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INTRODUCTION

PERSPECTIVES ON DEVELOPING MANAGEMENT THEORY, CIRCA 1999: MOVING FROM SHRILL MONOLOGUES TO (RELATIVELY) TAME DIALOGUES

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The publication of this special issue marks the tenth anniversary of the first *AMR* special issue on theory development, edited by David Whetten and published in October of 1989. The diversity of organizational theory and the range of ideas about how to develop organizational theory increased dramatically during the decade following that special issue. There has been vigorous and unresolved debate about whether such diversity is healthy for the field. There is far more consensus, however, that more integration of these varied ideas would help move the field forward. The set of articles in this special issue falls short of offering a single grand and persuasive framework that weaves together many or most major organizational theories. Compared to the set published in 1989, however, these articles suggest that our field is making discernible progress toward this goal.

As Karl Weick observes in his insightful conclusion to this special issue,

> The image of paradigm "wars" has the unfortunate connotation that it suggests the path to victory and truth lies in monologues that overwhelm rather than in dialogues that reconcile.... But there is hope. There are invitations to conversation densely seeded throughout the articles in this special issue (p. 804).

Weick's optimism inspired us to highlight the opportunities for ongoing, fruitful, interesting conversations about theory development suggested by the contents of this special issue. In Weick's language, our introduction to these articles focuses on propagating the "conversation seeds" we found in them.

The nine articles in this special issue cluster around three themes: (1) metatheorizing and the development of metatheories, (2) theories of process and time, and (3) new approaches to building "thick" theory. Our brief introduction to each of these themes highlights the conversation seeds embedded in each of the related articles that we found most provocative and promising.

THEME 1: METATHEORIZING AND THE DEVELOPMENT OF METATHEORIES

Much "critical" attention has focused on metatheory questions since the publication of the 1989 special issue on theory development. A growing and diverse group of scholars has struggled with questions like the following. How do we theorize? What do we theorize about? Why do we theorize? Who theorizes? How do we use the theory? What do our theories look like? (For example, see *Administrative Science Quarterly,* 1995.)

The term *meta* is used in two different ways by the authors of the first group of articles in this issue. Both William McKinley, Mark Mone, and Gyewan Moon, and Marta Calás and Linda Smircich, propose a "theory of theorizing" in organizational science. In contrast, Marianne Lewis and Andrew Grimes, in their article, critique the field's heavy reliance on single-paradigm research designs and propose a multiple-paradigm alternative that can be used to generate metaparadigm theories of organizations.

In their article, "Determinants and Development of Schools in Organization Theory," McKinley, Mone, and Moon tackle the broad sociology of science question: How do new schools of thought get accepted in organizational science? According to these authors, the primary challenge facing champions of new theoretical frameworks is "getting noticed" amidst the din of attention-sapping theoretical noise. The authors claim this is most likely to occur when a new school of thought is different enough from "received wisdom" to warrant a second look, but similar enough to what is known that it is comprehensible. In addition, they argue that the scope of a theory is positively correlated with its level of acceptance as a research tool.

Calás and Smircich do not address this question directly, but they do articulate a strong position regarding the adoption and diffusion of organizational theories. The essence of this argument is that, in this age of poststructuralism, there is no "objective" means of assessing the value of a theory because there is no stable structure of signification upon which meaning can rest. Therefore, theory development, like all other forms of discourse, is basically a political act. Given the problemization of author and subject inherent in the postmodern perspective, these authors assert that the legitimacy of the knowledge produced by a theorist is inextricably connected with the power of the knowledge maker.

In "Post Postmodernism? Reflections and Tentative Directions," Calás and Smircich pose a different "metatheorizing" question: (How) Can we do theory differently? Given the heavy emphasis that the postmodern perspective places on textual analysis, it is no surprise that Calás and Smircich point to postmodernism's impact on the way we write organizational theory. They argue that contemporary works in organizational science are likely to question for what, for whom, and by whom what is represented (in our theories) and what is not. They argue further that postmodernism has opened the "margins" of organization studies to be written by and for others whose theoretical voices have seldom been represented in our scholarship.

McKinley et al. share Calás and Smircich's interest in the relationships among scholars. Their interests, however, are strikingly different. McKinley and coauthors argue that scholars must understand their colleagues' needs and concerns so they can "design" their theories accordingly. In contrast, Calás and Smircich's appeal is for scholars to "open up space" for the marginal voices of their colleagues to be heard, because only by being heard can scholars gain legitimacy, which is the coin of the academic realm. Although this is not the primary focus of their article, Lewis and Grimes' response to the Calás and Smircich question is that scholars need to think differently. Lewis and Grimes argue that theorists need to embrace paradoxical thinking—to replace the flat narratives of single-paradigm theories with rich descriptions flowing from multiple-paradigm analyses.

The two articles on metatheorizing also have distinct views regarding the criteria of legitimacy. For McKinley et al., the legitimacy of a theory is reflected in its acceptance. If a large number of scholars are using a theoretical tool to guide their research, then it is deemed legitimate. In contrast, legitimacy is defined by Calás and Smircich as acceptability—that is, legitimate knowledge can only reside in small stories or modest narratives (that are mindful of parameters like locality, space, time, and so forth). This disagreement involves more than an esoteric distinction. If McKinley et al. are correct that the scope of a theory is correlated with its level of acceptance as a research tool, then there appears to be a conflict between what is required for a theory to gain acceptance (legitimacy and power for the theorist) and being deemed acceptable (credible).

Shifting from metatheorizing to developing metatheory, in their article, "Metatriangulation: Building Theory from Multiple Paradigms," Lewis and Grimes make a strong case for using multiple-paradigm theoretical lenses in the study of organizations. In addition, they lay out a detailed, multistep process, beginning with multiple-paradigm literature reviews, followed by multiple-paradigm research designs, and culminating in metaparadigm theory development. According to these authors, the primary advantage of a multiple-paradigm approach is that it fosters an enlarged and enlightened understanding of highly complex and inherently ambiguous organizational phenomena. Rather than seeking for "the" truth of a subject, the objective of a multiple-paradigm form of inquiry is to foster a more comprehensive understanding.

Examining Lewis and Grimes' call for multiple-paradigm research, from the perspective advocated in McKinley et al.'s analysis of the factors affecting the adoption of new schools of thought, raises some important institutionalization questions. For example, would the acceptance of the multiple-paradigm approach be higher or lower if it were framed as a psychological or as a sociological intervention? That is, should organizational theorists focus on cultivating a richer set of organizational experiences so they are more capable of generating complex views of organizing, or should they focus on cultivating the formation of research teams staffed with theoretically diverse members? Holding the two perspectives in tension, we wonder: Is it harder to break down the prejudices inherent within any given set of truth claims by training scholars in dualistic, paradoxical thinking strategies or by enhancing their skills in transforming destructive theoretical diatribes between unlike-minded scholars into constructive theoretical discourse?

Another striking difference between the metatheorizing views of McKinley et al. and Calás and Smircich is their choice of legitimating logic. McKinley and coauthors' arguments are very instrumental, whereas Calás and Smircich's views carry a strong moral connotation. The contrast between these legitimating perspectives is highlighted by the questions each would bring to a conversation about implementing Lewis and Grimes' metatriangulation approach to research.

The instrumental point of view would involve asking questions like the following. What decision rules should be applied in composing an "effective" set of multiple perspectives? Should the "set" be constructed so as to stretch the thinking of the theorist, to match the characteristics of the subject matter or organizational setting, to maximize the opportunity for paradoxical reasoning, or simply to try out new combinations? And how does one gauge the point of diminishing returns when adding more paradigmatic lenses? At what point does the marginal utility of additional paradigms no longer justify the associated increases in search and coordination costs?

In contrast, the moral perspective would involve asking more severe questions in a more personal manner. Which paradigms are not present in the set from which I selected my multiple lenses? Why aren't they part of the choice set? Why am I choosing these paradigms? How am I advantaged by these choices? How are my research results privileged by these choices? What are the ethics hiding behind my choices?

THEME 2: THEORIES OF PROCESS AND TIME

The distinction between variance theories (which use independent variables as necessary and sufficient causes of variation in dependent variables) and process theories (which use events and states to tell a story about how outcomes come about) has intrigued and puzzled organizational theorists for many years, especially since Mohr's (1982) thoughtful analysis of the differences between the underlying logic of the two kinds of theory. We are fortunate to have two well-crafted—and distinct—articles in this special issue that demonstrate how much progress has been made, and can be made, by researchers who develop and write such theories. The authors of these articles are careful to describe the hassles and hazards of trying to develop process theory. But each article also provides useful guidance to help scholars collect, analyze, be inspired by, and write up the often unruly evidence that is required for building good process theories. The first process theory article is "Strategies for Theorizing from Process Data," by Ann Langley, and the second is "Building Process Theory with Narrative: From Description to Explanation," by Brian Pentland.

An emphasis on process implies that time is a crucial element in constructing theory. The effects of using different kinds of intervals on theory, and the ways to use such intervals to strengthen theory, are articulated in a compelling and useful fashion in "Time Scales and Organizational Theory," by Srilata Zaheer, Stuart Albert, and Akbar Zaheer.

These three articles inform dozens of important questions about theory building, but we found two questions to be especially compelling for future theory building and evaluation.

The first question implied by these three articles is: Does a focus on time really change the theories that we develop? In each the authors answer with a resounding "yes," in their own way. Langley answers this question by describing seven different strategies for making sense of process data, and she shows how, as a result, different kinds of theory will be built. She goes beyond Mohr's original formulation to show what process theories are, how they differ from variance theories, what different kinds of data can be used to build process theories, and what different kinds of process theories exist. Langley shows how by choosing between different research strategies, researchers who use process data—often unwittingly—affect how well the theory fits different aspects of the evidence and make tradeoffs between dimensions of "good theory." She shows, for example, how using a visual mapping strategy to portray order of events is well suited for communicating events over time and relationships but less well suited for conveying emotions and interpretations, and she shows it is not necessarily well suited for detecting underlying causal mechanisms.

Pentland answers this question in a more focused way, emphasizing that writing a process theory is much like writing a narrative, because both seek to describe and explain sequences of events. He shows how our theories would change, and could provide better explanations for events, if more concepts for the structural analyses of analysis of narrative (such as focal actors, voice, and evaluative context) were used in a more mindful way by both people who write and evaluate organizational theory. For example, Pentland shows how careful attention to narrative voice—who is telling the story—can lead to more powerful, accurate, and interesting explanations about why events unfold. He shows how the unfolding of a software implementation could be understand more fully because, rather than being told as one story, it was told from the perspective of the three major groups involved.

Zaheer, Albert, and Zaheer show us how, depending on what time interval the theorist or researcher uses, the kind of theory developed (or confirmed) can vary widely. They suggest that the relationships and meaning embedded in theory change over different intervals and that the degree of support for a theory will often depend heavily on how often variables are measured, the time interval over which they are aggregated, and whether time intervals are measured objectively or subjectively. They give examples of theories of trust over the short and long term, and of how support received for a theory about the positive association between accounting earnings and stock market returns is weak over a 1-year period but very strong over a 10-year period. Zaheer et al. also show that the support for theory (e.g., satisfaction with a consumer experience) depends heavily on whether interpretations of events are measured before, during, or after the time period in question. Their perspective is compelling because it shows that, by changing time intervals and orientations, the theories we build, develop, and believe can shift dramatically.

The second question addressed by these papers is: Why do we still act as though explaining variation in dependent variables is the holy grail of organizational theory? These three articles reveal many reasons why it is misguided to act as if the amount of variation a theory (or, more precisely, independent variables derived from it) explains in some dependent variable or variables is the one true test of its validity. This assumption is rarely stated but reflects the behavior of many researchers, reviewers, and editors throughout the social sciences.

Zaheer et al. show how the amount of variance explained may depend heavily on specifying the right time interval, and they imply that we should not quite believe the amount of variance reported in many published studies, because effect sizes often depend on the time interval used. Their arguments imply that, even if the amount variance explained is the right way to test a particular theory, we still might question the effect sizes reported in many articles, because the intervals at which the data were gathered or aggregated to may not be appropriate for the theory tested.

The articles by Langley and Pentland imply the limits of using explained variance in different ways by showing us that developing and finding supporting evidence for theory may often have little to do with variables and variance explained. They imply there is not much use for independent and dependent variables in most process theories, which are about focal actors, voices, actions, choices, and events that are woven into an (empirically based) story about how and why something happens. Langley does show how process data have been used to inform variance theories, but she makes clear that these are different kinds of theory. The general message from these two articles, however, is one that remains difficult for our field to accept: the amount of variance explained by a theory may be irrelevant or deeply misleading, so we should stop treating it as the one, only, and best contest that ultimately determines the validity of our theories.

The best and most complete case we know of for rejecting the variance-explained criterion as our holy grail is in a recent essay by Mark Fichman (1999). He offers numerous compelling arguments why too much emphasis is often placed on the amount of variance explained, including some of the points about choosing the right time interval made by Zaheer et al. in this issue. Fichman shows further that when the underlying causal mechanism in a theory is a stochastic process, the variance-explained criterion makes little sense for testing the theory. He also demonstrates that when variation is irrelevant to the fundamental process, trying to explain variance is an irrelevant, and at times rather silly, distraction. For example, he shows how using variance explained as the primary way of explaining the speed of falling objects will lead a researcher to conclude that he or she "had a full grasp of falling objects without ever evoking gravity" (1999: 305).

As a set, the three articles in this special issue show other important ways that the variance-explained criterion provides incomplete or irrelevant information about the quality of a theory. We hope that these articles, in combination with other work, such as Fichman's essay, will reduce the often-unquestioned belief that, if a theory does not explain a lot of variance or cannot ultimately be tested by using statistical tests that yield such information, it is of little or no value.

THEME 3: NEW APPROACHES FOR BUILDING "THICK" THEORY

The final set of three articles continues this emphasis on nontraditional approaches to theory development. These are "Theorizing As the Thickness of Thin Abstraction," by Robert Folger and Carmelo Turillo; "Replication and Theory Development in Organizational Science: A Critical Realist Perspective," by Eric Tsang and Kai-man Kwan; and "Views from Inside and Outside: Integrating Emic and Etic Insights About Culture and Justice Judgment," by Michael Morris, Kwok Leung, Daniel Ames, and Brian Lickel. These authors suggest that thick theory (i.e., theory that is rich in detail and accurate in description) can be developed using approaches that previously have been categorized as approaches for building simple or generalizable theory—to the exclusion of accuracy and richness (Thorngate, 1976).[1] In particular, these articles address three key questions pertinent to building thick theory.

First, they ask: What are the taken-for-granted approaches for building thick theory? Folger and Turillo suggest that theorists place a premium on concrete experience as a tool for building thick theory. They note that thick, richly detailed case studies allow for comparing of personal experiences to described scenes. Similarly, Tsang and Kwan report that novel experiences, which are described in context by the researcher, are an expected element of thick theory. They argue that popular hermeneutic philosophy considers social science observations to be unique in nature, and, thus, theory building should rely on descriptions of unique experiences as a means of developing theory. In contrast, they suggest that replicability is commonly thought of as a "straightjacket that impedes rather than enhances the advance of social science" (p. 761). Finally, Morris et al. suggest that ethnographic accounts of insiders—that is, "emic" research approaches—are a taken-for-granted route to building thick theory. They argue that organizational theorists have furthered the notion that exploratory research aimed at building theory is served best by emic approaches that put researchers in close proximity to subjects. In contrast, they believe that popular wisdom dictates that "etic" approaches, which commonly rely on data collected across many subject populations, are best suited for testing theory.

A second question is: How might we build thick theory outside of these approaches? Folger and Turillo offer thought experiments—that is, "abstract hypothetical scenarios"—as a means of developing thick theory that flies in the face of conventional wisdom. Based on zero "real" data, thought experiments allow the researcher to focus on the underlying causal mechanisms and processes of a phenomenon that may not behave in a "lawlike" fashion in naturally oc-

[1] Note: The notion of tradeoffs between developing thick or accurate theory, simple theory, and generalizable theory is also echoed in Weick's later discussion of several other articles in this volume.

curring situations. Such mental modeling allows scientists to engage in embodied participation in a hypothetical scenario based on tacit knowledge, as well as direct and indirect experience. This method echoes calls for more "full-cycle" research (Cialdini, 1980) that starts with reflection on personal experiences or media reports as a means of developing theory, proceeds to formal testing of that theory, and then returns to real-world events as means of refining that theory.

In the opposite direction, Tsang and Kwan suggest that replication through "conceptual extension"—that is, repeating a study using the same population of subjects but different procedures (e.g., different ways of measuring constructs, structuring the relationship among constructs, and analyzing data)—and replication through "generalization and extension"—that is, repeating a study using different procedures and a different population of subjects—are under-appreciated methods for developing thick theory through the extensive use of simple, straightforward data. They argue that such straightforward data produce thick theory when replications produce findings inconsistent with the original study. Explanation of these findings often leads to creative insights and richer theory.

Finally, Morris and coauthors take a position between zero data and extensive replication by arguing that theory concerning culture advances through the interplay and integration of insights from emic and etic perspectives. Much like the replication studies described above, integrating etic and emic methods allows researchers to identify context-dependent effects as well as general effects. Explanation of such effects enriches theory by specifying mediating conditions and by identifying new variables of interest.

The third and final question is: How will our theory building be affected by these new approaches? Folger and Turillo argue that thought experiments might change the nature of theory building by focusing attention on identifying and understanding constructs rather than contexts. That is, because thought experiments are not muddied by uncontrollable variables, they allow theorists to think about how phenomena behave in a "pure" sense. Folger and Turillo also argue that because thought experiments allow scientists to theorize beyond accessible data, they emphasize the importance of study design as a means of theory development. They allow theorists to think about ideal experimental designs and how they might test hypotheses. Finally, Folger and Turillo note that thought experiments force scientists to become storytellers, capable of weaving a convincing tale of cause and effect. Such storytelling is a novel but logical means of improving the richness of theory.

Tsang and Kwan suggest that replication will change theory building by encouraging scientists to build a "multifocal" pattern of empirical evidence to support a theory. This is in contrast to the "scattered" pattern of evidence that currently exists in many paradigms. They argue that a multifocal pattern provides a more in-depth understanding of key variables, versus a shallow understanding of all variables. Key breakthroughs that provide richness to theory may only arise from multiple, slightly different examinations of a phenomenon. By dancing around the phenomenon, theorists will better understand the boundaries of variables and processes, thus improving the detail and accuracy of their description.

Finally, Morris et al. argue that integrating insights about cultural influence from emic and etic perspectives uncovers a broader range of constructs, each perspective revealing constructs likely to be missed by the other. In addition, a dual-perspective explanation provides theoretical and practical advantages by highlighting that cultural influences in part reflect unique tendencies contingent on local, historically bound factors and in part reflect instances of more general cause-effect relationships. Similar to Tsang and Kwan's argument above, Morris and his colleagues note that "looking at the same phenomenon from two perspectives adds depth and richness to the explanatory framework" (p. 791).

CONCLUSION

This special topic forum commemorates the 10-year anniversary of a similar venture published by *AMR* in October of 1989. Weick's comments following the nine articles published here characterize this set as rather "tame," compared with what is happening in the rest of the field. We do not quarrel with this observation; it is probably accurate. But Weick's observation caused us to pause—to realize how much has changed since 1989.

Consider a few examples. 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. Thus, Weick's finding these articles rather tame by the standards of 1999, and his finding that their strengths are more in bringing together what were once seen as bold and wild ideas, show how far we have come since 1989 and, perhaps, some of the places we should go next.

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