© *Academy of Management Review*
2020, Vol. 46, No. 4, 660–666.
https://doi.org/10.5465/amr.2021.0315

INTRODUCTION TO SPECIAL TOPIC FORUM

THE EVOLVING SCIENCE OF ORGANIZATION: THEORY MATTERS

HEATHER A. HAVEMAN
University of California at Berkeley

JOSEPH T. MAHONEY
University of Illinois at Urbana-Champaign

ELIZABETH MANNIX
Cornell University

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.

REFERENCES

Abbott, A. D. 1988. Transcending general linear reality. ***Sociological Theory***, 6: 169–186.

AMR. 2021. Academy of Management Review website. Retrieved from https://aom.org/research/journals/review

Antons, D., Joshi, A.M., & Salge, T.O. 2019. Content, contribution, and knowledge consumption: Uncovering hidden topic structure and rhetorical signals in scientific texts. ***Journal of Management***, 45: 3035–3076.

Bacharach, S. B. 1989. Organization theories: Some criteria for evaluation. ***Academy of Management Review***, 14: 496–515.

Baer, M., Dirks, K. T., & Nickerson, J. A. 2013. Microfoundations of strategic problem formulation. ***Strategic Management Journal***, 34: 197–214.

Bishop, C. M. 2006. ***Pattern recognition and machine learning***. New York, NY: Springer.

Blackburn, S. 1999. ***Think: A compelling introduction to philosophy***. Oxford, U.K.: Oxford University Press.

Boje, D. M., Oswick, C., & Ford, J. D. 2004. Language and organization: The doing of discourse. ***Academy of Management Review***, 29: 571–577.

Boisot, M., & McKelvey, B. 2010. Integrating modernist and postmodernist perspectives: A complexity science bridge. ***Academy of Management Review***, 35: 415–433.

Burns, T., & Stalker, G. M. 1961. ***The management of innovation***. London, U.K.: Tavistok Publishing.

Campbell, D. T. 1974. Evolutionary epistemology. In P. A. Schlipp (Ed.), ***The philosophy of Karl Popper:*** 413–463. LaSalle, IL: The Library of Living Philosophers.

Cappelen, H. 2018. ***Fixing language: An essay on conceptual engineering***. Oxford, U.K.: Oxford University Press.

Cornelissen, J., & Durand, R. 2012. More than just novelty: Conceptual blending and causality. ***Academy of Management Review***, 37: 152–154.

Cronin, M., Stouten, J., & van Knippenberg, D. 2021. The theory crisis in management research: Solving the right problem. ***Academy of Management Review***, 46.

Crossan, M. M., & Apaydin, M. 2010. A multi-dimensional framework of organizational innovation: A systematic review of the literature. ***Journal of Management Studies***, 47: 1154–1191.

Delbridge, R., & Fiss, P. C. 2013. Styles of theorizing and the social organization of knowledge. ***Academy of Management Review***, 38: 325–331.

Doty, D. H., & Glick, W. H. 1994. Typologies as a unique form of theory building: Toward improved understanding and modeling. ***Academy of Management Review***, 19: 230–251.

Doty, D. H., Glick, W. H., & Huber, G. P. 1993. Fit, equifinality, and organizational effectiveness: A test of two configurational theories. ***Academy of Management Journal***, 36: 1196–1250.

Drnevich, P. L., Mahoney, J. T., & Schendel, D. 2020. Has strategic management lost its way? ***Strategic Management Review***, 1: 35–73.

Edmondson, A. 1999. Psychological safety and learning behavior in work teams. ***Administrative Science Quarterly***, 44: 350–383.

Edmondson, A. C., & McManus, S. E. 2007. Methodological fit in management field research. ***Academy of Management Review***, 32: 1246–1264.

Eisenhardt, K. M. 1989. Building theories from case study research. ***Academy of Management Review***, 14: 532–550.

Felin, T., & Foss, N. J. 2009. Organizational routines and capabilities: Historical drift and a course-correction toward microfoundations. ***Scandinavian Journal of Management***, 25: 157–167.

Fiss, P. C. 2007. A set-theoretic approach to organizational configurations. ***Academy of Management Review***, 32: 1180–1198.

Fiss, P. C. 2011. Building better causal theories: A fuzzy set approach to typologies in organization research. ***Academy of Management Journal***, 54: 393–420.

Furnari, S., Crilly, D., Misangyi, V. F., Greckhamer, T., Fiss, P. C., & Aguilera, R. V. 2021. Capturing causal complexity: Heuristics for configurational theorizing. ***Academy of Management Review***, 46.

Gresov, C., & Drazin, R. 1997. Equifinality: Functional equivalence in organization design. ***Academy of Management Review***, 22: 403–428.

Gulati, R. 2007. Tent poles, tribalism, and boundary spanning: The rigor-relevance debate in management research. ***Academy of Management Journal***, 50: 775–782.

Habermas, J. 1984. ***The theory of communicative action: Reason and the rationalization of society***. Boston, MA: Beacon Press.

Hambrick, D. C. 2007. The field of management's devotion to theory: Too much of a good thing? ***Academy of Management Journal***, 50: 1346–1352.

Hannan, M., & Freeman, J. 1989. ***Organizational ecology***. Cambridge, MA: Harvard University Press.

Hanson, N. R. 1958. ***Patterns of discovery: An inquiry into the conceptual foundation of science***. Cambridge, U.K.: Cambridge University Press.

Hartshorne, C. & Weiss, P. (Eds.). 1935. **The *collected works* of Charles Sanders Peirce, *1931–1935***, vol. 1–6. Cambridge, MA: Harvard University Press.

Harvey, S., & Kou, C.-Y. 2013. Collective engagement in creative tasks: The role of evaluation in the creative process in groups. ***Administrative Science Quarterly***, 58: 346–386.

Huff, A. S. 2000. Changes in organizational knowledge production. ***Academy of Management Review***, 25: 288–293.

Jick, T. D. 1979. Mixing qualitative and quantitative methods: Triangulation in action. ***Administrative Science Quarterly***, 24: 602–611.

Kaplan, A. 1964. ***The conduct of inquiry: Methodology for behavior science***. San Francisco, CA: Chandler.

Katz, D., & Kahn, R. L. 1978. ***The social psychology of organizations***. New York, NY: Wiley.

Kuhn, T. S. 1970. ***The structure of scientific revolutions*** (2nd ed.). Chicago, IL: University of Chicago Press.

Lakatos, I. 1970. Falsification and the methodology of scientific research programs. In I. Lakatos & A. Musgrave (Eds.), ***Criticism and the growth of knowledge***: 91–196. Cambridge, U.K.: Cambridge University Press.

Leavitt, K., Schabram, K., Hariharan, P., & Barnes, C. 2021. Ghost in the machine: On organizational theory in the age of machine learning. ***Academy of Management Review***, 46.

Leung, M. D., & Koppman, S. 2018. Taking a pass: How proportional prejudice decisions not to hire reproduce gender segregation. ***American Journal of Sociology***, 124: 762–813.

Lewin, K. 1945. The research center for group dynamics at Massachusetts Institute of Technology. ***Sociometry***, 8: 126–135.

Loewenstein, J., Ocasio, W., & Jones, C. 2012. Vocabularies and vocabulary structure: A new approach linking categories, practices, and institutions. ***Academy of Management Annals***, 6: 41–86.

Makowski, R. 2021. Optimizing concepts: Conceptual engineering in the field of management. The case of routines research. ***Academy of Management Review***, 46.

Mantere, S., & Ketokivi, M. 2013. Reasoning in organization science. ***Academy of Management Review***, 38: 70–89.

Mantere, S., Leone, P. V., & Faraj, S. 2021. Open theorizing in management and organization studies. ***Academy of Management Review***, 46.

March, J. G., & Simon, H. A. 1958. ***Organizations***. New York, NY: John Wiley & Sons.

Miles, R. E., & Snow, C. C. 1978. ***Organization strategy, structure, and process***. New York, NY: McGraw-Hill.

Misangyi, V. F., Greckhamer, T., Furnari, S., Fiss, P. C., Crilly, D., & Aguilera, R. 2017. Embracing causal complexity: The emergence of a neo-configurational perspective. ***Journal of Management***, 43: 255–282.

Mitroff, I. I., & Featheringham, T. R. 1974. On systemic problem solving and the error of the third kind. ***Behavioral Science***, 19: 383–393.

Mullainathan, S., & Spiess, J. 2017. Machine learning: An applied econometric approach. ***Journal of Economic Perspectives***, 31: 87–106.

Nelson, R. R., & Winter, S. G. 1982. ***An evolutionary theory of economic change***. Boston, MA: Belknap Press of Harvard University Press.

Nembhard, I. M., & Edmondson, A. C. 2006. Making it safe: The effects of leader inclusiveness and professional status on psychological safety and improvement efforts in health care teams. ***Journal of Organizational Behavior***, 27: 941–966.

Ocasio, W., Loewenstein, J., & Nigam, A. 2015. How streams of communication reproduce and change institutional logics: The role of categories. ***Academy of Management Review***, 40: 8–48.

Olson, M. 1965. ***The logic of collective action: Public goods and the theory of groups***. Cambridge, MA: Harvard University Press.

Ostrom, E. 1990. ***Governing the commons: The evolution of institutions for collective action***. Cambridge, U.K.: Cambridge University Press.

Pfeffer, J. 2014. The management theory morass: Some modest proposals. In J. Miles (Ed.), ***New directions in management and organization theory***: 447–456. Newcastle, U.K.: Cambridge Scholars.

Ployhart, R. E., & Bartunek, J. M. 2019. There is nothing so theoretical as good practice—a call for phenomenal theory. ***Academy of Management Review***, 44: 493–497.

Popper, K. 1959.***The logic of scientific discovery***. London, U.K.: Hutchinson.

Ragin, C. C. 2000. ***Fuzzy-set social science***. Chicago, IL: University of Chicago Press.

Ragin, C. C. 2009. ***Redesigning social inquiry: Fuzzy sets and beyond***. Chicago, IL: University of Chicago Press.

Rietzchel, E. F., Nijstad, B. A., & Stroebe, W. 2007. Relative accessibility of domain knowledge and creativity: The effects of knowledge activation on the quantity and originality of generated ideas. ***Journal of Experimental Social Psychology***, 43: 933–946.

Rerup, C. 2009. Attentional triangulation: Learning from unexpected rare crises. ***Organization Science***, 20: 876–893.

Rindova, V., & Courtney, H. 2020. To shape or adapt: Knowledge problems, epistemologies, and strategic postures under Knightian Uncertainty. ***Academy of Management Review***, 45: 787–807.

Rindova, V., & Martins, L. 2021. Giving shape to possibilities: A design science approach to developing novel strategies. ***Academy of Management Review***, 46.

Romme, A. G. L. 2003. Making a difference: Organization as design. ***Organization Science***, 14: 558–573.

Rouse, E. D. 2020. Where you end and I begin: Understanding intimate co-creation. ***Academy of Management Review***, 45: 181–204.

Rousseau, D. M. 2006. Is there such a thing as "evidence-based management?" ***Academy of Management Review***, 31: 256–269.

Rousseau, D. M. 2020. The realist rationality of evidence-based management. ***Academy of Management Learning & Education***, 19: 415–424.

Saetre, A., & Van de Ven, A. H. 2021. Generating theory by abduction. ***Academy of Management Review***, 46.

Shackle, G. L. S. 1973. ***Epistemics and economics: A critique of economic doctrines***. Cambridge, U.K.: Cambridge University Press.

Simon, H. A. 1977. ***Models of discovery***. Dordrecht, Netherlands: D. Reidel.

Simon, H. A. 1978. Rationality as process and as product of thought. ***American Economic Review***, 68: 1–16.

Simon, H. A. 1996. ***Science of the artificial*** (2nd ed.). Cambridge, MA: MIT Press.

Solomon, M. 2001. ***Social empiricism***. Cambridge, MA: MIT Press.

Stanford Encyclopedia of Philosophy. 2021. Abduction. Retrieved from https://plato.stanford.edu/entries/abduction/

Tukey, J. W. 1962. The future of data analysis. ***Annals of Mathematical Statistics***, 33: 1–67.

Van Aken, J. E. 2005. Management research as a design science: Articulating the research products of mode 2 knowledge production in management. ***British Journal of Management***, 16: 19–36.

Van de Ven, A. H. 1989. Nothing is quite so practical as a good theory. ***Academy of Management Review***, 14: 486–489.

Van de Ven, A. H. 2007. ***Engaged scholarship: A guide for organizational and social research***. Oxford, U.K.: Oxford University Press.

Van de Ven, A. H., & Johnson, P. E. 2006. Knowledge for theory and practice. ***Academy of Management Review***, 31: 802–821.

Van de Ven, A. H., & Poole, M. S. 1995. Explaining development and change in organizations. ***Academy of Management Review***, 20: 510–540.

Van Maanen, J., Sørensen, J. B., & Mitchell, T. R. 2007. The interplay between theory and method. ***Academy of Management Review***, 32: 1145–1154.

Vermeulen, F. 2005. On rigor and relevance: Fostering dialectic progress in management research. ***Academy of Management Journal***, 48: 978–982.

Weick, K. 1989. Theory construction as disciplined imagination. ***Academy of Management Review***, 14: 516–531.

Whetten, D. A. 1989. What constitutes a theoretical contribution? ***Academy of Management Review***, 14: 490–495.

Williamson, O. E. 1996. ***The mechanisms of governance***. Oxford, U.K.: Oxford University Press.

Woodman, R. W., Sawyer, J. E., & Griffin, R. W. 1993. Toward a theory of organizational creativity. ***Academy of Management Review***, 18: 293–321.

Zollo, M., & Winter, S. G. 2002. Deliberate learning and the evolution of dynamic capabilities. ***Organization Science***, 13: 339–351.