

## INTRODUCTION TO SPECIAL TOPIC FORUM

### WHERE ARE THE NEW THEORIES OF ORGANIZATION?

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We summarize the manifest and latent content of the articles that make up the Special Topic Forum on Theory Development. Rather than offering new theories, however, most of the articles offer a series of critical accounts of current organizational theory and a range of novel ideas about the process of theory construction. We conclude by speculating about the institutional barriers to new theory creation and how those barriers might be changed.

This special topic forum (STF) was inspired by the observation that most of the theories of organization used by contemporary management researchers were formulated several decades ago, largely in the 1960s and 1970s, and these theories have persisted, mostly intact, since that time. This is so despite massive growth and change in the size, prevalence, and influence of organizations in modern society. Organizational theory, as Davis has observed, can sometimes appear to be a “living museum of the 1970s” (2010: 691). Where, we asked, are the new theories of organization?

Few of the manuscripts we received in response to our call actually offered new theories of organization. In retrospect, this was perhaps an overly ambitious expectation on our part. The articles that make up this special topic forum, however, offer an arguably more interesting interpretation of our call. Rather than generating new theories, they collectively make two unique contributions. First, they offer an implicit critique of contemporary theories of organizations. Their critique has three main arguments: (1) as a discipline, we have failed to develop our own theories; (2) our theories fail to capture the rich manifestation of organizations in society; and (3) our research and theorizing is an inherently conservative practice. Second, and more optimisti-

cally, the articles offer a clear direction forward by identifying a powerful set of suggestions regarding how we can generate new theory. They collectively point to a need to become more attentive and self-reflective regarding the process of theory creation.

#### CHALLENGES FACING CONTEMPORARY THEORIZING

Foremost, the articles offer a collective and self-reflective critique of the current state of theory in management research. In fact, they offer a surprising degree of agreement on the challenges that contemporary organizational theorists face.

#### Generating Indigenous Theory

Many of the authors in this STF make the observation that management research has failed to cultivate any truly *indigenous* theories of management and organization. Oswick, Fleming, and Hanlon make this observation most clearly, convincingly demonstrating that most (i.e., roughly two-thirds) of the research conducted in management is rooted in theories borrowed from other disciplines. Their point is reinforced by both Shepherd and Sutcliffe and

Sandberg and Tsoukas, who note that current organizational theory is inattentive to and disconnected from contemporary management practices.

This line of argument is in sharp contrast to earlier observations that management theory had succeeded in establishing itself as an independent discipline. Pfeffer (1997), for example, argued that as early as the 1980s the study of management had successfully migrated from the core disciplines of engineering, psychology, and sociology to form its own category of scholarship. Hinings similarly noted that as researchers moved from psychology and sociology faculties to work in business schools, organization theory "developed as a discipline in its own right with its own problematics, theoretical structures and methods" (1988: 2).

The articles that make up this STF, however, cast doubt on this conclusion. Instead, they paint a picture of a discipline that has somewhat awkwardly imported theories, research questions, and methods from foreign disciplines without fully adapting them to the new context. Rather than treating organizations as a subject of inquiry in their own right, management theorists appear to have treated organizations merely as new empirical sites to test, prove, and tweak old theories.

The prevalence of retrofitting theories borrowed from outside disciplines within management research is well documented (e.g., see Floyd, 2009; Suddaby, 2010; Whetten, Felin, & King, 2009). Less well understood are the problems and long-term implications of doing this. While we have some sense that core constructs and methods borrowed from other disciplines may have to be adapted to accommodate the empirical context of organization, there is no well-articulated understanding that theories generate both ways of seeing and ways of not seeing. When we import theories from psychology and sociology, we also import core questions, assumptions, and metaphors, each of which has the potential to create blind spots for management researchers.

Perhaps the most glaring consequence of a lack of indigenous theory is the growing chasm between management research and management practice (Bartunek & Rynes, 2010; Pfeffer, 2007; Rynes, 2007). Pferrer and Fong (2002) examined the impact of management ideas on management practice and concluded that most inno-

vative ideas in management practice come from consulting, from business journalism, and from companies themselves, rather than from management theorists. Numerous other studies and essays (i.e., Bartunek, 2007; Davenport & Prusak, 2003; Mol & Birkinshaw, 2007) reinforce the basic understanding that since the emergence of organization theory as a separate discipline of research some fifty years ago, management theories have been divorced from management practice.

The critique that the articles in this issue offer suggests an answer to why this gap exists and how it has managed to persist for so long. The answer, in short, is that management theory has not yet lost its colonial roots. We still look to the "founding fathers" for our fundamental questions and our methods for answering them. We carry the historical baggage of their underlying assumptions. And, like most colonial outposts, we retain a sentimental attachment to the tools, constructs, and limitations of our core disciplines (Weick, 1996).

Collectively, thus, the articles in this STF have reformulated the original question in our call for papers to ask not for new theories of organization but, instead, to ask, "Where are the indigenous theories of organization?"

### **Capturing the Empirical Complexity of Contemporary Organizations**

A related theme shared by the STF articles is the critical observation that current management theories have failed to keep pace with changes in the size, complexity, and influence of modern organizations. That is, as an extension of the growing chasm between management theory and practice, there is a concomitant underestimation of the significance of organizations in modern life and a lack of attention to their complexity, influence, and power.

When management theory was in its early stages of formation as a distinct discipline, there was an excited awareness and vigorous discussion about the growing significance of organizations in contemporary society. These concerns were reflected in both the popular and academic press. One of the most popular accounts of the phenomenon was William H. Whyte's 1956 book, *The Organization Man*. The book described, with a mix of apprehension and awe, the growing impact of organizations on

American life. Shortly thereafter, Robert Presthus, in a book called *The Organizational Society* (1963), documented the growing influence of organizations on human behavior and personality. Presthus noted that with the emergent dominance of large organizations after the Second World War, there was a concomitant emergence of a "bureaucratic personality," characterized by a concern with careerism, a reduction in entrepreneurial behavior, and an increasing tendency to subordinate individual interests to the demands of the workplace. These popular texts drew from seminal research by organizational scholars of the day, such as Selznick (1949), Gouldner (1954), and Michels (1959), that focused attention on the rise of organizations as a dominant social institution. The books were part of a growing fascination with organizations, both in their own right and as a relatively novel and potentially dangerous phenomenon.

Since that time, the phenomenon of organizational dominance captured by Whyte and Presthus has significantly accelerated. Organizations have become much larger and more complex than they were in the 1950s and 1960s. In 1955 General Motors was the largest corporation in the world. It employed 624,000 individuals and earned a profit of \$1.2 billion, prompting then President Charles Wilson to proclaim to the U.S. Congress that what was good for GM was good for the country. Today, the largest corporation (Walmart) generates nearly \$14 billion in profit and employs 2.1 million people in over 15 countries around the world.

Organizations have also become much more powerful than they were when Whyte and Presthus wrote their books. Corporations now rival countries in terms of productivity. Of the 100 largest "economies" in the world, 51 are corporations (Korten, 2001). While corporations do not yet have the right to vote, they have acquired substantial power to fund and influence the outcome of elections in the United States (Clawson & Neustadt, 1989).

In sum, the world that Whyte and Presthus were so concerned about seems to have become reality. Yet this reality is not adequately reflected in contemporary management theory. Writing in 1990, Richard Daft and Ari Lewin, the editors of *Organization Science*, expressed concern about the inability of management research to keep pace with the modern complexity and power of organizations. Noting the "cata-

clysmic changes occurring in the environment of organizations," Daft and Lewin (1990: i) observed that management research simply was not keeping pace with those changes and concluded by expressing their concern "that organization theory . . . [was] in danger of becoming isolated and irrelevant" to the empirical reality of organizational society.

This conclusion is shared by the contributors to this special issue. Smith and Lewis premise their argument on the observation that extant management theories are too simplistic and static to fully capture the dynamic changes in the size and complexity of modern organizations. Such complexity, Smith and Lewis argue, increasingly generates *tensions, dualities, paradoxes, or contradictions* in organizations. A number of theorists have attempted to adapt existing theories, such as contingency theory, institutional theory, or identity theory, as a means of dealing with this phenomenon, with varying degrees of success. Other theorists have generated new terms for this research, including *ambidexterity* research or *exploration/exploitation* theory. Smith and Lewis weave together these somewhat disparate threads to create an organizationally indigenous theoretical perspective that they term *paradox theory*.

Sandberg and Tsoukas point to scientific rationality as the primary reason that organizational theory has become detached from the reality of organizational life. The slavish adherence to scientific rationality, these authors argue, creates three wedges between management theory and organizational reality. First, science is reductionist, and in the process of particularizing organizational phenomena, it eliminates the gestalt of experience that is meaningful to those engaged in organizations. Second, science seeks to generalize, and as a result of aggregating phenomena to broad categories, it eliminates the contextual reality of organizational experience. Finally, science abstracts the subjective experience of time. Because of this, Sandberg and Tsoukas observe that management theorists lose the holistic grasp of organizations. Their advice is for management theorists to replace scientific rationality with a more practical rationality.

An alternative suggestion, raised by Kilduff, Mehra, and Dunn, is based on the observation that science is a fairly broad category subject to multiple ontological and epistemological inter-

pretations that can generate a wide range of types of science, each of which holds different implications for organizing. The same should be true for management theory. That is, if there are multiple interpretations of science, then management scholars should benefit by adopting a version that offers a better fit between the empirical context and the philosophical assumptions needed to theorize them.

Collectively, thus, the STF articles have also reformulated the central question in our call for papers to ask not for new theories of organization but, instead, to ask, "Where are the organizations in our theories?"

### Conquering Conservatism

Many of the STF articles indicate that organizational theorizing is inherently cautious, not only in researchers' reluctance to adopt new theories but in the way they adopt them. So, for example, Osrick and colleagues note the "domestication" of foreign theories when they are adopted by organizational scholars. This occurs as the theories are taken up and consumed within organization and management theory (OMT)—a process that involves repeated recontextualization as initial ideas articulated in texts produced outside the field are rearticulated in texts published in OMT journals and books. This process is repeated many times, with the ideas appearing in "seminal" OMT work then being taken up by other organizational researchers and appearing in subsequent texts. As Maguire and Hardy (2009) point out, this process, where original texts are successively taken up in subsequent texts, involves the translation of meaning. Initial ideas intended by the original author are not necessarily reproduced and are just as likely to be modified, transformed, or subverted. Thus, the conceptualization of metaphor imported from elsewhere (e.g., Black, 1962; Ortony, 1975) was initially recontextualized in texts authored by OMT researchers such as Gareth Morgan, whose highly cited work (e.g., Morgan, 1980) then appeared in articles on organizational themes ranging from management consultancy to improvisation and organizational change. Osrick and colleagues argue that, as a result of this process, theoretical ideas pertaining to metaphor were made more seductive and instrumental, leading to a reification of the phenomenon itself.

Translation may be inevitable, but the interesting questions are "What makes domestication as a particular form of translation so ubiquitous?" and "Why are meanings neutered and sanitized during their translation in OMT, rather than radicalized and extended?" The contributors to this STF offer some suggestions.

One reason alluded to in some of the STF articles may lie with the exacting pressures of work facing contemporary academics and the cognitive limits of researchers in processing the huge amount of published work that now exists. As Shepherd and Sutcliffe point out, the bodies of literature relevant to OMT are vast, diverse, and, with the growing number of journals in the field, increasing rapidly. They go on to argue that, consequently, preconceived notions about what is important stem from what is known and familiar. In addition, theorists may be drawn to "overworked localities in organizations, to ready-made problems, to fashionable styles of thinking" (Shepherd & Sutcliffe, this issue: 363). Individuals may not therefore have the time or intellectual resources to develop exciting, disruptive, and challenging new theories. Alvesson and Sandberg suggest that most theorists have no such aspirations anyway. They argue that only certain theories—social constructionism, postmodernism, feminism, and critical theory—are concerned with radicalizing ideas and undermining conventional wisdom. Most theorizing has far more conservative aims. As Shepherd and Sutcliffe argue, "top-down" deductive theorizing—one of the common ways of building theory in our field—aims at discovering a problem in the literature, such as a tension, opposition, or contradiction among divergent perspectives, and then sets out to find a solution. Alvesson and Sandberg refer to this as "gap-spotting," rather than the more radical "problematizing" that they propose. Inductive theorizing from the "bottom up"—another common approach—is no better because, according to Shepherd and Sutcliffe, it tends to be limited to rich descriptions of specific cases, rather than producing more abstract theories. Should we therefore only expect to "inch" toward new and better theories, as suggested by these authors?

A second reason that may explain such conservatism is associated with the widely accepted practices of crafting journal articles that dominate the field. As has been noted elsewhere, journals are produced by organizations



that enact highly predictable and routinized bureaucratic processes, and academic communities are institutionalized fields with widely shared norms about publishing (Alvesson, Hardy, & Harley, 2008; Boxenbaum & Rouleau, this issue; Gabriel, 2010). The effects of bureaucratization are compounded by the politics of publishing (Grey, 2010). Defining a gap in the existing literature is supported by widely shared institutionalized practices and is likely to be more politically acceptable than proposing problematizations that undermine its fundamental assumptions; the exact nature of any gap will be the result of negotiations among researchers, editors, and reviewers (Bedeian, 2003, 2004). Consequently, as Gabriel (2010: 761) notes in his sobering assessment of the field, "One of the most political processes in which most of today's academics will ever become involved" is publishing an article. What gets published, what gets rejected, and everything that goes on in between are "barely concealed exercises in power and resistance" (Gabriel, 2010: 761).

A third reason for conservatism has been observed by the contributors, who point out that established theories in the field tend to be viewed as sacred canons, making it difficult or awkward to contradict them. In this regard, all researchers are located within larger discourses that dominate our thinking, as noted in Kilduff et al.'s discussion of a range of rationalized logics that have developed within the discourses of the philosophy of science, which not only affect research but are used by scientists to justify their practices publicly and privately. In our own field, dominant discourses also prevail. For example, Sandberg and Tsoukas show clearly how the framework of modern scientific rationality—the scientific ideal of attaining objective and valid knowledge about the world through detached observation and analysis—has dominated and continues to dominate the field. A further problem is that even when trying to resist these dominant discourses, our articles serve to reproduce them. In order to resist them, we must engage with them, and, in doing so, we help to reproduce them: "The process of resistance also involves the reification and reproduction of that which is being resisted, by legitimizing and privileging it as an arena for political contest" (Thomas & Davies, 2005: 700).

Accordingly, when we try to position ourselves as different from X, better than Y, and going beyond Z, we are helping to reinforce and legitimate X, Y, and Z. Whether this is in the form of circumscribed gap-spotting or more radical problematization, or whether it is through the use of more specific suggestions, such as contrastive explanation (Tsang & Ellsaesser, this issue), paradox (Smith & Lewis, this issue), or practical rationality (Sandberg & Tsoukas, this issue), we cannot help but give legitimacy and visibility to the object of our critique. Even if we use bricolage conceptually, we use evolution and differentiation rhetorically, thereby connecting what is novel with what has gone before: evolution reemploys "categories and concepts that are readily available to the researcher," while differentiation "requires juxtaposition with existing knowledge" (Boxenbaum & Rouleau, this issue: 282). In effect, this is what we are supposed to do. As McKinley, Mone, and Moon (1999) proposed, theories should demonstrate both novelty and continuity; they must differ from and at the same time be connected to the established literature in order to be seen as meaningful (Alvesson & Sandberg, this issue). But in making the connection, can we also be truly novel?

The articles in this STF thus provide some interesting answers to our initial question, "Where are the new theories of organization?" While sanguine about the prospects of new theories, the authors nonetheless offer suggestions and solutions to some of these challenges, as we discuss in the following section.

## NEW WAYS OF THEORIZING

Authors in this STF have proposed a number of approaches to develop more creative, insightful organizational theories. At the risk of oversimplifying, we classify these approaches along two dimensions: (1) theorizing within one literature (or knowledge domain) or across multiple bodies of literature and (2) theorizing with implicit assumptions or explicit constructs in the focal literature. Figure 1 summarizes these various approaches, which we briefly describe below.

The top right quadrant of Figure 1 is characterized by sweeping theorizing approaches that focus on combining implicit assumptions across multiple bodies of literature. One approach in-

**FIGURE 1**  
**Map of Different Theorizing Approaches**

	Theorizing within one literature	Theorizing across multiple bodies of literature
Theorizing with implicit assumptions of the literature	- Problematization	- Combining epistemologies - Metaphorical bricolage
Theorizing with explicit constructs of the literature	- Contrasting - Practical rationality - Inductive top-down theorizing	- Blending

involves combining multiple epistemological philosophies to produce a creative theory. A second approach highlights the importance of epistemic scripts—that is, the implicit cognitive templates that underpin our collective understanding of how new academic knowledge is produced. We discuss each in turn.

Kilduff and colleagues propose that organizational theorists should avoid entrenched knowledge silos and should instead adopt a broader view of what we consider “science” by promoting and combining various epistemologies—that is, the fundamental logics that govern the production of new knowledge. These include the logic of pure research, which emphasizes the enduring structural content of scientific theory (e.g., fundamental physics); the logic of induction, which emphasizes the interpretation of patterns inherent in empirical data (e.g., looking for trends in financial data); the logic of problem solving, which emphasizes practical action and an open and interdisciplinary community of ex-

perts (e.g., stopping the oil flow at the Deepwater Horizon drilling rig); strong-paradigm logic, which emphasizes the articulation of procedures to solve outstanding puzzles within paradigmatic communities (e.g., close organizational culture or development centered on one proprietary technology); and the logic of emancipation, which emphasizes subversive challenges to prevailing knowledge assumptions (e.g., green alternatives to standard technology).

Another approach combining implicit assumptions from multiple bodies of literature is proposed by Boxenbaum and Rouleau and involves using the epistemic script of what can be called “metaphorical bricolage.” Conceiving an imaginative new theory often requires scholars to follow the script of bricolage, in which they select and creatively assemble concepts, empirical material, and metaphors from a wide range of literature to create a new perspective on organizational life. Metaphors are readily available elements in the theory builders’ environ-

ment and are by themselves not novel to organizational theory. Rather, it is the specific combination of selective metaphors that acts as a catalyst for bringing a unique and coherent new way of perceiving organizational life. The partial and ambiguous applicability of metaphors stimulates theory builders to be creative in their interpretations and to generate new insights. Assembling multiple metaphors, especially when these come from a wide range of literature, enables the expression of theoretical creativity. The authors illustrate how foundational texts on organizational institutionalism contain an intricate bricolage of metaphors from multiple knowledge domains, including culture, biology, construction, religion, theater, market, system, power, and hard sciences. In a sense, metaphors could be construed as implicit images of or cognitions about our perceived realities. Although formulating new theories requires scholars to follow, often implicitly, the epistemic scripts of metaphorical bricolage, academics generally resort to different epistemic scripts to present and gain legitimacy for their new theories, because metaphorical bricolage has yet to become a legitimate knowledge production tool among the gatekeepers of prestigious academic journals.

The top left quadrant of Figure 1 characterizes other theorizing approaches that still focus on the implicit assumptions but within the boundaries of a given literature. Alvesson and Sandberg suggest that we can generate novel research questions through problematization, which involves challenging the implicit assumptions of an existing theory. They propose a number of principles for researchers to follow: (1) identify a domain of literature where one can focus on challenging its assumptions; (2) articulate clearly the various kinds of assumptions underpinning existing theory; (3) identify limitations and problems of these assumptions; (4) develop new assumptions and formulate research questions; (5) relate the alternative assumption to a targeted academic audience and foresee the audience's likely response to this alternative; and (6) evaluate whether the alternative assumptions are likely to generate a theory that will be perceived as interesting by the target audience. They illustrate how questioning various implicit assumptions in a given literature—organizational identity—can allow scholars to generate novel research questions.

In contrast, the bottom left quadrant of Figure 1 characterizes theorizing approaches that focus much less on implicit assumptions and more on the explicit constructs within a given literature. Tsang and Ellsaesser describe how scholars can challenge a theory's explicit constructs through contrastive explanation. The contrastive approach focuses on comparing the explanatory power of the current key constructs with alternative constructs or explanations. With regard to transaction cost economics (TCE), for example, a contrastive question can be "Why do firms form joint ventures in order to minimize the sum of production and transaction costs rather than maximizing profits through improving their competitive positions vis-à-vis rivals?" The explicit construct of profit maximization through competitive rivalry following "rather than" constitutes the alternative (i.e., contrasting foil) to the traditional TCE focus on organizing for cost minimization. The foil explicitly clarifies specific aspects of the fact that researchers intend to focus on and, thus, makes the task of explaining more manageable. To illustrate, instead of asking the very broad question "Why is there an organization?" Coase's (1937) article focuses more narrowly on why some production activities are organized in firms rather than markets and makes his theorizing task more manageable.

Contrastive questions can help broaden theory. For example, scholars seeking to expand the scope of TCE can ask, "Why are production activities, rather than philanthropic activities, organized within firms?" or "Why are philanthropic activities organized within firms rather than markets?" Contrastive questions can also help develop deeper theories. To illustrate, the single focus of TCE on transaction costs leads some scholars to ask the contrastive question "Why do firms economize on transaction costs rather than maximizing transaction value?" (Zajac & Olsen, 1993). Tsang and Ellsaesser then offer some heuristic rules to help researchers apply the contrastive explanation approach to expand or deepen existing theories.

A second approach that focuses on explicit constructs within one literature involves practical rationality. Sandberg and Tsoukas argue that practical rationality can act as a complement to traditional scientific rationality theories, which construe the world as discrete entities and maintain separation between the object

of study and the researcher. To develop organizational theory that describes and explains more closely managerial practice and, thus, increasing research relevance, researchers should focus explicitly on what practitioners actually do, which tools they use, how they interact with others, and for what purposes. The aim is to capture the logic of practice, which involves (1) entwinement instances in which people are absorbed in their work, routinely act, and are largely unreflexive about their actions, and (2) the temporary breakdowns (e.g., critical incidents) that signal nonroutine situations stimulating practitioners to detach from their routines, reflect, and revise taken-for-granted assumptions. Practical rationality incorporates the recursive patterns of explicitly articulated work interactions, human purposes and motives, contextual richness, and connections among events across time.

A third approach that focuses on explicit constructs within one literature involves what Shepherd and Sutcliffe term *inductive top-down theorizing*. Organizational theorists can develop new insights not through inductive analysis of empirical data alone but by considering the current body of literature (including papers, books, presentations, working papers) as another source of explicit empirical data. By developing a gist (a holistic representation of the literature), researchers can then focus their attention on specific aspects of the literature to identify a tension, opposition, or contradiction, which represents the starting point for novel theorizing. Researchers engage in conscious information processing by performing constant comparison, which provides the raw material on which unconscious processing can generate flashes of insight.

One exemplar of this inductive top-down theorizing approach can be found in Smith and Lewis's development of a theory of paradox (this issue). They reviewed 360 articles focused on organizational paradox that were published across 12 management journals in the past 20 years. Their review reveals a lack of conceptual and theoretical coherence in this vast literature. This enables them to ask fundamental questions, including "What is—and is not—a paradox?" "How are paradoxes, tensions, and dualities as concepts different from one another?" "Can leaders and organizations resolve tensions, or must they accept their persistence?"

"How can leaders deal with paradoxes to impact organizational outcomes?"

Addressing these questions helps them bring more definitional clarity to various constructs in this mushrooming literature and allows them to synthesize various types of paradoxes into four kinds, as well as to integrate the latter into a dynamic model of organizing that emphasizes temporary yet cyclical responses to paradoxical tensions so as to foster organizational adaptation in the short and long term. Exemplars of other articles using inductive top-down theorizing published in the *Academy of Management Review* include Van de Ven and Poole's (1995), in which the authors reviewed and synthesized a large number of academic articles on change at multiple levels of analysis, and Huy's (2001) article on temporal capability, in which Huy proposed various ideal types of various change intervention approaches relying on data comprising academic articles and case examples on organizational change.

Finally, the bottom right quadrant of Figure 1 characterizes a theorizing approach that focuses on explicit constructs and combines multiple bodies of literature. Oswick and colleagues propose that blending explicit constructs from multiple knowledge domains can produce creative output, depending on the characteristics of the input domains. The higher the similarity between input domains, the less likely the resulting theory will be perceived as radically new. For example, the idea of "organizational culture" exhibits modest novelty because the two input concepts, "organization" and "culture," are relatively similar to each other. Both concepts share the same input frame—human social groups—such as the modern workgroup and tribal clans.

To generate radically new insights, blending among noncontiguous domains is suggested. Blending concepts that are very different from or even clashing with one another exhibits high creativity. For example, studying organizations as special kinds of prisons represents a highly imaginative and counterintuitive blend of domain concepts that span freedom and empowerment, as well as incarceration and control.

Four specific blending approaches can be used: (1) focusing on dissimilarities among similar domains (e.g., how is managing different from leading?); (2) highlighting seemingly dichotomous concepts that are, in fact, mutually



implicated (e.g., organizational resistance could be a form of organizational compliance); (3) using counterfactual reasoning, where the conventional logic is inverted (e.g., exploring how activists help organizations, whereas consultants work against organizations); or (4) using anomalous reasoning, comparing disparate and unrelated domains on the basis of similarity (e.g., organizations versus slavery). Obviously, this blending process is both challenging and risky in that it requires a high degree of imagination and has low odds of producing new creative insights that underpin radically new theories.

Many authors do caution, however, that their suggested approaches might help but cannot guarantee success in creative theorizing. Applying any methodology systematically does not guarantee a creative outcome. A host of other factors are important, such as imagination, reflexivity, scope of knowledge mastered, and a broad understanding of different metatheoretical perspectives. Moreover, direct application of one literature to another is seldom a rewarding approach. When we borrow from existing theories, we also have to return "dividends" to the source. For example, if organization theorists borrow from psychology to develop the idea of "organizational memory," they should return fresh insights to the original source idea of memory itself. A creative contribution reflects a two-way exchange of new insights.

## CONCLUSION

So, are we delusional in seeking or expecting novel new theories, and, if not delusional, are we—in this STF—closer to or farther away from finding novelty and innovation? Have things changed since the last special issue on theory in this journal, which Weick characterized as "a safe rather than bold set of articles" (1999: 803)? The editors of that issue did not disagree but argued that this was because the bold work had already taken place: the battles had been fought and won, and the special issue represented a more measured approach to consolidate those gains.

In 1989, the notion that skilled organizational theorists are not just objective scientists, but also good storytellers who weave convincing tales of cause and effect, was unknown in many corners of the field and ridiculed in most others. It is now accepted by a large proportion of organizational

theorists and is often used as an argument—even by some of the most traditional, quantitative, and positivistic researchers we know—about why good theory requires using many of the same methods as good literature. In 1989, the notion that a good theory could be developed that lacked independent and dependent variables was new and controversial; it is now viewed as a trivial truth by many of the same people who found this idea to be absurd. We could name at least fifteen respected organizational theorists who would have rejected process theories as "unscientific" in 1989 but find such theories to be a useful and important way of describing and explaining events in 1999. Finally, in 1989, the notion that there is no objective means of assessing the value of a theory would have been viewed as heresy in many of the same corners that it is now accepted as given truth, or at least a plausible and troubling possibility (Elsbach, Sutton, & Whetten, 1999: 633).

These gains have certainly not been lost in the last decade—but perhaps it is now time for less consolidation and more provocation once again. If we truly want exciting new theories, we must brace ourselves for a return to the senseless landscape and "the insane pursuit of originality" noted by Weick (1999: 803). Taking such a direction requires us to reflect on the nature of the task, as well as the ways in which existing practices that may make this task more difficult have become institutionalized.

One question that must be asked, therefore, is whether we should try to find bold new theorizing in "top-tier" U.S. journals, such as the *Academy of Management Review*. U.S. journals have been criticized for their self-referentiality (Grey, 2010; Meyer & Boxenbaum, 2010; Üsdiken & Pasadeos 1995; Wasti, Poell, & Çakar, 2008). If, as Oswick and colleagues suggest, the tendency is to rely on finding novel ideas from outside the field and if, for the reasons above, we cannot defeat domestication, then should we look to non-U.S. journals to provide intellectual innovation because of their predilection for citing more widely than their U.S. counterparts and for being more likely to cite philosophers, sociologists, and other influential intellectuals from different linguistic communities (Meyer & Boxenbaum, 2010)? Or, perhaps, do we need a more radical overhaul of publication procedures, such as that advocated by Tsang and Frey (2007), where papers should be accepted "as is" on the basis of a one-time review?

Another question concerns whether we should rely on journals at all to find bold new theoriz-

ing. Certainly, the place of journals as outlets for our work has been consolidated with the advent of business school rankings, league tables, and research assessment exercises. However, the growth in journals has been at the expense of research monographs.

A recent visit to London's largest historical bookshop revealed a mere four short shelves of books on organizational theory (rather less than the shelf space in sociology occupied by authors between Bauman and Bourdieu), nearly all of them textbooks. Instead of writing or reading books, there are now more academics each seeking to publish more papers in a larger number of journals (Gabriel, 2010: 762).

Is this perhaps another reason why there are no new and exciting theories? Do we need books to provide the space within which we can be novel and where we do not have to pay such expensive homage to those who have gone before? Have recent changes in the field compromised our ability to innovate theoretically? And, if so, can we do anything about it? Can we maneuver between the institutional constraints, or can we act in ways that release constraints? Is it, as Weick (1999: 803) intimated in the last special issue, that there is nothing bold left to say, or is it, with the long-standing drive to publish, not perish, coupled with the more recent rush to dominate the rankings and league tables, that there is no longer any place left in which to say it?

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