COMMENT:
Modeling Structures of Organizations

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Theory building has a long and, often, noble tradition in the field of organizations. Whereas publishing (and career survival) depend largely on empirical work, when we think about the most prominent people in our field, such as Karl Weick, Jim March, or Jeff Pfeffer and Gerry Salancik, we think of their theories, not their *t* tests. The publication of the *Academy of Management Review* in 1976 legitimized this role for the rest of us mortals by providing a serious outlet for theory building.

The primary problem with theory building as an enterprise is that quality control is difficult. Consensus on what separates high-quality theory from mediocre theory is hard to come by. The "good theory" seems to have fallen into the same category as the famous Supreme Court dictum on pornography: I may not be able to define it, but I know it when I see it. With the tastes for different theories as numerous as the number of organizational scholars themselves, even the Supreme Court's guideline is not particularly useful.

To discuss the quality of a theory, at a minimum we must start with some criteria that we are willing to apply generally. In this regard, I have found two prominent scholars' comments to be useful: Weick's (1979) general–accurate–simple (GAS) model and Dubin's (1978) work on theory building itself. Weick argued that theories have three desirable qualities:

1. They should be general; that is, they should apply to a wide range of social phenomena or number of contexts.

2. They should be accurate; that is, their predictions about relationships among variables and outcomes should be borne up under empirical scrutiny.

3. They should be simple or parsimonious in their explanation.

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He further articulated why it is almost impossible to do all three simultaneously, postulating that theories will tend to accomplish two of the three goals, at the expense of the third. For example, case studies often provide us with a theory of what happened in a particular firm, yielding an accurate and relatively simple explanation but one that does not easily generalize outside the boundaries of the specific organization being studied. Armchair theorizing often yields elegantly simple and general explanations of phenomena but often does not predict accurately. Finally, Weick noted that sophisticated computer models, such as those of the weather, can yield highly accurate predictions and can be applied to a wide set of conditions, but their structure is often impenetrable to the human mind. They therefore do not provide us with nice, simple explanations of phenomena.

Weick (1979) did not pontificate about which combination is supreme; instead, he argued, all three types of theories have their place. It is instructive, however, that relatively few theories in organizational theory can be categorized as sacrificing simplicity for the sake of accuracy and generality.

Dubin (1978), in contrast, places more emphasis on the building blocks of theorizing. In his award-winning book, he argued that developing theory is a logical process. The guts of a theory—the propositions and their attendant hypotheses—are derived, not made up, from a set of clearly articulated premises. He goes into great detail about where these premises come from, because they drive the theory-building process. The four necessary building blocks are as follows:

1. "units," or variables (concepts, objects) that can take on particular values
2. "laws of interaction," or a set of statements describing how the units relate to one another
3. specifications on the "boundary conditions" within which the laws of interaction will apply
4. a description of the various "system states" the model can go through (Dubin argued that this last point is important in specifying how systems change).

Dubin readily acknowledged that some theories are better than others, but his primary concern is that many claims pass as theory when, in fact, they do not meet even the minimum requirements to be a theory. That is, they have not been logically thought through sufficiently to qualify as serious theory.

If we combine Weick's (1979) and Dubin's (1978) perspectives, we see that we are faced with a formidable task. To create theories that are both complex enough to deal with the real world and accurate enough to be useful and interesting (thereby, as Weick suggested, necessarily sacrificing some of their parsimony), we face the even more difficult task of meeting Dubin's reasonable but demanding requirements of working through the logical implications of the theoretical premises.

Precedent exists for this formidable task, however. For more than 40 years, the systems dynamics group at MIT has been producing computer models that show

how organizational systems behave in complex ways (see George Richardson's [1996] two-volume review of the field). The systems dynamics group, however, especially under the auspices of its founder, Jay Forrester, has applied its craft not to the development of theory but rather to solving organizational problems directly, such as how to create a policy that will control corporate inventory levels or reduce urban blight. Nonetheless, the systems dynamics group has taught us two important lessons about systems in general (Senge, 1990). First, systems—even the simplest feedback systems—are humanly impossible to grasp in their behavior. Thus, it is impossible to produce an armchair theory of a system that will be even close to accurate and general. Second, the structure of a system, especially the location of the feedback loops and delays, has a far greater impact on the behavior of the system as a whole than do the levels and flows within the system. These are two general principles of systems theory that seem to apply to many cases, and they argue for careful modeling rather than simple generalizations.

This is where Carley's work (and others in the world of computational modeling) comes in. By simulating on a computer, she goes beyond the problem-solving focus of systems dynamics to build generalized theories of organizations. Moreover, she addresses the shortcomings in traditional theory development. Through the discipline of computational models, we are forced to explicate carefully the premises and microassumptions underlying our theory. We allow untold complexities to emerge and play out. We sacrifice simplicity (in that the resulting theoretical predictions are not intuitive and linear) for the benefit of logical accuracy (if not empirical accuracy) and generality. That is, computational theorizing is a step forward in completing the circle of theoretical development in the field of organizations.

Despite this obvious advantage, the field of organizations appears generally resistant to computational theorizing (e.g., Starbuck, 1976). For one thing, there seems to be a confusion in the field about the role the computer plays in this process. The computer does not test theory; it generates theory. It demonstrates the immutable logical consequences of a set of premises. From Dubin's (1978) perspective, there is no better theory than that.

But even in cases in which the computational models have successfully generated popular theory, the computer's role has been systematically ignored. For example, if we were to compile a "Top 10" list of theories of the past 30 years, the garbage can model of decision making would most likely make the list. Although it is a popular model of decision making that is included in many organizational theory textbooks (e.g., Daft, 1989), it is surprising how few scholars acknowledge that the theory itself was born from a computer program. A cursory reading of the original Cohen, March, and Olsen (1972) piece in *Administrative Science Quarterly* immediately reveals that the theory is based on a computer simulation of decision making. The authors even take up six pages (pp. 19–24) to include the FORTRAN source code of the computer program that produced their theoretical claims. This origin is well known among computational modelers, but textbook writers who summarize this

work often leave out the fact that the model is computer-based (Daft, 1978, for example, has four pages of discussion of the model and no mention of a computer simulation as its origin). My informal observations are that most organizational scholars who are familiar with the garbage can model are not aware of the fact that it is a theory derived from computer simulations. By systematically ignoring the role the computer has played in the development of this theory, the field has unwittingly expunged the computer's rightful place in the history of this part of organizational theory.

Another barrier to acceptance of computational theorizing is the use of language. For example, Carley (as do others) uses the term *experiment* to describe the combination of premises and assumptions coded in the computer runs; *results* are observed as the computer spits out derivations. These terms also are used by noncomputational researchers, but the terms carry different meanings. The difference is not simply that one is performed with artificial data and the other with "real" data. Rather, the purposes of the two are different. An "experiment" to a social psychologist is an attempt to test a theoretical prediction, whereas an "experiment" in a computational model creates a theoretical prediction. The "results" of an experiment in social psychology allow a conclusion to be drawn about whether the theory was confirmed or disconfirmed. The "results" of an experiment in a computational model are the theory itself and its boundary conditions. A social psychologist (or other social scientist) is likely to dismiss computational modeling out of hand, arguing that one cannot do "experiments" and come up with "results" with any real meaning in the artificial world of the computer. And they would be right, if by "experiments" they meant the theory-testing variety. If computational theorists were to use different terms for these modeling components (Carley's occasional use of the term *virtual experiment* is a step in the right direction here), they might find less confusion and less resistance to their contributions.

Similarly, the way the experiments are talked about leads the reader to think of these as testing mechanisms rather than theory-generating mechanisms. For example, Carley states that density is expected to impact performance because the higher the density the higher the management work load but the greater the communication and so potential for noticing errors. A naive reader might interpret that as a hypothesis about to be tested in the computer runs. Also, when talking about the "results," Carley found that in a stable environment, despite being able to change, organizations get locked into competency traps not in what they do, but in how they do it. Again, this could easily be misinterpreted to mean that she found evidence that organizations actually do "get locked into competency traps," when, of course, what she has found is that under the conditions specified by the model, she now can make a reasonable theoretical prediction that organizations will find themselves so locked or incapable of adapting.

In closing, I would like to take the liberty of comparing computational theorizing with another popular brand of theorizing done by economists. Economic theorists are systems thinkers—witness their mantra "in equilibrium" or "an equilibrium solution." They understand the importance of feedback and dynamics. Yet the modal economic theorist in *Econometrica* insists on limiting herself to mathematically tractable statements. A set of assumptions is laid out, and mathematical theorems are derived. The problem with this approach to theorizing is that it is artificially limited in scope. It is not simply that the real world is more complex than that; the real world is more complex than any model or theory. The problem is that the theory itself is prevented from addressing interesting nonlinear dynamics, the kinds of dynamics that characterize and determine the behavior of virtually all systems. Thus, although economists are good theorizers (they clearly spell out premises and logically derive conclusions), they could be better theorists if they removed the shackles of pencil-and-paper mathematics and followed the lead of Carley and others into the realm of computational theory.

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