefore, improving our theory-building skills—whether our theory-building process involves inductive, deductive, or abductive approaches—merits time and attention. Below, we discuss critical

steps in theory building and explain how the articles selected for this STF relate to those steps.

#### FORMULATING THE PROBLEM

This is often the first—and arguably the most important—task in the theory-building process (Baer, Dirks, & Nickerson, 2013; Van de Ven, 2007). The first article in this STF, by Cronin, Stouten, and van Knippenberg (2021), identifies a *knowledge production problem* (Huff, 2000; Van de Ven & Johnson, 2006)—that is, a problem producing knowledge that is useful to managers and their workers. These authors maintain that we need *programmatic theory*, also known as normal science (Kuhn, 1970), which emerges from progressive research programs (Lakatos, 1970) and involves multiple theory components (“unit theories”), each focused on a specific aspect of a larger phenomenon. With programmatic theories, we can more clearly identify important managerial problems that can lead to useful and useable knowledge.

Programmatic theories provide frameworks—shared understandings of what matters and why—that help scholars make sense of what has been published or presented by members of their communities of scholarship and practice. The challenge resolved by programmatic theory is not that we produce too much theory (Hambrick, 2007; Pfeffer, 2014) but rather that we need to organize our knowledge via reconciling and integrating individual unit theories into a coherent architecture, while balancing the goals of completeness and parsimony (Whetten, 1989). Cronin and colleagues point to the research study by Crossan and Apaydin (2010) as an exemplar that systematically reviewed research on innovation and so revealed inconsistencies and gaps in our knowledge to be explored in future research.

## DEVELOPING NEW THEORY

With programmatic theory in place, the next article, by Saetre and Van de Ven (2021), focuses on a later step, generating new theory by abduction, meaning discovering anomalies that are not explained by existing theories, and developing plausible explanations for those anomalies (Hanson, 1958; Peirce, 1931–1935, as cited in Hartshorne & Weiss, 1935; Stanford Encyclopedia of Philosophy, 2021). Abductive reasoning can operate at the individual level (Mantere & Ketokivi, 2013; Van Maanen, Sørensen, & Mitchell, 2007) or the collective (group or organizational) level (Harvey & Kou, 2013; Rouse, 2020). At the individual level, abduction unfolds through an evolutionary sequence of variation, selection, and retention (Campbell, 1974; Van de Ven, 2007; Weick, 1989). In the variation stage, researchers observe the evidence and previous arguments, and generate multiple possible explanations. In the selection stage, they compare explanations and choose the one that most likely explains their observations. In the retention phase, they apply the explanations to subsequent empirical observations. At the collective level, abduction proceeds through a Hegelian dialectic, a collaborative (between-person or between-group) sequence of debate consisting of an initial proposition (the thesis), a negative reaction to it (the antithesis), and a reconciliation of thesis and antithesis (synthesis) that yields a novel and more robust proposition (Van de Ven & Poole, 1995; Vermeulen, 2005). As a collaborative process, collective-level abduction requires psychological safety in order to reach its potential (Edmondson, 1999; Nembhard & Edmondson, 2006).

*Observing* anomalies is easier when researchers triangulate across their individual experiences (Jick, 1979; Rerup, 2009) and give more attention to the full range of observations (i.e., do not downplay anomalies) (Simon, 1977, 1978). Observing anomalies is also easier when collective members have diverse experiences, have familiarity with diverse literatures, and interact with colleagues in diverse domains (Van de Ven, 2007; Weick, 1989). *Confirming* anomalies is easier when researchers consider evidence about the nature and context of the focal phenomenon from both up close and afar (Rousseau, 2006, 2020). *Generating* new explanations is more likely when researchers work with others, rather than alone, through extensive periods of brainstorming (Rietzchel, Nijstad, & Stroebe, 2007; Woodman, Sawyer, & Griffin, 1993). Finally, *evaluating* new explanations is more rigorous when researchers

use diverse selection criteria to converge on the most plausible explanation (Campbell, 1974; Weick, 1989).

Both generating theory by abduction, which involves individuals and small groups, and asking questions in programmatic normal-science theories, which occurs among larger collections of researchers, can help researchers avoid the Type III error of not formulating the right problem (Drnevich, Mahoney, & Schendel, 2020; Mitroff & Featheringham, 1974). As the mathematician, John Tukey (1962: 13–14) stated: “Far better an approximate answer to the right question ... than an exact answer to the wrong question.”

## DEVELOPING BETTER CONCEPTS

How well researchers succeed at developing programmatic theory and at explaining anomalies through abduction both depend on the quality of the concepts they develop and employ—that is, concept clarity, precision, and accuracy. However, concept quality also depends on the quality of existing theory and explanations of anomalies. As the pragmatist philosopher Abraham Kaplan (1964: 53) stated, “proper concepts are needed to formulate a good theory, but we need good theory to arrive at the proper concepts.”

Taking up the issue of how we develop better concepts, Makowski (2021) shows how conceptual (re-)engineering methods from philosophy (Blackburn, 1999; Cappelen, 2018) can help management researchers systematically create, edit, and update concepts. Conceptual engineering is used to create new concepts; reengineering to edit and update existing ones. Both processes require researchers to semantically adjust their theories—sometimes their entire paradigms. For example, over the past six decades, the concepts of organizational routines and capabilities (March & Simon, 1958; Nelson & Winter, 1982; Zollo & Winter, 2002) have been iteratively reengineered. For example, Felin and Foss (2009) maintained that in the literature on routines and capabilities the interpretation of the relevant patterns of behaviors evolved from (a) the individual to the collective, (b) the intentional to the unintentional, and (c) the observable to the unobservable. They suggested a reengineering of the constructs of organizational routines and capabilities to make them more explicit about micro foundations and origins.

In engineering new concepts, researchers often use metaphors and analogies from other social-science fields and beyond. However, these rhetorical devices need to be deliberate and justified. For example, on what grounds can organizational change be likened

to Darwinian rather than Lamarckian evolution (Hannan & Freeman, 1989; Nelson & Winter, 1982)—why can't new features adopted by organizations be "learned" and passed on to their descendants? Updating existing concepts proceeds through extension (attaching new meanings to concepts), intension (deepening the meanings attached to concepts by making them more abstract), or trimming (removing some existing meanings attached to concepts).

### COLLABORATING THROUGH OPEN SCIENCE

Mantere, Leone, and Faraj (2021) submit that theory development in management can benefit from following open-science practices, which have become common natural-science fields such as biology—that is, making available to other scholars data, working papers, reviews, and responses to reviews. Open-science practices facilitate expanding both conceptual vocabularies and ideas about relationships between concepts by expediting collaborations *within* and *between* research programs and topic domains (Loewenstein, Ocasio, & Jones, 2012; Ocasio, Loewenstein, & Nigam, 2015). Researchers who are loosely connected through open-science practices can share concepts, framings, explanatory theories, and empirical examples. Adopting these practices, however, entails normative shifts: management researchers will have to enact the epistemological principles of free criticism (Habermas, 1984; Popper, 1959) and diversity (Boje, Oswick, & Ford, 2004; Solomon, 2001).

There are several benefits of collective theorizing, enabled by open-science practices—however, each comes with a corresponding risk. *Theoretical deepening via coconstructing* can increase the precision of theoretical explanations, although it risks theoretical myopia. *Theoretical expansion via branching out* across existing lines of research and topic domains can make theory more generally applicable, but it risks theoretical dilution. *Theoretical rejuvenation via hybrid theorizing* between research programs and within topic domains can motivate new contributions, even though it hazards theoretical shallowness. *Theoretical generativity via cross-pollinating* between research programs and topic domains can generate useful knowledge to better satisfy societal needs, but it may lead to theoretical faddishness. Importantly, even when the tradeoffs favor open theorizing, there can still be collective-action problems (Gulati, 2007; Olson, 1965) that require institutional design changes to mitigate such problems (Ostrom, 1990; Williamson, 1996).

### THEORY IN THE AGE OF MACHINE LEARNING

Leavitt, Schabram, Hariharan, and Barnes (2021) maintain that Machine Learning (ML)—a subset of artificial intelligence in which computer algorithms build probabilistic models to classify, cluster, or predict—can complement traditional theory-building. ML algorithms differ in fundamental ways from standard, human-driven algorithms based on "if, then" logic. ML algorithms "learn" from existing patterns in data and derive predictions about patterns in other data. They can construct complex patterns that would be missed by researchers relying on theoretically informed hypotheses or traditional human-driven algorithms (i.e., traditional computer programs) (Bishop, 2006; Mullainathan & Spiess, 2017). ML algorithms predict both variation and central tendencies, and so offer more nuanced results compared to most existing theories and human-driven algorithms. However, prediction, even of variation across observations, is not enough: we also need to explain—to offer causal arguments about relationships between concepts (Bacharach, 1989; Cornelissen & Durand, 2012). That is the essence of theory. And although ML algorithms are often considered to be *inductive* tools (e.g., Antons, Joshi, & Salge, 2019), they can contribute to explanation via *abductive* reasoning (e.g., Leung & Koppman, 2018), similar to the way qualitative researchers assess extreme cases (e.g., Eisenhardt, 1989). While ML algorithms often outperform humans because these algorithms can take into consideration all bivariate and higher-level interactions among variables, we need theory to determine what variables might be missing from ML algorithms, and what variables are irrelevant, even if they are highly correlated with the outcomes under study. In sum, ML algorithms and theories are complements, not competitors. A final point is that adopting ML tools and techniques can encourage multidisciplinary research, bringing management researchers together with computer scientists and statisticians, which can encourage new lines of thinking.

### USING HEURISTICS

Complementary to ML techniques, configurational theorizing can enable "capturing the whole"—that is, describing the logic underpinning classification schemes. Furnari, Crilly, Misangyi, Greckhamer, Fiss, and Aguilera (2021) propose that contemporary management challenges are complex—they are often characterized by interdependencies among multiple explanatory variables. Complexity makes resolving management problems and forecasting the

consequences of future actions difficult (Doty & Glick, 1994; Doty, Glick, & Huber, 1993). Further, when researchers seek a close fit between theory and method (Edmondson & McManus, 2007; Van Maanen, Sørensen, & Mitchell, 2007), methodological limitations and assumptions can imprint on theories—for example, linearity assumptions (Abbott, 1988; Delbridge & Fiss, 2013) and thinking about net effects (Fiss, 2007; Ragin, 2009). Imprinting methodological limitations and assumptions on theories often pushes researchers to isolate the estimate of an explanatory variable's effect.

Furnari et al. (2021) argue that configurational theories, such as Burns and Stalker's (1961) thinking on organic and mechanistic organizations, and Miles and Snow's (1978) ideas about prospector, analyzer, and defender strategies, are better-matched for complex phenomena. Furnari and his colleagues use these classic works to illustrate three iterative stages: *scoping*, to identify relevant attributes that might plausibly form configurations; *linking*, to provide analytical precision concerning how attributes connect to one another; and *naming*, to label configurations that evoke coherent orchestrating themes. Configuration theorizing, which can also be found in more recent set-theoretic approaches (Fiss, 2011; Ragin, 2000), emphasizes two characteristics of causal complexity. First, thinking about *conjunction* brings to the foreground the fact that explanatory factors jointly produce an outcome (Boisot & McKelvey, 2010; Misangyi, et al., 2017). Second, thinking about *equifinality* makes it clear that a complex system can reach the same equilibrium from different initial conditions and following different paths (Gresov & Drazin, 1997; Katz & Kahn, 1978). Thus, Furnari et al. conclude that developing better heuristics for configurational theorizing is foundational for improving theory-building skills.

### APPLYING DESIGN SCIENCE

Finally, Rindova and Martins (2021) propose insights about how we can move from theory to practice via *design science*, which offers an approach to knowledge generation that differs fundamentally from that of explanatory science. While explanatory science involves analyzing data to figure out how things come to be, or how they operate, design science probes “how things ought to be” (Romme, 2003; Van Aken, 2005). This involves a shift in temporal focus from the past and present to the uncertain future. In turn, this temporal shift requires researchers to imagine possibilities and generate options, rather than making definitive choices among existing

alternatives. Drawing from an evolutionary epistemology, design-science activities make it possible to increase novelty, variation, and option generation in both the problems uncovered and the solutions explored. Novelty and variation become valued goals in their own right, which is especially valuable when confronting ill-structured problems (Simon, 1996).

Rindova and Martins (2021) identify three components of the design-science approach: The first involves articulating and shaping intentions to change an existing situation into a preferred one (Rindova & Courtney, 2020; Shackle, 1973). The second involves designing without *final* goals, which emphasizes variation. The third involves validating and creating provisional solutions and novel strategies through stakeholder dialogue. These components extend the micro foundations of managerial actions with respect to the generation of strategic foresight and shaping intentions, as well as works that join stakeholder management and complex societal problems.

### DISCUSSION

The seven articles in this STF take us on a journey toward a deeper understanding of the importance of theory in management research. Given the number of submissions and their quality, we feel it is fair to conclude that management scholars still care a great deal about theory-building. The diversity and breadth of papers, and their willingness to tackle difficult, sometimes seemingly intractable problems, also gives us a sense of hope for the future. It is clear, as we look across the intellectual contribution of these articles, that there is much to inspire future research. We also see patterns *across* the papers. We can easily imagine bringing together several pairings (or even larger groupings) of these scholars to take management research to the next level. For example, if we pair the disciplined imagination of systematic abduction with the perspective on designing for the future, we might be able to create not only what might be but also what *should* be. Or, if we apply the tools of ML, with their ability to detect complex and often obscure patterns, to help management scholars shift their focus to the right problems, we might be able to build more programmatic theories. In addition, if we place more emphasis on open science and bind it to the rigor of conceptual competency and the heuristics of configurational theorizing, we might be able to more rapidly advance theory by opening up the “black box” of scientific discovery.



Given that the mission of AMR is “to publish theoretical insights that advance our understanding of management and organizations” (AMR, 2021) it seems only proper that we should frequently revisit the relevance and role of theory to our field. As is true of most self-reflective endeavors, we have made some progress, but we also have not dealt yet with some critical issues. For example, although we again seek to pose new questions and undertake new ways of answering them, it is not clear how we can overcome the lure of investigating the old and familiar. We have started, in some instances, to look for inspiration outside of our immediate fields, but the difficulties of truly moving well beyond our own expertise for inspiration and insight are daunting, and examples are few. Finally, how do we make some of these important changes without focusing on our model of PhD education, tenure, and career progression, which has not changed much in decades?

As management scholars, we can and should be dealing with big, challenging, “wicked” problems. Gender and ethno-racial inequality, climate change, and poverty—these complex problems require multidisciplinary approaches. Since organizations—business firms, nonprofits, government agencies, civil-society groups—are the most powerful actors in the 21st century, scholars of management and organization should dive into these substantively important questions and add our knowledge and our voices to the multidisciplinary conversation. If we want to continue to be relevant as scholars, we may have to do more to move on to the “next stage” of our scientific revolution.

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