© *Academy of Management Review*
2007, Vol. 32, No. 4, 1145–1154.

INTRODUCTION TO SPECIAL TOPIC FORUM

THE INTERPLAY BETWEEN THEORY AND METHOD

JOHN VAN MAANEN
Massachusetts Institute of Technology

JESPER B. SØRENSEN
Stanford University

TERENCE R. MITCHELL
University of Washington

This special issue contains six papers that address a variety of practical research process questions. The papers explore how theory and method inevitably interact in particular organization and management studies. Here we offer an overview of how theory and method have been treated to date by organization researchers and suggest that respecting both the primacy of theory and the primacy of evidence is no easy task but a necessary balancing practice that characterizes high-quality research.

The aim of organizational and management research is to speculate, discover, and document, as well as to provisionally order, explain, and predict, (presumably) observable social processes and structures that characterize behavior in and of organizations. In this long march, theory and method surely matter, for they are the tools with which we build both our representations and understandings of organizational life and our reputations. Theory and method, while generating much descriptive, prescriptive, and critical literature, are often treated as conceptually independent. Scholars-in-training take separate courses from separate professors in "Theory" and "Method" much as undergraduates take separate classes in chemistry and history.

With respect to some issues, this separation is sensible. Articles about how to develop and write about theory often emphasize processes and strategies that are relatively independent of methods. Theories are evaluated on such dimensions as internal consistency, logic, organization, clarity, and readability (Klein & Zedeck, 2004). Also, as Whetten (1989) points out, theories are often judged in terms of their novelty, contribution, and contemporary interest. All of these criteria can be used across methods. And methods, too, have their own internal logics. Rules of sampling, observational recording, statistical assumptions, interviewing techniques, and mathematical procedures can be learned independently of theoretical content.

However, it is our stand that theory and method are—or should be—highly interrelated in practice. Theories without methodological implications are likely to be little more than idle

We offer a well-deserved thank you to those who made this Special Topic Forum on the Interplay Between Theory and Method possible. In particular, we appreciate the work of Art Brief, former *AMR* editor, who both hatched and organized this special issue, as well Susan Paulie, former managing editor of *AMR*, for her careful administrative oversight and the guidance and encouragement she gave contributors (and guest editors) throughout this long process. The process was made longer and far more difficult by the unwelcome and tragic visit of Hurricane Katrina to New Orleans and Tulane University, where the *AMR* editorial offices were then housed. Post-Katrina work was picked up at the *AMR* offices at Pace University in New York by Susan Zaid and Annemarie Koory and aided by Gail Feldman, the current managing editor of *AMR* at The Pennsylvania State University. We are grateful to our reviewers as well for the tireless effort and anonymous expertise they brought to this venture. Most critically, however, we honor the thoughtful and scholarly achievements of all those who submitted papers for consideration in this special issue. Although only a few papers were selected for publication, all had merits of various kinds, and we were impressed by both the quality and quantity of this work.

speculation with minimal empirical import. And methods without theoretical substance can be sterile, representing technical sophistication in isolation.

While such observations are commonplace, the relationship between theory and method remains a complicated one and a source of some befuddlement, if not controversy within and across various organizational research communities. Such difficulties are not always acknowledged. To wit, textbook treatments of the theory-method relationship continue to suggest that methods generate meaningful data used to test, in weak form, the plausibility of theories or, in strong form, the validity of theories given modest to severe boundary constraints (e.g., Blalock, 1969; Bryman, 1989; Dubin, 1978; Yin, 2002). As an ideal representation, the interplay of theory and data is not problematic but follows a prescribed—almost magical—sequence. In conventional form, problems are identified that are of interest to a identifiable research community (perhaps more than one), specific research questions or hypotheses are posed that rest on the theoretical resources those in the research community possess (or seek), appropriate research strategies based on either or both deductive or inductive logic are then spelled out, qualitative or quantitative measures are chosen and put to work, data compilation and analysis then follow, and, with pluck and luck, plausible (or verifiable) inferences and conclusions result. End of story.

Reflexivity need not go deep to question this overly simplified and idealized version of the interplay between theory and data. Practicing organizational researchers know both from experience and readily available collegial critique that any narrative suggesting an orderly, standard model of the research process is rather misleading. What seems apparent to those who have carried out organizational research projects is that method can generate and shape theory, just as theory can generate and shape method. There is a back-and-forth character in which concepts, conjectures, and data are in continuous interplay. If one thinks of concepts and conjectures as existing on a conceptual plane and of data residing on an empirical one, the more links and the more varied the links between the two planes, the more promising the research. One function of empirical studies, then, is to generate the kind of data that can be used in the theorizing process itself, thus allowing a study to progress as a cognitive or sense-making venture that unfolds over time (Bailyn, 1977; Weick, 1989; see also Alvesson & Kärreman, this issue).

Flexibility in the connections within and between the conceptual (ideas) and empirical (data) planes and allowing for a logic of discovery rather than only a logic of validation is seemingly a prerequisite if research is seen as a cognitive process. Yet rarely are such matters discussed—at least in print—since the flow of research is lengthy and uneven, is seen most clearly in hindsight, and, perhaps most important, is contextually idiosyncratic, often chaotic, and always personal. How we arrive at conclusions, insightful or otherwise, is difficult to penetrate when publication norms do not favor the presentation of results in the manner in which they evolved and when the personal history of how the research process unfolded over time may be revised or forgotten as the project moves toward its final printed version.

In addition, we are often unaware of (or not encouraged to articulate) our basic epistemological or ontological upbringing and assumptions. Authors who subscribe to logical positivism or empiricism may view things quite differently from those who favor hermeneutic, interpretive approaches or positions guided by postmodernism or critical theory. The logics, schools, and paradigms that populate our research landscape are many and varied, and they have substantial impact on both theory and method.

What this STF brings to the table is a rather broad consideration of just how these fundamental tools of varied research trades—theory and method—interact across a range of conceptual and empirical domains, both narrow and broad. In our call for papers issued in the spring of 2003, we invited papers that would "take up questions as to how method—old and new—helps to develop theory and (or) how theory—old and new—helps to develop method." We illustrated this invitation with a few topical areas where submissions were particularly welcome, including how to think about and refine research constructs and variables, how exploration and development of theory enhance or limit method choices (and the reverse), how formal modeling and/or simulation techniques contribute to theory development, and how inductive or

deductive research strategies constrain or broaden theoretical choice and elaboration.

Theory can drive method through its level of analysis, its stage of articulation, the types of constructs it proposes, and its descriptive or prescriptive nature. For example, theory can inform research design (Edmondson & McManus, this issue), choice of measures (Harrison & Klein, this issue), and samples (Kalnins, this issue). Method can help to develop and enhance theory development through the analysis of configurations (Fiss, this issue), simulation modeling (Harrison, Lin, Carroll, & Carley, this issue), and analysis of surprising or unexpected data to generate new theoretical insights (Alvesson & Kärreman, this issue).

The extent to which the relationship between theory and method inspires reflection if not controversy is reflected in the fact that our call was answered with almost fifty papers, of varying quality and purpose but all concerned in some way or another with the interplay of theory and method. In reading through these papers (and beyond), it seems clear to us that the tensions many scholars file under the heading "the relationship between theory and method" in many respects derive from the difficulties found in balancing between the conceptual and empirical planes. In other words, all researchers struggle with deciding when they should be true to their theory and when they should be true to their data. All of the papers in this STF wrestle with this tension and emphasize, in different and thoughtful ways, how this tension manifests itself and can be addressed in concrete research settings.

Theorizing is how we think about the relationships among the elements in the world that occupy our research attention. Yet the social world is complex and full of random noise that may obscure the processes we are interested in (Leifer, 1992). The methods we use to perceive the social world are imperfect as well. If we pay too much attention to available or potentially available data, we are trapped by operations, and theorizing is stifled. If we pay no attention to data, our theorizing will be rather too remote and will occur all on the conceptual plane. In either case, the potential interplay between method and theory is limited. The key, then, is to find a way to serve two masters at once. Research methods play a central role here because they must be designed so that they sufficiently respect both the primacy of theory and the primacy of evidence.

THE PRIMACY OF THEORY

Theory is not well defined or understood, nor is it one-dimensional (e.g., see Sutton & Staw, 1995, and the range of comments regarding their stand on "what theory is not"). As Weick (1995) points out, a theory, in principle, could be a guess, conjecture, speculation, supposition, proposition, hypothesis, conception, or model, with those at the formal end of the spectrum more likely to be in print. But, even in print, what is conventionally treated as theory displays high variation in terms of range, focus, interest, complexity, sweep, elegance, level of analysis, presentational character, implications for next steps in the collective research process, and so forth.

DiMaggio (1995), building on D'Andrade (1986), notes that some theories seek to provide covering laws that govern the relationships among variables or constructs in a highly specified field of study and use their methods as ways of possibly verifying such covering laws. Some seek to provide novel explanations or provoke a sort of unexpected enlightenment among knowledgeable readers by putting forth new and potentially useful constructs or uncovering previously unsuspected relationships existing with some consistency across a studied domain. Methods in the service of such a goal defamiliarize if not deconstruct the existing conceptual landscape, bring new classification and category schemes to the fore, and question conventional domain assumptions. Other theories seek to provide step-by-step accounts of particular social processes and place their methods in the service of showing the plausibility and presumed strength of the various linkages involved in the constructed analytic narrative.

In general, regardless of theoretical aims, many researchers continue to suggest that the more we underpin our theories with empirical observations that more or less fit the theory, the more convincing such a theory is. Disconfirmability has long reigned as the number one criterion by which theories should be judged. In this respect, empirical validation (or lack of same) is held to trump the host of other persuasive tactics used to improve a theory's reception—its memorable character, its parsimony,

its complementary or stand-alone position among other theories, its interest value to a particular research community, its rhetorical style, and so on (e.g., see Edmondson, 1984; Lindblom, 1987; Van Maanen, 1995).

This rather questionable assumption privileges data over theory and rests on a rather naive image of data as unaffected by the research methods employed. It neglects the importance in scientific progress (such as the development of a theory of relativity) of "denying the data" (Leifer, 1992) and clinging to theories until suitable methods of observation can be found. What Leifer says of sociology can be said of organizational research as well: "Virtually all of what actually happens is accepted as data and allowed to render theories false. This combination of a passive orientation and Popper's falsifiability criterion has disastrous consequences" (1992: 286). In particular, it may well limit theorizing, since it relegates hunches, intuition, wonder, imagination, speculation, inspiration, and the like (all closer to the guessing end of Weick's theory continuum) to late-night sessions in the tavern and forces researchers to worry more about the close correspondence of data and concepts than the nature of the theory being built. The focus is more on theory as a product of methodological savvy than a result of a mental or cognitive process.

By stretching our understanding of how theory and method interact, as well as our prescriptions for research as theory validation and data as our ultimate jury, we of course create other problems. Organization studies, as we have suggested, is an expanding, multidisciplinary field. We have built a large and somewhat porous tent within which a variety of methods, theories, and epistemological schools of thought coexist (and, perhaps, coevolve). Judging this as a virtue or vice depends, in part, on how deeply committed one is to a particular research style and, in part, on how one views "progress" in the field. Since resources are always limited, choices must be made as to which (and how) lines of research will be extended or cut back. The grounds for such choices are obscure, but surely one of them concerns the empirical verification a particular theory-building program generates. Since research is not a democracy, nor should it be (Levett & Gross, 1994), the problem is how to encourage and facilitate new, rich, and interesting theory while sustaining standards of excellence, competence, and contribution.

There are, alas, no terribly satisfying answers to the dilemmas posed above. Theorizing always entails trade-offs and compromises between such matters as simplicity and complexity, originality and semblance, and specificity and generality. Attempts to verify or validate will and usually should be tried, of course, and are useful for theorizing since they represent an asserted link between the conceptual and empirical planes. But if the correspondence between concepts and data is loose in organizational research and the worlds we study are open rather than closed, the value of a theory cannot be reduced to its claimed verification (Gergen, 1976; Knorr-Cetina & Cicourel, 1981; Weick, 1989). Hence, the point of theorizing, when viewed as a cognitive process, is not simply to produce validated knowledge but, rather, to suggest plausible connections and relationships that have not yet been glimpsed. In this sense, building interesting theory is then the name of the game, and, for our field in this moment of time, we believe such an emphasis is appropriate.

THE PRIMACY OF EVIDENCE

The counterpoint to this argument can be seen by asking a simple question: Where do interesting theories arise, and how? How do they emerge from methods designed to order and tame empirical observations, be they constructed and compiled as survey materials, ethnographic fieldnotes, laboratory trials, secondary data sets, or simulation results? How do we sort out helpful theoretical leads from not so helpful? Good theory is difficult to produce, and, unlike pornography, we may not even recognize it when we see it. Moreover, good theory seems likely to be more the result of many thought trails, modest speculations, and approximations than a bold stroke or "ah-ha" epiphany occurring at blinklike speed. Moving back and forth from data-based theorizing to intuition resting on experience, habits of mind, and research context plays an important role in generating interesting theory, as does absorbing what one can of the scholarly literature in the field and working through conjectures without being tethered to data.

Perhaps the best answer we currently have to the problems of discovery was provided by Charles Sanders Peirce (1995/1903), who argued that discovery rests primarily on abductive reasoning. As a foundation for inquiry, abduction begins with an unmet expectation and works backward to invent a plausible world or a theory that would make the surprise meaningful. As Kilduff (2006) recently advocated, good theory comes from engagement with problems in the world, not gaps in the literature. In this sense, abduction assigns primacy to the empirical world, but in the service of theorizing. Like the dog that did not bark in the fictional world of Sherlock Holmes, unmet expectations are clues that motivate theorizing, and, precisely for this reason, they are to be welcomed and embraced by researchers.

Note, too, the interplay of observational and conceptual work in abduction. From the fragments of what we glimpse of the empirical world that can be observed come theories that cannot be observed—at least directly. Peirce explained abduction as less a logic than a path of critical reasoning in which conjectures follow surprises. Discrepancies uncovered by unmet expectations can be analyzed in terms of their location (where did it happen), timing (when did it happen), frequency (how often does this happen), and magnitude (how important is this discrepancy). Some conjectures may account for the surprise better than others, and, thus, they push inquiry forward, leading over time to more surprises and conjectures. Deduction and induction follow and complement abduction as logics more suitable for the always imperfect testing of plausible theories (e.g., see Ketner, 1995).

It is important to note, however, that abduction is a continuous process, taking place in all phases of the research process. Analysis proceeds by the continuous interplay between concepts and data. Surprises can occur at the beginning, middle, or end of a research process. First and second drafts, for example, may be more valuable for generating unmet expectations and bringing to light unseen puzzles than for tidying up, presenting, and defending plausible theory and its empirical support. Coding and classifying may transform data used for one purpose to another in ways that guide and reflect the evolving concepts of the analyst. Interesting theory in Davis's (1971) now classic view turns on transformations, such as what was once thought to be a dependent variable turns out to be—at least for the moment—an independent one, or what was once thought to be a homogeneous construct turns out to be—for now—a rather heterogeneous one. What makes for interesting scholarly work is the discontinuity between some (but not all) of the theoretical assumptions of the researcher and the research audience and some (but not all) of the discovered and claimed facts of the matter.

The implications for method given the role abduction appears to play in research are numerous, but three stand out. First, the data researchers have to work with should be sufficiently detailed, rich, and complex such that the organizing processes and causal conjectures can be approached and explained as to why they appear plausible. Second, by generating explanations for their findings, researchers are forced to link their results to the conceptual plane and, by so doing, can then move back again to try to substantiate these post hoc interpretations by conjuring up consequences for them (i.e., more theory) and checking them out against the available empirical evidence they have in hand. This is the long march. Third, what Bailyn (1977) calls a "principle of opposites" appears to be helpful in this process. The principle suggests that if you have qualitative data, count and classify what you can; if you have quantitative data, don't ignore its qualitative potential by not examining the extremes or not looking at data that do not fit the general picture.

It is an irony of scholarly practice in organizational research that the process of abduction, which likely goes on in most if not all promising research projects, is largely hidden from view. As mentioned at the outset, the publication conventions currently attached to research journals such as *Administrative Science Quarterly, Academy of Management Journal,* and *Organization Science* hide the discovery process by requiring a rather strict separation between the presentation of results and conclusions and between the presentation of theory and method. This process is often messy, idiosyncratic, and difficult to articulate. There are, no doubt, reasons for these journal policies, along with many disagreements over whether the field is well served overall by such conventions. It is nonetheless clear that one negative consequence is that those wishing to learn the craft of research are largely

left in the dark if they try to follow how the results of a study were sequentially interpreted and how these interpretations were checked out (if at all) by the data in hand. The research process, when put into print, makes it appear as a validation exercise in which imagination, false speculation (from which springs abductive reasoning), and faith-based assumptions about being able to sort out useful conjectures from the not-so-useful play no role.

This is, of course, an old complaint and now something of an institutionalized one, but we seem not to have moved toward much of a resolution, beyond that of an infrequently published confession as to how one's research "actually" unfolded (e.g., Barley, 1990; Frost & Stablin, 1992; Hammond, 1967; Stablin & Frost, 2004). A difficulty with this rather spare literature, which features a good deal of recounted abduction, is that research confessionals are interesting apparently only insofar as they index well-received work, making it appear as if discovery and creativity in research are rarified and out of reach for most of us. Rather, discovery and creativity growing out of assigning primacy to empirical puzzles should be seen as general social and cognitive processes potentially available to all.

THE PROBLEM OF READER RESPONSE

We have thus far argued that the interplay between theory and method demands of the researcher the ability to serve two masters simultaneously—to be true both to the power and elegance of ideas and to the demands of empirical reality. As if this does not pose enough of a challenge, successful research also demands a recognition that the interplay between theory and method occurs within a context defined by the consumers of organizational research. What do audiences desire and expect of organizational and management research? How does this shape the character of the research that is performed? Clearly, as noted previously, some of the demands of the audience derive from broader topics and world views that shape the acceptance of theoretical perspectives and methodological approaches. The popularity of agency theories and the econometrics that surround them is a good example in this regard (e.g., see Ferraro, Pfeffer, & Sutton, 2005; Ghoshal, 2005; Kanter, 2005). That this corresponds to what seemed so recently to be the ultimate triumph of shareholder capitalism is certainly no accident.

In addition, research audiences read swiftly, and they read only a few papers carefully. Those papers attended to most closely are quite likely to reflect preexisting interests and to draw on the knowledge of readers who can best scrutinize and judge the claims put forth. When a paper is widely read, the audience grows more general, and the study's reception will be shaped by perspectives and concerns rather different from those of close, attentive, specialized readers. Simplification results, attentiveness to the theoretical arguments or empirical materials fades, and conclusions are more likely to be taken at face value if they reflect the current cultural context of the times and that of the reading audience.

This is to say that theories are created by readers too. Even when a paper is cleverly theorized, carefully illustrated, and empirically supported, that paper's reception will be influenced by matters far beyond the author's control. Studies in the sociology of science demonstrate the importance of a theory's reception among readers and how authors and readers influence one another both directly and indirectly (e.g., Ashmore, 1989; Collins & Pinch, 1982; Law, 2004; Merton, 1973). General readers, it seems, look most for enlightenment and attend little, if at all, to the largely unspecified historical context of the theory put forth or of the methods involved. Collegial readers whose work might complement that of the author will attend more carefully to the work but only insofar as it provides grist for their own research mills. Specialized readers who participate in the research domain targeted by the author will no doubt read most carefully (and skeptically), since their own theories and methods may be put to test by the author.

Such matters are hardly startling, but they do suggest why highly specialized scientific journals seem so unfriendly to the uninitiated and so full of congealed sentences, arcane neologisms, and mind-numbing qualifications. Yet few papers have long lives. Those that do are usually those that have somehow escaped their intended specialized audiences and have traveled by virtue of their resonance to the presuppositions of readers not well grounded in the subject matter addressed by the author.

There is, of course, little we can do about this as we go about putting our research into print. It is a bit paradoxical as well. Indeed, others making use of our work for purposes we did not anticipate or intend is something we must simply learn to live with. If we are lucky, we might, in fact, learn something about our methodological and theoretical choices from the reactions of those readers who carry quite different interests and perspectives from our own but nonetheless have been taken by some aspect of our work.

The issue here is that both theory and method not only are constructed and represented by an author but are reconstructed and rerepresented after they are written. Thus, our writings can—presumably when the conjunction of the planets is just so—take on a life of their own. DiMaggio (1995) points out how even half-baked theories can turn out better or worse depending on who takes them up at a later stage. If one is fortunate enough to say something that attracts the favorable attention of Karl Weick, Jim March, or Dick Scott (or, gulp, Malcolm Gladwell), their work (and standing in the field) is likely to fair well. To underestimate the value of postpublication theory construction and method assessment is to deny just how much research is, in fact, a collaborative and cooperative matter occurring between authors and readers. In this sense, the interplay of theory and method is a collective process that extends well beyond any single research project.

PAPERS IN THE SPECIAL TOPIC FORUM

With these considerations in mind, we now quickly introduce the six papers contained in this special issue. Each pushes our thinking about the interplay between theory and method forward in important ways. A summary of each paper is presented in Table 1. We should note that the papers cut across professional circles and interest groups of the Academy of Management and offer something of value across various levels of analysis, contrasting theoretical interests, differing methodological predilections, and distinct substantive domains. In some papers new methods are proposed to deal with old and familiar problems. In other papers new theoretical approaches are suggested that could extend an existing research line in promising ways. All mix an interest in both theory and method and manage to open up the back-and-forth features of the research process that are too often kept out of sight.

The first paper is the broadest in reach of our set. Amy Edmondson and Stacy McManus take up the largely implicit maxim that high-quality field research in organization studies is produced when there is an appropriate fit between theory and method. They point out, however, that little explicit attention has been given to just what might represent a reasonable fit. They then introduce a useful contingency framework connecting prior work in a research area to the design of a new research project, paying particular attention to when and why qualitative or qualitative data (or both) might be sought.

Peer Fiss follows this concern for fit with a paper suggesting that much of the research on organizational configuration is limited by a mismatch between theory and method. To overcome this knotty problem, he offers set-theoretic methods that permit the algebraic manipulation of proposed attributes that might constitute a particular configuration as a methodological alternative to conventional correlational or variable approaches, and he argues that such methods allow for the investigation of equifinality and limited diversity. Such methods are open to both categorical and ordinal data and provide a way to direct verbal statements into logical categories that, when properly analyzed, can tease out complex cause-and-effect relationships and can generate new insights for organizational theory and strategy researchers.

The third paper, by David Harrison and Katherine Klein, takes up management research concerned with diversity. The authors begin by noting that reviews of the diversity literature reveal few clear or consistent findings and ask why. While some might conclude from the weak evidence that diversity should be abandoned as a theoretical construct, these authors argue instead that diversity is too general a construct. Theory development, thus, is retarded and confused by mismatched operationalizations and research designs. Harrison and Klein usefully distinguish among three distinctive types of difference-based subconstructs of diversity—separation, variety, and disparity. They then develop guidelines for building more precise conceptualizations of diversity type, measurement techniques, and theory tests.

Simulation modeling is the topic Richard Harrison, Zhiang Lin, Glenn Carroll, and Kathleen

TABLE 1
Summary of Articles in the Special Topic Forum on the Interplay Between Theory and Method

Authors	Paper Title	Core Questions Addressed	Purpose of Paper	Emphasis on Theory or Method
Edmondson & McManus	"Methodological Fit in Management Field Research"	What does "methodological fit" mean, and how could it be applied to a research project?	To provide some decision rules to guide the methodological choices made by researchers	Roughly equal emphasis
Fiss	"A Set-Theoretic Approach to Organizational Configurations"	How do we alter the mismatch between theory and method in research on organizational configuration?	To introduce a new method of studying organizational configuration	Mostly method
Harrison & Klein	"What's the Difference? Diversity Constructs As Separation, Variety, or Disparity in Organizations"	Why have so few consistent findings emerged in research on diversity in organizations?	To develop guidelines for diversity research that recognize the varied forms of diversity	Mostly theory (construct refinement)
Harrison, Lin, Carroll, & Carley	"Simulation Modeling in Organizational and Management Research"	Where can simulation modeling contribute most effectively to organizational and management research?	To provide a broad description of the advantages (and special problem) of computational modeling in organizational studies	Roughly equal emphasis
Kalnins	"Sample Selection and Theory Development: Implications of Firms' Varying Abilities to Appropriately Select New Ventures"	How does sample selection influence theory development?	To reduce misinterpretation and inappropriate theoretical conclusions in studies where sampling is critical	Mostly theory
Alvesson & Kärreman	"Constructing Mystery: Empirical Matters in Theory Development"	How do we open up established theory to develop novel theorizing?	To show how "data" can be used to problematize established theoretical understandings and move toward new knowledge	Mostly method

Carley examine in the fourth paper. The point of departure here is the altogether plausible contention that management and organization theory is becoming increasingly complex and that simulation modeling offers a methodology to advance theory on complex systems. In this sense theory development is hampered by limitations in our ability to observe complex social processes in the empirical world. The authors' aim is to promote a broader understanding of simulation modeling and to illustrate the potential contributions it might well make in several developing research fields. In so doing they pay special attention to the attractions and special epistemological problems that are attached to computational modeling and, along the way, point to a number of recent examples of its use in organizational research.

In the fifth paper in the STF, Arturs Kalnins examines the role sample selection plays in developing theory. He points out—by way of examples chosen from firm-level decisions to invest in new ventures or products—that the sample observed is always the result of some selection process the researcher may or may not be fully aware of. More to the point, the sam-

pling process itself may generate empirical findings consistent with a theoretical explanation that may, in fact, play no causal role, or it may cancel out empirical relationships that are consistent with a causal process proposed by a theory. Rather than construing the sample selection issue as a narrow methodological question involving the accuracy of parameter estimates, Kalnins shows how empirical data can be an unreliable guide for both theory validation and theory generation.

The last paper, by Mats Alvesson and Dan Kärreman, represents a challenging call for researchers across the theory and method spectrum to actively seek and create surprise and mystery in their work as a way of opening up established theory. These authors are also the most explicit about how abduction might be furthered if researchers systematically searched for deviations in what is to be expected in particular empirical contexts given the available theory. They argue that coming up with new ideas and theoretical leads is less an inductive matter (as some grounded theorists might have it) and more a matter of rethinking established theory by specifically looking to cases where the theory will not hold. Data are then more useful as a way to move between conceptual and empirical planes than as a resource to be trotted out solely for verification purposes.

SOME FINAL WORDS

The papers that follow provide a number of theoretical and methodological suggestions across a wide variety of management and organizational fields (and subfields) and so deepen our understanding of the research process viewed broadly. There are both general and specific implications for students of organizational strategy, design, culture, performance, growth, complexity, and learning, as well as some sharp insights into how empirically grounded middle-range theorizing can be improved. The papers could be slotted into substantive fields as disparate as firm-level decision making and gender studies. Temporally, the papers collectively consider theory from conception to growth to maturity to challenge and decline or reinvigoration through evidentiary-based doubt and discovery.

This said, there is still a good deal to ponder. What constitutes good, useful, or worthy theory in our field remains up in the air and cannot be resolved through empirical validation alone. Discovery may rely on abduction, but the forms abductive reasoning can take are not well understood, and we have too few cases of success and failure in hand to warrant much more than blind guesses as to its paths. And while we know readers surely add to the theorizing process, how, precisely, they do so and in what fashion are far from clear. Much remains to be done if the organizational research process is to be further deconstructed in order to be reconstructed along improved lines. The upside to all this is that while our theories, methods, facts, assumptions, and topical interests may all change over time, the field itself will remain, and work will go on much as before as long as some audience continues to look toward us to provide representations and explanations of organizational life. This is unlikely to change.

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John Van Maanen (jvm@mit.edu) is the Erwin Shell Professor of Organization Studies in the Sloan School at the Massachusetts Institute of Technology. He received his Ph.D. from the University of California, Irvine. His research interests include occupational sociology, organizational careers and cultures, and, currently, identity in work settings.

Jesper B. Sørenson (sorensen@stanford.edu) is associate professor of organizational behavior in the Graduate School of Business and (by courtesy) in the Department of Sociology, Stanford University. He received his Ph.D. from Stanford. His research interests lie at the intersection of organizations and labor markets, with a recent focus on the relationship between corporate demography and income inequality, as well as the impact of work environments on entrepreneurial activities and outcomes.

Terence R. Mitchell (trm@u.washington.edu) is the Edward Carlson Professor of Business Administration at the University of Washington. He received his Ph.D. from the University of Illinois. His current research interests are motivation, leadership, decision making, and turnover, with a focus on individual behavior in organizations.