© *Academy of Management Journal*
2018, Vol. 61, No. 2, 371–377.
https://doi.org/10.5465/amj.2018.4002

FROM THE EDITORS

NEW WAYS OF SEEING: RADICAL THEORIZING

At *Academy of Management Journal*, we encourage authors to produce novel, interesting, and theoretically bold work, and recommend that they ask themselves how their manuscript challenges, changes, or advances what we know at a theoretical level before submitting their paper for review. The strength of a manuscript's theoretical contribution is a key element that is considered by reviewers and associate editors alike when evaluating the merits of a study.

In our endeavor to stimulate "New Ways of Seeing" in management research, this editorial focuses on "radical theorizing," which is the generation of completely new theoretical insights that may lead to a substantial departure from existing paradigms. Theorization, in general, can be viewed as disciplined imagination (Mills, 1959; Weick, 1989), and it allows scholars to systematically explain and predict outcomes of interest (Cook & Campbell, 1979). Radical theorizing, though, emphasizes the achievement of significant creative leaps, as evidenced, for instance, by drawing on pertinent distal theories and formulating novel relationships (see Colquitt & Zapata-Phelan, 2007). It can be viewed as the process by which we accomplish theoretical prescience (Corley & Gioia, 2011; Kuhn, 1962)—creating understandings that resolve existing ambiguities over future states. Interestingness is a necessary but not sufficient condition for the outcome of this process (an idea that is original and has both scientific and practical utility); it is not a process with which to derive ideas that are merely counterintuitive or painfully obvious. Through radical theorizing, scholars introduce new research directions that may fundamentally shape future discourse on a topic.

This editorial offers ideas on how scholars might want to think about radical theorizing. We borrow from research on innovation and organizational learning as well as the philosophy of science to discuss the important role of radical theorizing in achieving scientific progress, different approaches to radical theorizing, and insights one may obtain from observing the work of radical theorizers of the past. We conclude by offering some practical tips on how researchers can get started with radical theorizing in their own work.

RADICAL THEORIZING AND SCIENTIFIC PROGRESS

Radical theorizing is a fundamental driver of scientific progress. Kuhn (1962) argued that scientific progress is not accomplished by a linear accumulation of knowledge (which he refers to as "puzzle-solving" efforts in normal science); rather, it is achieved through periodic revolutions that abruptly transform the nature of scientific inquiry in a domain. At the outset of these scientific revolutions, scientists uncover an increasing number of anomalous results, which lead the field into a crisis and call for new paradigms. Before a new paradigm can emerge, however, scientists have to come up with new theories that subsume the existing results together with the anomalous results into a novel framework. As Kuhn (1962: 53) explained:

> Assimilating a new sort of fact demands a more than additive adjustment of theory, and until that adjustment is completed—until the scientist has learned to see nature in a different way—the new fact is not quite a scientific fact at all.

Different prescientific discoveries, which arise through scattered individual efforts, will then compete for status and may enter a paradigmatic stage when they are more successful in solving the problems that had been deemed as acute. In the new paradigm, the anomalous observation will have become the expected outcome.

As research on innovation and organizational learning reminds us, the exploration of a radically new scientific solution is not a risk-free undertaking, given that such endeavors face a priori uncertainty and a posteriori variance in acceptance and performance (March, 1991; Taylor & Greve, 2006). The distinction between exploration of novel, potentially disruptive solutions ("distant search") and exploitation of existing solutions via incremental improvements ("local search") is important, because the practices and approaches that lead to the former outcomes differ substantially from those that lead to the latter outcomes.

SOME APPROACHES TO RADICAL THEORIZING

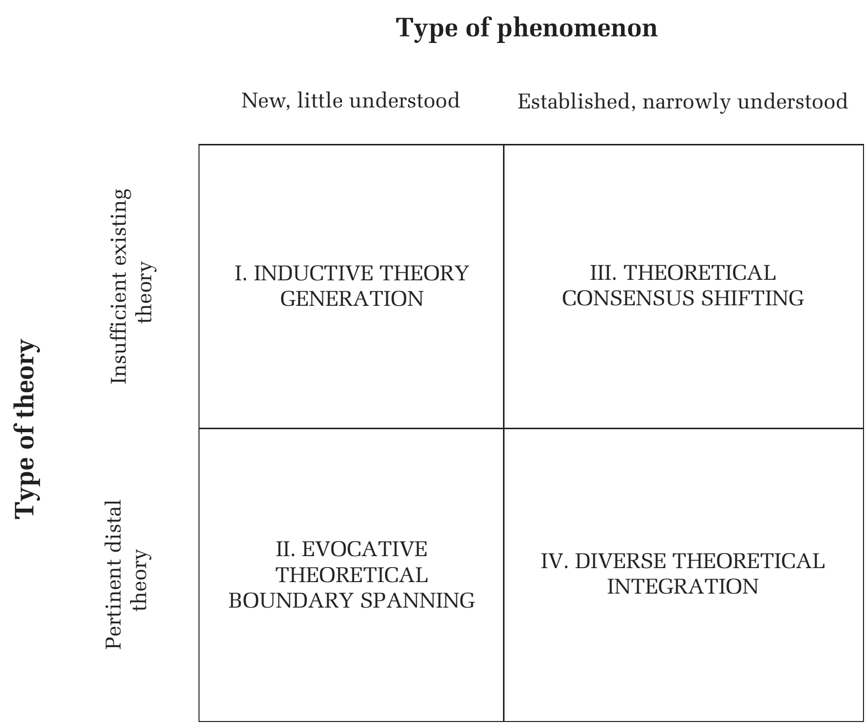
Seminal works that engender radical theorizing serve two important purposes: they (1) alert observers to the existence of a crisis and insufficiency of existing theories and (2) provide an initial set of evidence, albeit tentative, that could spark a new direction of thinking. Radical theorizing achieves these two purposes in different ways. In Figure 1, we illustrate four broad approaches to radical theorizing. Specifically, we develop a typology based on the interplay of the *type of phenomenon* being investigated (new and little understood vs. established but narrowly understood) and *state of current theory* in explaining the phenomenon (insufficient existing theory vs. pertinent distal theory). This typology is not meant to be exclusive, but, rather, serves as an illustration of how some authors have approached radical theorizing.

Inductive Theory Generation

Organizations and environments are dynamic and continuously evolving, causing new phenomena to emerge and existing phenomena to change. The gap between new, little understood phenomena and insufficient existing theory requires generation of new theory, often using an inductive approach. When there is little available theory to explain a phenomenon, research on the subject focuses on analyzing “the actual production of meanings and concepts used by social actors in real settings” (Gephart, 2004: 457). One approach that is employed widely in inductive new theory generation is the grounded theory approach developed by Glaser and Strauss (1967), who contended that new theory could be developed by paying careful attention to the contrast between “the daily realities (what is actually going on) of substantive areas” (Glaser & Strauss, 1967: 239) and the interpretations of those daily realities made by those who participate in them. New theory is generated through a bottom-up iterative approach that entails constant comparison of insights from analyses and collection of new data based on the theory being constructed.

Despite its suitability in understanding new phenomena, inductive theory generation poses major challenges to researchers. One common issue is the “failure to ‘lift’ data to a conceptual level” (Suddaby, 2006: 636). As Charmaz (2005: 511) stressed, “Without theoretical scrutiny, direction, and development,” data generated by inductive investigations “can culminate in mundane descriptions.” Thus, movement from relatively narrow or phenomenon-driven observations to more abstract theoretical categories and definable conceptual structures that depict causal relationships and processes inherent in the phenomenon being studied is central to new theory generation through this approach. An example of

FIGURE 1
A Taxonomy of Approaches to Radical Theorizing



this type of theorizing is the recent work by Petriglieri, Ashford, and Wrzesniewski (2018) on a new and ill-understood phenomenon of independent workers—people not affiliated with an organization or established profession ("gig workers"). Such independent work poses different challenges from those presented by the office building and factory floor workplace contexts that have typically been researched. Using an inductive qualitative empirical approach, Petriglieri et al.'s (2018) paper advances new theory about the management of precarious and personalized work identities; in doing so, it moves beyond description of a new phenomenon to generate novel theory for identity scholars as a result of examining this new phenomenon. Specifically, their study shows how existing theories on work identities fail to explain the unique ways in which independent workers manage emotions associated with precarious and personalized work identities, thereby rendering their work identities viable and their selves vital.

Evocative Theoretical Boundary Spanning

Developing radical theory requires acknowledgment of "the important dialectic between theory and the phenomena or practices to which it relates" (Gulati, 2007: 780). Emergence of new phenomena that are not substantially or entirely explained by established theories lend opportunities to stretch the theoretical boundaries and look for explanations beyond our own discipline—"observable behavior or outcomes allow one to define, refine, or discard theory" (Gulati, 2007: 780). Such a boundary-spanning approach allows researchers to overcome the gap between a phenomenon or novel observations and existing theoretical explanations within a discipline by borrowing established theories from related or distal disciplines. Contrary to the inductive approach that focuses on actual observations as a basis for theory construction, the boundary-spanning approach attempts to understand phenomena through both theory building and testing the application of theories from other disciplines. In doing so, "they expand a given literature by taking it in a new and different direction" (Colquitt & Zapata-Phelan, 2007: 1286).

The challenge in the boundary-spanning approach is identifying an appropriate theory that can explain an ill-understood phenomenon as well as being cognizant of the nuances in applying a distal theory to a new context so as to avoid issues of overgeneralization and force-fitting. Researchers can also face challenges in overcoming established disciplinary thinking in the domain of the phenomenon as well as the domain of the distal theory that can create barriers to such cross-fertilization across disciplines. However, with increased emphasis on pluralistic theorization and the "big tent" view, our domain is opening up to boundary-spanning research.

For instance, the microfoundations research in upper echelons has forged boundary-spanning research to open the ill-understood "black box" and stretch the boundaries of executive characteristics by borrowing from diverse disciplines such as psychology and political science. Chatterjee and Hambrick (2007) built from research in psychology to introduce the concept of "narcissism" (inflated self-views and preoccupation with having those self-views continuously reinforced) from clinical psychology to the realm of strategy and demonstrated the importance of hubris as a key mechanism in driving executive influence on firm strategies.

Theoretical Consensus Shifting

Radical theorizing does not always require looking at a new phenomenon. Established and well-researched phenomena can also be reconceptualized using a completely new theoretical lens. As Kuhn (1962: 49) noted, "The prelude to much discovery and to all novel theory is not ignorance, but the recognition that something has gone wrong with the existing knowledge and beliefs." The aim of consensus-shifting theorizing is to challenge the taken-for-granted assumptions and logics in established research that explain a seemingly well-understood phenomenon. These limitations lay the foundation for introducing new explanations counter to established consensus in the field (Hollenbeck, 2008). Consensus shifting can be triggered by powerful empirical anomalies and inconsistencies that contradict existing theory or widely accepted theories from another discipline used to negate the logic of existing theory. The critical but challenging aspect of theoretical consensus shifting is to convincingly prove existing thinking to be wrong and prove new proposed theory to be correct (Hollenbeck, 2008). Achieving these two aspects requires in-depth understanding of the existing research that allows researchers to then track the irregularities, anomalies, and counter examples that can help develop a convincing argument for presenting an alternative explanation for a given phenomenon.

One particularly notable example of consensus shifting pertains to the evolution of explanations of superior firm performance. The early structure–conduct–performance explanations from industrial organization economics laid the foundation for development of a paradigm in strategy by theorizing industry factors as the main drivers of firm strategies and performance (Porter, 1981). This dominant explanation was challenged by subsequent theories such as the resource-based view (Barney, 1991), which highlights heterogeneity in resources and capabilities of firms as critical determinants of firm success, and upper echelons theory (Hambrick & Mason, 1984), which emphasizes managerial cognitions.

Diverse Theoretical Integration

The theoretical integration approach entails taking two established perspectives that speak to the same or similar phenomena in different ways, bringing their insights together in a novel way to present a holistic understanding. A promising motivation for radical theorizing, though, is to reconcile vigorous theoretical tensions and contradictions in explaining a phenomenon and generating theoretical agreement about how to explain the phenomenon. The latter approach focuses on "clear lines of discrepant thought simultaneously existing in the literature" (Hollenbeck, 2008: 020). Yet, integrating contradicting theories can be very challenging. As Mayer and Sparrowe (2013) noted, many attempts to integrate theories fail to achieve their intended goals because of difficulties in handling differences between the theories and deciding the type of filters to be used to determine what to bring together from the theories. For diverse theoretical integration, what is needed is a vigorous internal debate in the professional literature, with the aim of shedding light (and not just heat) on the issue.

An example of such integration is a study by Chattopadhyay, Glick, and Huber (2001), who reconciled the contradictory predictions of threat–rigidity theory and prospect theory about how executives respond to opportunities and threats. They differentiated the underlying dimensions of threats and opportunities—controllability and gain–loss—to posit that controllability aspects of the threat–opportunity categorization are more pertinent to threat–rigidity theory, whereas gain–loss dimensions of the categorization are more in line with prospect theory. By differentiating the threat–opportunity categorization, they highlighted the complementarity of the two theories, which were earlier deemed as competing (either/or).

LEARNING FROM RADICAL THEORIZERS OF THE PAST

Armed with these possible approaches of radical theorizing, it may be informative to look backward to see what we can learn from some of management's most prototypical radical theorizers (Smith & Hitt, 2005). History reminds us that we can all take meaningful steps toward being more avant-garde in our explanations of phenomena in the management discipline. Admittedly, some dispositional factors these giants of the past enjoy are not accessible to us all. For example, Victor Vroom has described how vocational proficiency tests identified him at a young age as well suited to being either a musician or psychologist. His apparently innate combination of imaginativeness and perspicaciousness may have been central to his ability to formulate expectancy theory (Vroom, 1964). We suggest, though, that lesser mortals like ourselves can also become more radical in our theorizing by carefully attending to the ways of some truly exceptional role models.

The first of these is Denise Rousseau, creator of psychological contract theory (Rousseau, 1989), who developed her work via the rigorous process of inductive theory generation (see above and Figure 1). She describes how her work originated in her father's dissatisfaction with his career, emerged through inordinate amounts of time observing organizations, and developed by hashing out the ideas with a wide range of brilliant colleagues in many disciplines. She diligently sought to find her ideas played out in the world via the pages of the *New York Times*. One thing we notice from Rousseau's characterization of her work is that radical theorizers gain broad knowledge from a wide range of sources. As scholars, the saying goes, we specialize by "continually learning more and more about less and less." This concentrated focus may be necessary to produce research efficiently. However, in doing so, we create routines that favor deep, domain-specific knowledge over wide knowledge from multiple domains, diminishing our ability to develop radical theory. Intentionally pursuing varied types of knowledge gives us a base from which to develop new ways of seeing, thus freeing us from the "nearness trap," wherein we prefer ideas that are near to existing approaches (Ahuja & Lampert, 2001).

Another radical theorizer of note is Barry Staw, an evocative theoretical boundary spanner who helped us understand what he called "escalation of commitment" (Staw, 1976). Staw, who, as a young man, was classified as "available for service" for Vietnam, noticed something unique about the war effort. He saw the conflict as a series of commitments that were hard to break. Setbacks, therefore, were not a signal to withdraw but rather a sign that more resources were required. This led him to theorize that escalation often emerges as an effort to rationalize a previously committed course of action, which was borne out in his famous "Big Muddy" study. One thing we learn from Staw is that radical theorizers appear to embrace openness to new ways of seeing. After all, the starting point for radical theory is often dissonance between a firmly embedded viewpoint and observation of phenomena that contradict the view. Openness to new ideas that uncover apparent contradictions frees us from the "maturity trap," wherein we prefer the mature to the nascent (Ahuja & Lampert, 2001).

Lastly, we draw attention to a radical theorizer who worked with a diverse set of coauthors, and persisted to overcome likely formidable hurdles inherent to advancing theory in novel ways by integrating theory across disciplines: Ken Smith. Smith's contributions explore macro phenomena such as top management teams, venture growth, and firm actions, but with incorporation of more micro-based theory such as demography, cooperation, knowledge, and communication (Collins & Smith, 2006; Smith, Carroll, & Ashford, 1995; Smith, Smith, Olian, Sims, O'Bannon, & Scully, 1994). We view Smith as an evocative boundary spanner insofar as he shaped the competitive dynamics and competitive advantage research in strategy. By infusing the Austrian School of economics theories (Kirzner, 1973) into strategy research, he highlighted the dynamic nature of competitive interactions at the dyadic interfirm level. His work, along with that of others, charted a new direction by introducing the competitive dynamics area. One thing to be learned from Smith is the importance of building diverse authorship teams and being passionate and fearless about ideas despite inevitable pushback from not fitting squarely into an established discipline, level of analysis, or epistemology. Radical theorizers often have to be so because those around them may be nestled in the "familiarity trap," preferring the familiar to the unfamiliar (Ahuja & Lampert, 2001).

PRACTICAL TIPS: HOW TO GET STARTED

Given what we have learned about prior radical theorizing, we also want to look forward, offering suggestions by which we might all become more radical in our theorizing. First, because the ability to generate novel theoretical approaches is based on the availability of ideas, one of the most foundational first steps is to heed Rousseau's example by working to expose ourselves to a broad range of knowledge. In essence, you can engage in *academic wanderlust*—questioning the tried-and-true approaches by engaging with ideas in domains not only adjacent to your own but distal to them. While it is tempting to focus narrowly on reading only what we need to write our own papers ("local search"), we advocate reading beyond your own discipline, across widely different areas of scientific study and epistemologies ("distant search"). For instance, you might set up journal alerts from related disciplines and scan article titles and abstracts. Ask yourself, "How do these scholars' world views and approaches differ from (and challenge) mine?" Furthermore, take advantage of social media, like Twitter and other platforms, which can provide you with a vast amount of quickly digestible information, including new research as well as others' views and experiences. In addition, it may be worthwhile to attend conferences other than those in business or management to get exposure to new ideas. It may not always be the case that such philandering will offer useful insights—perhaps sometimes it may even get us into trouble—but, to plan for the radical, some inefficiencies have to be tolerated.

Second, like Barry Staw, we suggest it is critical to embrace conflict, disagreement, and questions, because they could be pointers to anomalies and limitations in existing theories and establish the seed for new theorizing. Cherish points at which you disagree with others about predictions and phenomena. Build this, like Ken Smith, into your authorship teams. Rather than trying to shut others down and win arguments, investigate. Figure out the boundary conditions and assumptions you each have that are behind your views. Question these assumptions and predictions—when might they be wrong? When might the opposite pattern emerge? In this sense, it is important that we pursue rather than dismiss irregularities. Fleming saw mold in his samples, but he still had to take what Mintzberg calls the "creative leap" of pursuing what it could mean rather than dismissing findings that did not fit his worldview (Mintzberg, 1979). Might things that are frequently dismissed because they are outside of your, or your literature's, scope or world view be opportunities for novelty?

Third, like Rousseau, try to see the world through others' eyes (students, friends, family, etc.)—what is surprising to them about their work, their organization, or their industry? Why might it be? Ask people what others would find surprising to learn about their work, workplace, or industry, and why is that? Students can be a source of fresh ideas and perspectives—encourage them to explore their ideas, and try to mute your inner critic. Then, try to see why others find ideas new and interesting, and investigate if there is disagreement. When developing radical theory, not everyone is going to love your ideas, as radical theorizers can attest. Your ability to have your ideas accepted, though, may largely be a function of the extent to which you can link your solutions to problems as others see them. Therefore, listen intently and ask more questions than you answer. Do what Sutton (1997) referred to as "closet qualitative research." You might also reach out to scholars in other disciplines, as they follow different paradigms that may hold the key to solving problems in your own field (Gruber, Harhoff, & Hoisl, 2013).

Fourth, to identify new problems, you might consider spending time as a *corporate anthropologist*. Instead of spending your entire sabbatical at different academic institutions or attending the conference circuit, we suggest embedding yourself in organizations, particularly those that face problems that have proven resistant to being solved or that work with entirely new approaches. Observing these kind of environments can not only provide insights into the limits of existing theories but also help identify problems that have escaped the attention of scholars.

Finally, we also believe it is key to consider radical theorizing in view of the entirety of your research. If you are anything like us, you probably think about your work in terms of individual studies rather than an integrative portfolio of projects—an innovation portfolio. Research suggests that organizations that consistently produce above-average returns have learned to manage their innovation projects as part of an integrated whole rather than thinking of their innovative efforts as sets of ad hoc and isolated efforts (Nagji & Tuff, 2012). What is true for organizations may also be true for us scholars—when thinking of our projects as stand-alone efforts, we may have difficulty articulating our overall research strategy (e.g., in our promotion packet). More problematically, we may spend too much of our limited resources, money, time, and energy on the tried and true and not enough on new-to-the-field work—a strategy, which, over time, may jeopardize the overall returns to our research efforts (such as citations).

What are the characteristics of an innovation portfolio that produces above-average returns? The work by Nagji and Tuff (2012) offers some insights. These authors examined companies in the industrial, technology, and consumer goods sectors and looked at whether any particular allocation of resources across three different types of projects—core, adjacent, and transformational—corresponded to significantly better performance. "Core projects" are those that are firmly in our area of expertise and designed to apply existing theories to problems central to our field of inquiry. "Adjacent projects" are those that stretch our expertise by, for example, utilizing theories from adjacent domains to address problems central to our field of inquiry. "Transformational projects" require us to acquire new expertise sets in an effort to build new theories for existing or newly identified problems. Nagji and Tuff (2012) found that companies that allocated about 70% of their efforts to core initiatives, 20% to adjacent ones, and 10% to transformational ones outperformed their peers. When we think about how to spend our limited resources, we may want to follow a similar breakdown.

Balancing our portfolio according to these findings is easier said than done. Previous research has found that evaluators have an inherent distaste for novelty (Boudreau, Guinan, Lakhani, & Riedl, 2016). If we are anything like the people in that study, when selecting new projects, we run the risk of favoring—too much—the incremental over the radical. Given the greater likelihood of success of more traditional or incremental projects, radical ideas are likely to be crowded out over time. Just like retirement portfolios have to be rebalanced every now and then, the same might be true for our research portfolios. We suggest there is value in taking stock on an annual basis and reflecting on the performance of our portfolio and its composition—dropping underperforming projects and making sure that we dedicate sufficient effort to more radical projects by saying "no" to new projects in the 70% share.

In conclusion, we hope that this editorial stimulates the appetite of scholars to engage in radical theorizing. In order to be able to achieve significant scientific leaps—some of which may be required to solve the world's most pressing problems—scholars have to embrace radical theorizing in generating "New Ways of Seeing."

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