

Comments on "What Theory is Not"

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Sutton and Staw display an unerring eye for the dodges we authors use as substitutes for theory. But these ruses are easier to acknowledge than to do without. Why is this the case? Sutton and Staw suggest that the problem lies in a combination of education (social science faculty don't train our students adequately in theory construction) and talent (not enough of us have that ineffable something that makes a good theorist). Without denying the importance of these factors, especially the first, I would suggest that three additional issues render the problem of theory even more complicated than Sutton and Staw suggest.

1. There is More Than One Kind of Good Theory

There are at least three views of what theory should be, and each of them has some validity. Each of them also has limitations.

Theory as covering laws. A familiar position, which Sutton and Staw implicitly reject, is that theories should consist of covering laws: generalizations that, taken together, describe the world as we see (or measure) it. The fact that most social scientists to some extent embrace this approach renders Sutton and Staw's argument more radical than it sounds, for they reject some key tenets of behavioral science as it is usually practiced: a focus on explaining variance rather than regularities; the view of scientific progress as a kind of R^2 sweepstakes; and the image of a world in which variables explain one another—all parts of the perspective that Abbott (1988) has derided as "ordinary linear reality." At the limit—a limit reached by economists who admit that they don't care if their assumptions are implausible, so long as their R^2 s are high (Friedman, 1953)—this view provides the "what" of theory that Sutton and Staw argue is insufficient unless accompanied by the how and why.

Theory as enlightenment. A second view of theory, especially prominent in those neighborhoods of the social sciences influenced by the humanities, is as a device of sudden enlightenment. From this perspective theory is complex, defamiliarizing, rich in paradox. Theorists enlighten not through conceptual clarity (a postmodernist once told me that to define what she meant by "postmodernism" would be unfaithful to the theory), but, like R. Crumb's Zen master Mr. Natural, by startling the reader into satori. The point of theory, in this view, is not to generalize, because many generalizations are widely known and rather dull. Instead, theory is a "surprise machine" (Gouldner's, 1970, unflattering assessment of Parsons' system), a set of categories and domain assumptions aimed at clearing away conventional notions to make room for artful and exciting insights.

Theory as narrative. A third perspective on theory emphasizes narrativity: theory as an account of a social process, with emphasis on empirical tests of the plausibility of the narrative as well as careful attention to the scope conditions of the account. The minimalist version of this approach (Collins, 1981, on "micro-translation") simply requires that hypotheses detailing regularities in relations among variables be accompanied by plausible accounts of

how the actions of real humans could produce the associations predicted and observed. More assertive versions argue that theory begins with baseline generator models—formal models of human behavior that specify principles of individual or group action that through computer simulation generate observed distributions of outcomes (Fararo, 1989).

Sutton and Staw clearly have in mind a temperate version of the narrative approach when they speak of theory. Although I share their bias, I think we must also make room for the other versions, as well as for hybrids (e.g., Cohen, March, and Olsen, 1972, which combined narrative modelling with Zen-like paradox). And if we admit other approaches, we must tolerate their limitations. As Sutton and Staw point out, for example, variance theories can inch toward process theories, as when they treat sequences as dependent variables or specify scope conditions in ways that call attention to interaction effects that illuminate process. But, ultimately, from the covering-law perspective, the point of theory is to explain things, and explanation means accounting for variance: In this view, the distance between hypothesis and theory is vanishingly small, and if you need a lot of hypotheses to explain a lot of variance, then so be it. Similarly, because enlightenment theories are often intuitive, they may employ references or diagrams or graphic presentations of data as rhetorical devices to elicit epiphanies.

2. Good Theory Splits the Difference

One can go beyond simply recognizing the diversity of useful and plausible approaches to suggest that many of the best theories are hybrids, combining the best qualities of covering-law, enlightenment, and process approaches. One reason that theory construction is so difficult to teach is that these approaches, as we have seen, are driven by different purposes and embody different values. Consequently, the researcher who tries to combine them faces not a list of brightline standards, but a set of vexing choices.

Clarity vs. defamiliarization. By defamiliarization, I refer to the process of enabling a native—of a society, an organization, or an academic discipline—to see his or her world with new eyes. Arguably, good theory should accomplish this. But it must not go too far. The conventional justification for neologisms is that the old words carry too much baggage to convey new ideas or perspectives. At the same time, too many neologisms render a theory too strange for people to grasp. Similarly, it is often necessary to frame a theory in paradoxical terms in order to get readers to pay attention. Arguably, all good theory has a germ of paradox. Some of the best theory has no more than a germ: Hannan and Freeman's (1977) original paper on ecology was powerful because it effected a tiny but crucial shift in the reader's focus from change in surviving organizations to patterns of birth and death. Other exemplary theories—e.g., Cohen, March, and Olsen's (1972) exposition of the garbage-can model—are awash in paradox. Too much paradox, however, and an interesting new theory begins to sound preposterous. The trick is in the balance.

Focus vs. multidimensionality. Most graduate programs highlight the importance of focus in theory: Take a strong position or a new model and push it as far as it will go. Sutton and Staw endorse this when they argue that hypotheses, especially disparate hypotheses drawn from different theoretical traditions, do not constitute theory. Graduate programs also highlight exegesis and teach students to pick apart a paper or study for the factors or variables it omits. Some theorists even view "multidimensionality"—the extent to which a theory includes reference to agency, culture, structure, and several other abstract categories in its rhetoric—as a decisive criterion of its adequacy (Alexander, 1982). Alas, one person's multidimensionality is another's goulash; one author's focus, another's crude reductionism. Again, I side with Sutton and Staw in their general orientation toward what one might call "strategic reduction": abstracting away enough of the world's confusion to develop pointed explanations of organizational phenomena. But where one draws the line is still more art than science.

Comprehensiveness vs. memorability. Theories that are both enlightening and focused tend to emphasize processes and associations that many readers find surprising. We are rewarded for deriving logical deductions from theoretical first principles that generate surprising predictions linking domains that are often considered separate; for example, demonstrating that people who receive little autonomy on the job give their kids little autonomy in the home (Kohn and Schooler, 1978). The trouble is that the most interesting causal factors are often not the most important. A few years after graduate school, I gave a talk at the University of South Carolina on the effects of network position on certain organizational outcomes. During the question period, the late Bruce Mayhew asked why I had spent forty of my forty-five minutes talking about network measures, when organizational size explained twice as much variance in my dependent variables. If I really cared about the outcomes I was trying to explain, why hadn't I focused on size?

In formulating an answer, I realized that I had never thought of size as interesting: Didn't everyone know that size would influence many measures of organizational persistence and effectiveness? Why talk about what everyone knew? At the same time, if our job is to explain the world, rather than to note small but paradoxical statistical relationships, shouldn't we focus precisely on the measures and processes that explain the most? Our collective preoccupation with theoretical novelty often leads organizational researchers to overlook crucial if banal patterns in their data (sometimes even omitting "dull" variables at the cost of misspecifying statistical models). Once again, one must find the point on the tightrope at which balance can be achieved.

3. Theory Construction is Social Construction, Often after the Fact

Even if one constructs a careful hybrid theoretical strategy and finds the proper balance between the conflicting values that good theory may embody, that theory's fate will be determined in part by factors outside one's control. If the

production of good theory requires the utmost care, theory's reception is ordinarily helter-skelter: a process of appropriation driven more by resonance than by reason, in which complex arguments are reduced to slogans and related to one another along binary dimensions more redolent of Levi-Strauss's tribal cultures than of graduate theory classes. And not only is theory created by its readers as well as its writers—it is then recreated by the authors who employ it.

The value of resonance. Here is an hypothesis (not a theory): The reception of a theory is shaped by the extent to which a theory resonates with the cultural presuppositions of the time and of the scientific audience that consumes it. An example helps explain this. Before going to watch sea lions mate on a central Californian beach several years ago, I read an account of the importance of this beach as a site of contests between males for dominance. The winning males, so the story went, were rewarded with the affection of female sea lions—and with it the opportunity to pass their genes to new generations, as nature selected for sea lion machismo. Shortly thereafter I met a woman biologist who had been studying the sea lions and other species famed (among male biologists) for their dominance contests. According to this scientist, the beaches attracted a large but finite proportion of the sea lions. While the big bulls blustered at one another on the beach, a sizable minority of sneaky little male and female sea lions frolicked about the outer islands, breeding happily, beyond the view of much of the scientific world.

At first, such critics received little attention. But as feminist theory (e.g., Haraway, 1989) made scientists more sensitive to the ways in which human culture influences how biologists understand and portray the natural world, their voices penetrated scientific debate, enriching theories of biological evolution. The change was not so much in the theories available to biologists, much less in the social lives of sea lions, as in the intellectual environment—an environment shaped by the humanities and social sciences—into which evolutionary arguments were released.

I suspect that the same thing happens in organizational science, as cultural change enhances or corrodes our capacity to see aspects of the organizations we study, by limiting the metaphors we think with. If one observes the progress of organization theory from the 1950s through the 1990s, it is intriguing to consider the relative impact on our theories of organizational change, on the one hand, and changes in broader preoccupations and cultural repertoires, on the other. To what extent did the appeal of political theories of bureaucracy (Cyert and March, 1963; Allison, 1969) reflect the inchoate decline of political orthodoxy in that decade? To what extent did the popularity in the U.S. of theories that emphasized limits to rational control of organizations (garbage-can, resource-dependence, ecological, institutional theories) in the 1970s bear an affinity to doubts and fears associated with a decline in America's international hegemony? To what extent does the popularity of realist depictions of organizations as dense spots in multiple networks of relations (the so-called network organization

debated in Nohria and Eccles, 1992), rather than highly bounded bureaucracies, reflect actual organizational change, and to what extent does it mirror the antinomian temper of the 1990s and the widespread feeling that every surface unity masks a more complex underlying structure?

Theories into slogans. People read quickly. Unless their teaching or research leads them to attend to a paper or book with special care, they will pick from the field of ideas in any theoretical work those that resonate with preexisting expectations and assumptions and forget the rest. In many cases, they will further simplify the ideas they retain until those ideas fit neatly into preexisting schemas (Fleck, 1979; D'Andrade, 1995). The more widely a theoretical paper or book is read, the greater the proportion of readers who are not specialists in the subject matter it addresses. The greater the proportion of nonexpert readers, the greater the extent to which its reception is determined by a cognitive field different than that of its authors, and the greater the extent to which its arguments are refashioned and simplified.

I became familiar with this process when a paper that Woody Powell and I coauthored (DiMaggio and Powell, 1983), "The Iron Cage Revisited," came to be assigned widely to graduate classes in organization theory and sociology. Within the field of institutional theory it represented a relatively materialist variant, emphasizing, as it did, the role of interorganizational networks in driving processes of imitation and diffusion that tended to make organizations similar to one another. Somewhat to my surprise, I began receiving papers that cited our paper as support for the proposition that all organizations become like all others, regardless of field. Somehow the network argument that we authors regarded as so central had been deleted in the paper's reception. Within a few more years, the paper had turned into a kind of ritual citation, affirming the view that, well, organizations are kind of wacky, and (despite the presence of "collective rationality" in the paper's subtitle) people are never rational.

Related to this is a tendency for the field to classify theories on the basis of primordial antinomies rather than coherent and multidimensional analytic categories. For example, students (and sometimes non-students who should know better) often divide the world of organization theories between organizational culture, garbage-can, institutional, and loose-coupling approaches, on the one hand, and transaction cost, technical functionalist, population ecology, and agency theory approaches, on the other. This classification seems to owe more to intuitive notions of "hard" and "soft" than to analytic positions on the role of environments, open vs. closed systems, rational action, or other factors that provide the basis for systematic multidimensional mappings.

Post hoc theory construction. Theories are not just constructed, they are socially constructed after they are written. Theoretical ideas take on a life of their own. In some cases, sophisticated ideas are degraded. In other cases, half-baked ideas go back into the oven, coming out in

more satisfactory form. To some extent, the quality of a theory is a function of the quality of the people who employ it. In drafting a recent paper on polarization, I found that the most useful papers and books on the topic all cited Georg Simmel's (1955) famous essay on conflict, so I returned to the original, which I had not read for many years. Much as I admire Simmel, I did not find the ideas that claim his paternity in very crisp form. My conclusion is that if your theory happens to be taken up by the likes of Lewis Coser (1956), Ron Breiger (1974), and Peter Blau (1977), your reputation is in very good hands. The supreme example of this phenomenon in organization theory must be Chester Barnard (1938). I doubt that *The Function of the Executive* satisfies any formal criterion of good theory construction. But for whatever reasons, it inspired Herbert Simon, who ignored all that was quaint and vacuous in the book and joined with James March to turn Barnard's convoluted prose into a crystalline and compelling propositional inventory (Simon, 1957; March and Simon, 1958). From this promising beginning, the book's fame grew, until several years ago, a belated festschrift featured the most diverse group of organizational theorists imaginable, each paying homage to Barnard and his work (Williamson, 1990). One could point to formal qualities that made *Function* a plausible candidate for such treatment: Barnard's prose is often ambiguous and the book's argument is undisciplined, both of which mean that a contemporary theorist seeking some sign that Barnard anticipated one of her or his best ideas has a target as wide as an aircraft carrier. But one would not recommend that the aspiring theorist emulate these qualities.

CONCLUSION

I have suggested two modest revisions to Sutton and Staw's argument. First, good theory is so difficult to produce routinely, in part, because "goodness" is multidimensional: The best theory often combines approaches to theorizing, and the act of combination requires compromise between competing and mutually incompatible values. Second, theory construction is a cooperative venture between author and readers: Theory reception rides on much more than scientific potential; in the short run, we tend to reduce theories to slogans; and in the long run, brilliant expositors can turn muddled theories into canonical masterpieces. If the first set of points, on tensions within theories, highlights the need for theorists to exercise judgment and pluck, the second suggests the importance of environment and luck.

Let me be quite clear, however, that these remarks are meant to qualify Sutton and Staw's account, not to question their main lines of argument or the usefulness of their superb description of non-theory. Any readers who find in my qualifications warrant to disobey Sutton and Staw's injunctions deserve whatever the reviewers deal them.

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