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Administrative Science Quarterly, Vol. 40, No. 3 (Sep., 1995), 385-390.

Stable URL:
http://links.jstor.org/sici?sici=0001-8392%28199509%2940%3A3%3C385%3AWTINTI%3E2.0.CO%3B2-%23

Administrative Science Quarterly is currently published by Johnson Graduate School of Management, Cornell University.

What Theory Is *Not*, Theorizing *Is*

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Products of the theorizing process seldom emerge as full-blown theories, which means that most of what passes for theory in organizational studies consists of approximations. Although these approximations vary in their generality, few of them take the form of strong theory, and most of them can be read as texts created "in lieu of" strong theories. These substitutes for theory may result from lazy theorizing in which people try to graft theory onto stark sets of data. But they may also represent interim struggles in which people intentionally inch toward stronger theories. The products of laziness and intense struggles may look the same and may consist of references, data, lists, diagrams, and hypotheses. To label these five as "not theory" makes sense if the problem is laziness and incompetence. But ruling out those same five may slow inquiry if the problem is theoretical development still in its early stages. Sutton and Staw know this. But it gets lost in their concern with theory as a product rather than as a process. To add complication and nuance to their message, I want to focus on the process of theorizing.

Before I do so, I want to confess to considerable unease in saying anything that could dilute their basic point. It's conceivable that journals are suffering from a rash of submissions with half-baked pretensions to theory, and Sutton and Staw are trying to root them out and put future authors on notice. Any commentary that complicates that message or adds nuance to it may subvert a straightforward plea. I'd hate to get in the way of the message that we need better theory. I would also hate to dilute the message that some authors who think they are moving us toward better theory actually are doing the opposite. Thus, I endorse Sutton and Staw's message that theory is not something one "adds," nor is it something one transforms from weaker into stronger by means of graphics or references, nor is it something that can be feigned by a flashy conceptual performance. That having been said, I want to complicate their essay by a closer look at the theorizing process.

My main reaction to their argument is that I am less certain than Sutton and Staw seem to be that the five article parts they discuss are not theory. I say this because most theories approximate rather than realize the conditions necessary for a strong theory and because these five parts themselves have gradations of abstractness and generality.

I begin with the issue of approximation. Most products that are labeled theories actually approximate theory. Merton (1967) has been most articulate about this point and suggests that approximations take at least four forms: (1) general orientations in which broad frameworks specify types of variables people should take into account, without any specification of relationships among these variables; (2) analysis of concepts in which concepts are specified, clarified, and defined but not interrelated; (3) post-factum interpretation in which ad hoc hypotheses are derived from a single observation, with no effort to explore alternative explanations or new observations; and (4) empirical generalization in which an isolated proposition summarizes the relationship between two variables, but further

0001-8392/95/4003-0385/$1.00.

interrelations are not attempted. While none of these are full-blown theories, they can serve as means to further development. If they are serving this function, then it is imperative that the author make this clear. But the mere presence of any one of Merton's four is not proof that shoddy theorizing is underway. And all four of these might assume the guise of one or more of the parts of an article that trouble Sutton and Staw.

The assertion that theory is approximated more often than it is realized can be illustrated by Runkel and Runkel's (1984: 129–130) marvelous effort to argue that theory is a continuum rather than a dichotomy:

> Many social scientists hesitate to claim they are writing *theory.* We see titles of articles, even books, like *An Approach to a Theory of . . . , Notes Toward a Theory of . . .* , and *A Prolegomenon to a Theory of. . . .* Instead of *theory,* we see words and phrases that mean about the same thing: *Conceptual Framework for, Some Principles of, Model of.* Rarely do we see a title that says straight out: *A Theory of. . . .*
>
> Perhaps some social scientists yearn for a Theory That Sweeps Away All Others. Perhaps they avoid being accused of overweening ambition by claiming not to be writing a Theory, but only a conceptual framework or a model. We think it too bad to reserve *theory* to mean only Good Theory or Grand Theory or Unassailable Theory. We would like writers to feel free to use *theory* whenever they are theorizing. Modesty is all very well, but leaning over too far backward removes a good word from currency.
>
> *Theory* belongs to the family of words that includes *guess, speculation, supposition, conjecture, proposition, hypothesis, conception, explanation, model.* The dictionaries permit us to use *theory* for anything from "guess" to "a system of assumptions, accepted principles, and rules of procedure devised to analyze, predict, or otherwise explain the nature or behavior of a specified set of phenomena" (*American Heritage Dictionary*). Social scientists will naturally want to use terms with more care than they are used by the general populace. They will naturally want to underpin their *theories* with more empirical data than they need for a *speculation.* They will naturally want a *theory* to incorporate more than one *hypothesis.* We plead only that they do not save *theory* to label their ultimate triumph, but use it as well to label their interim struggles.

If everything from a "guess" to a general falsifiable explanation has a tinge of theory to it, then it becomes more difficult to separate what is theory from what isn't, especially if theory development starts with guesses and speculations and ends with explanations and models.

There is a third sense in which theories in organizational studies are approximations, and this involves slippage in the theory construction process. Theorists start with a vision for a theory and change it "from entwined ideas at the edge of words to a linear order in which the ideas are unraveled and set forth in the form of a propositional argument" (TenHouten and Kaplan, 1973: 147). A nonlinear vision loses accuracy when it is converted into propositions. That is normal, natural trouble in theorizing. But it is also one more reason why theories approximate, why they are one-sided accentuations (Bacharach, 1989: 497), and why it can be tough to separate texts that are not theory from texts that are. A text that looks like "not theory" may simply be a

clumsy attempt to disassemble a gestalt into linear propositions. With more practice and more nuanced language comes more of the originating insight.

So part of what Sutton and Staw make clearer to me is how hard it is in a low-paradigm field, in which people are novice theorists, to spot which of their efforts are theory and which are not. This difficulty arises because theory work can take a variety of forms, because theory itself is a continuum, and because most verbally expressed theory leaves tacit some key portions of the originating insight. These considerations suggest that it is tough to judge whether something is a theory or not when only the product itself is examined. What one needs to know, instead, is more about the context in which the product lives. This is the process of theorizing. If we take a closer look at Sutton and Staw's five forms of "no theory," some seem closer to theory than others, and all five can serve as means to theory construction.

Unconnected references, especially those that are ceremonial citations, are not theory when they merely point to theories. I agree with Sutton and Staw that we need more precise descriptions of what is being extracted from references. This prescription needs to be conveyed to reviewers as well as authors, since reviewers make the same error. They are just as likely as authors to recommend additional references as substitutes for theory without being any clearer about why the reference is relevant. Furthermore, if authors do pinpoint and paraphrase key ideas in references, rather than simply point to them, this takes space. Reviewers and editors may need to be more tolerant of space-consuming exegesis if they want something other than cryptic citations.

Data by themselves are not theory, and Bacharach (1989: 497) made the same point in the *Academy of Management Review* special issue on theory. That having been reaffirmed, theorists also need to be attentive to Starbuck's (1993) argument that, just as the best medical doctors treat symptoms directly *without* relying on diagnosis to determine treatments, the best theorists may make prescriptions based on data alone *without* introducing theory between data and prescriptions. In both cases, diagnoses and theories come last and summarize observed relations between treatments/prescriptions and symptoms/data. In both cases, there are more combinations of symptoms than there are diagnoses or theories, which means that translating symptoms into diagnoses discards information. Since there are also more treatments than diagnoses, basing treatments on diagnoses injects random errors. The key links are between symptoms and treatments, with feedback from treatments making these links clear. Once these effects become clear, *then* the theorist knows better what is being treated and can attempt a diagnosis or explanation. Starbuck (1993: 91) summarized his argument this way:

> Academic research is trying to follow a model like that taught in medical schools. Scientists are translating data into theories, and promising to develop prescriptions from the theories. Data are like symptoms, theories like diagnoses, and prescriptions like treatments. Are not organizations as dynamic as human bodies and similarly complex? Theories do not capture all the information in

data, and they do not determine prescriptions uniquely. Perhaps scientists could establish stronger links between data and prescriptions if they did not introduce theories between them. Indeed, should not data be results of prescriptions? Should not theories come from observing relations between prescriptions and subsequent data?

Sutton and Staw are still right, data are not theories. If data alone are presented in lieu of theory, and reviewers are tempted to reject the effort as bad theory, the data may be tied to prescriptions and treatments as a means to get to theory. If this is the case, then the data have more theory relevance. This rationale needs to be spelled out by the author, but it is a reason why data may be closer to theory than they appear to be.

I suspect that tight coupling between treatments and symptoms, with belated theorizing of the outcomes, is a fairly common tactic in theory construction. In my own *ASQ* paper reanalyzing the Mann Gulch disaster (Weick, 1993), the argument developed partially by taking the Mann Gulch data as symptoms and, through a series of thought trials corresponding to treatments, seeing which concepts made a difference in those symptoms. This exercise in disciplined imagination resulted eventually in the theory that sensemaking collapses when role structures collapse and in the realization, helped through discussions with Lance Sandelands, that Freud had said the same thing 70 years earlier. Consistent with Sutton and Staw's point, I did not simply submit the Mann Gulch symptoms/data as theory. But consistent with Starbuck's point, neither did the theory come early in the process. The differential "responsiveness" of data to changes in a treatment is frequently an informative precursor to theorizing. Some people who seem to confuse data with theory may simply be midway through this process. Their progress may or may not warrant publication. But their blurring of lines between data and theory may foreshadow active rather than lazy inquiry.

Lists of variables are farther from a well-developed theory than are stories, but lists still can approximate a theory. The tacit message in a list is that items *not* on this list are less crucial determinants than those that are on it. Another tacit message of a list is that the more items on the list that are activated, and the stronger the activation of each, the more determinate is the relationship. Lists also convey the tacit message that causation is assumed to be simultaneous rather than sequential, that history is less crucial than contemporary structure, that relations among items are additive, and that items toward the top of the list are more important than items toward the bottom. I realize these informal theoretical messages may be inadvertent and simply wrong. But as long as there is an implied set of relations among items in the list, or one can infer such relations, there are the beginnings of a theory. I would never accept a list as a theory, nor would Sutton and Staw. I might, however, be more inclined to treat the list as closer to theory than they would, since relatively small amounts of tweaking and articulation could make it an explanation.

Diagrams are not theory. But if you compare diagrams to lists, it should be clear that diagrams are more explicit than

lists about sequence, about more and less determinate relationships, and about pathways of influence. These connections are clear in many of the diagrams found in Staw's theorizing (e.g., Figure 2, Staw and Ross, 1987: 720). I personally envy his skill at crafting meaningful boxes and arrows because, having tried over and over without success to do it, I know how hard it is. When Staw and Ross (1987) diagram the antecedents of escalation, their diagram shows that as commitment escalates so, too, does the number of people involved and the level of analysis necessary to capture the key dynamics (e.g., from individual domain to project domain to structural domain). The implied proposition is that commitment that escalates within a level is less costly than commitment that escalates between levels. They also imply that the more domains that are activated by the commitment, the more likely it is to escalate. These patterns are implicit in their diagrams, rather than explicit in their propositions. But they are plausible assertions suggested by the way they represent the phenomenon, and they generalize across particulars.

Finally, stand-alone hypotheses ("empirical generalizations" in Merton's list) are not themselves theory because authors remain silent about why these hypotheses and not other ones are being stated. Nevertheless, isolated hypotheses are close to theories and lack only connections with propositions and concepts to make them into such (Bacharach, 1989: 498–499). Such connecting may necessitate little more than raising the level of abstraction of key terms in the hypotheses.

To summarize my point about the five parts, the issue seems to be one of means and ends. And the question is, Do you publish just ends, or do you publish what Runkel and Runkel called "interim struggles?" The process of theorizing consists of activities like abstracting, generalizing, relating, selecting, explaining, synthesizing, and idealizing. These ongoing activities intermittently spin out reference lists, data, lists of variables, diagrams, and lists of hypotheses. Those emergent products summarize progress, give direction, and serve as placemarkers. They have vestiges of theory but are not themselves theories. Then again, few things are full-fledged theories. The key lies in the context—what came before, what comes next? And this question of context can be phrased in terms of Sutton and Staw's five parts. If prior and subsequent steps in theorizing are merely more of the same—diagrams preceded this paper and diagrams will be the focus of the next paper—then the theorizing is less robust and promising than if people are moving from one of the five, through a second of the five, on to a third of the five. Furthermore, references and data seem to have less generality and seem to be farther from theory than do lists, diagrams, and hypotheses. If that is plausible, then it means that it is easier to reject papers that use the first two in lieu of theory than those that use the last three.

So where does this leave us? It says in part that if much of what we do consists of approximations, then, as Sutton and Staw say, we may expect too much of any one attempt at theorizing. If any explanation will always be deficient in one or more of the qualities of generality, accuracy, and

simplicity, then the best we can hope for are tradeoffs. Actually *ASQ* already knows this. Sutton and Staw begin their essay by quoting this sentence from the Notice to Contributors: "If manuscripts contain no theory, their value is suspect." The next two sentences, which they left out, speak of tradeoffs: "Ungrounded theory, however, is no more helpful than are atheoretical data. We are receptive to multiple forms of grounding, but not to a complete avoidance of grounding."

Perhaps the ultimate tradeoff is the one between process and product, between theorizing and theory, between doing it and freezing it. If one or more of the five texts are not part of an interim struggle that is clearly articulated and documented, then their use in lieu of theory warrants rejection. If, however, the five are part of an interim struggle that is moving from one text to another, a struggle whose past and future is made clear by the author, then I would be tempted to give the author another shot at articulating both the process and the product and to ask for a revise and resubmit.

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