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Theory Construction as Disciplined Imagination

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The process of theory construction in organizational studies is portrayed as imagination disciplined by evolutionary processes analogous to artificial selection. The quality of theory produced is predicted to vary as a function of the accuracy and detail present in the problem statement that triggers theory building, the number of and independence among the conjectures that attempt to solve the problem, and the number and diversity of selection criteria used to test the conjectures. It is argued that interest *is a substitute for validation during theory construction, middle range theories are a necessity if the process is to be kept manageable, and representations such as metaphors are inevitable, given the complexity of the subject matter.*

Theorists often write trivial theories because their process of theory construction is hemmed in by methodological strictures that favor validation rather than usefulness (Lindblom, 1987, p. 512). These strictures weaken theorizing because they de-emphasize the contribution that imagination, representation, and selection make to the process, and they diminish the importance of alternative theorizing activities such as mapping, conceptual development, and speculative thought.

Theory cannot be improved until we improve the theorizing process, and we cannot improve the theorizing process until we describe it more explicitly, operate it more self-consciously, and decouple it from validation more deliberately. A more explicit description is necessary so we can see more clearly where the process can be modified and what the consequences of these modifications might be.

Theorizing consists of disciplined imagination that unfolds in a manner analogous to artificial selection. To understand this analogy, we should first see descriptions of the theorizing process and how these descriptions often misrepresent the process. Second, we can learn how some of these misrepresentations can be corrected if theorizing is viewed as disciplined imagination, where the "discipline" in theorizing comes from consistent application of selection criteria to trial-and-error thinking and the "imagination" in theorizing comes from deliberate diversity introduced into the problem statements, thought trials, and selection criteria that comprise that thinking. An elaboration of the theorizing process model is thus organized around the three components of problem statements, thought trials, and selection criteria.

Descriptions of Theory Construction

An understanding of the terms *theory*, *validation*, and *quality of theory* is necessary for an understanding of the model. *Theory* is a dimension rather than a category (Mohr, 1982, p. 6; Runkel & Runkel, 1984, pp. 129–130), which means that the more fully a generalization satisfies the criteria of a theory, the more it deserves the label *theory*. By theory we mean "an ordered set of assertions about a generic behavior or structure assumed to hold throughout a significantly broad range of specific instances" (Sutherland, 1975, p. 9). The dimensions implied by the definition are indicated by the terms *ordered*, *generic*, and *range*. As generalizations become more hierarchically ordered, behaviors and structures that are the focus of the generalizations become more generic, and as the range of specific instances that are explained becomes broader, the resulting ideas are more deserving of the label *theory*.

Verification and *validation* are used interchangeably to mean the demonstration, beyond pure chance, that the ordered relationship predicted by a hypothesis exists and thereby lends support to the hypothesis (adapted from Lastrucci, 1963). Proof, in other words, consists of verification of a probabilistic statement. As Lastrucci (1963) noted,

> Thus, for example, to say that the theory of inherited characteristics has been "validated" by demonstrating it in a given number of predictable instances is tantamount to saying that the expressed relationship is a reliable one. To an increasing extent, scientists tend to avoid implications of causality by thinking of verification as an expression of high reliability. (pp. 236–237)

Finally, a good theory is a plausible theory, and a theory is judged to be more plausible and of higher quality if it is interesting rather than obvious, irrelevant or absurd, obvious in novel ways, a source of unexpected connections, high in narrative rationality, aesthetically pleasing, or correspondent with presumed realities. Each of these outcomes is more likely when theorists develop fuller problem statements, create more diverse thought trials, and apply multiple selection criteria more consistently to these thought trials.

Previous Descriptions

Given these background assumptions, we can now look more closely at what has been said previously about the actual activities that go on during theory construction. Unfortunately, the literature on this topic is sparse and uneven, and tends to focus on outcomes and products rather than process. For example, Freese (1980), in constructing his review of formal theorizing for the *Annual Review of Sociology*, discovered the "incredible anarchy" of "language, conceptions, proposals, interpretations, and results of formal theorizing" (p. 189). Freese's attempt to impose some order on this anarchy is impressive and recommended reading. Other suggestions of process are found in sources such as Reynolds (1971), Blalock (1969), Johnson, Dandekar, and Ashworth (1984), and Merton (1967). Representative previous descriptions of process include the work of Homans, Kaplan, and Freese.

Homans (1964) described theory construction as the concurrent development of concepts, propositions that state a relationship between at least two properties, and contingent propositions whose truth or falsity can be determined by experience (a noncontingent proposition is a straightforward mathematical deduction). Of particular interest is Homans' irritation with theorists who equate theory with conceptual definitions; he stated that "much official sociological theory consists in fact of concepts and their definitions: it provides the dictionary of a language that possesses no sentences" (p. 957). As Homans makes clear, researchers cannot make deductions from concepts alone even though Parsons repeatedly tried to do so. The lesson to be learned is that any process must be designed to highlight relationships, connections, and interdependencies in the phenomenon of interest.

Kaplan (1964) contrasted knowledge growth by intention with knowledge growth by extension (p. 305). This contrast, which resembles Barlett's (1958) distinction between interpolation and extrapolation, suggests two different processes of theory building. Intention is used when a partial explanation of a whole region is made more and more adequate. This strategy is illustrated by the work of Darwin and Freud, but it also seems applicable to the work of Bateson, J. D.

Thompson, and Selznick. Theorizing in this mode lays out the lines that will be followed in subsequent theory and observation. Representative metaphors are developing a photographic negative, bringing binoculars into sharper focus, or gradually adding light to a darkened room.

Knowledge growth by extension is used when a relatively full explanation of a small region is then carried over to an explanation of adjoining regions. This strategy is illustrated by the expansion of studies of conditioning into a concern with more complex forms of learning. The work of Perrow on normal accidents, Bruner on narrative rationality, and Staw on escalation illustrate this strategy. Representative metaphors include a mosaic built piece by piece, science as an edifice that is constructed much like an erector set, and a puzzle that is gradually solved as more pieces are put into place.

Freese (1980) made a related distinction in which he distinguished between two strategies: (a) the strategy of developing generalizations in open systems through the use of inductive abstraction, a strategy evident in the work of Blau or Thibaut and Kelley; (b) the strategy of developing predictions in hypothetical or artificial closed systems, as represented in the work of Harrison White or Ken MacKenzie.

Closest in spirit to the current model is Bourgeois (1979). He suggested that seven steps are involved in building theories of the middle range and he presented these steps as chapter headings in a thesis. They include, (1) partitioning of the topic under investigation, (2) method of theory construction, (3) review of literature, (4) construction of theory-induction from empirical base, (5) extension of theory-deduction into propositions, (6) metaphysical elaboration, and (7) conclusion.

Although this list suggests that theory building is virtually indistinguishable from problem solving, there are some important subtleties. First, Steps 3, 4, and 5 occur concurrently rather than sequentially. Second, Step 6, metaphysical elaboration, is described as a receptacle for the intuitions that surface during the theory-building task. These intuitions consist of "conceptualizations that might not fit the categories delineated or forced by the imposed rigor of the general theory building" (p. 445). This wisdom of the theoretician, expressed in discursive form, consists of speculative ideas and deductions that may be untestable; these may be crucial outcomes of the theorizing process. Third, Bourgeois insisted that the process continuously should weave back and forth between intuition and data-based theorizing and between induction and deduction. He concluded with five prescriptions such as "read some of the old masters," "ground your theory on data," and "take advantage of serendipity."

Closest in content to our argument is Campbell's (1962, 1969, 1974) discussion of theorizing as ideational trial and error. Campbell (1974, p. 415) argued that the process of knowledge building is an evolutionary sequence that involves trials in the form of conjectures and errors in the form of refutations. Thus, as Popper (1966) said, imagination becomes a "benign environment that permits our hypotheses to die in our stead." Learning is viewed as a cumulative achievement, and theorizing is viewed as "selective propagation of those few social constructions that refer more competently to their presumed ontological referents" (Campbell, 1986, p. 118). Selection of these more competent social constructions is done either by the external environment or by mental selectors that represent that external environment and select on its behalf (Campbell, 1974, p. 430).

Although variations on Campbell's ideas form the framework of our argument, the present model places greater emphasis on representations as a selection environment and less emphasis on validation as the ultimate goal of theory construction.

Limitations of Previous Descriptions

While each of the authors just mentioned has important ideas about the process of theory building, the descriptions portray theorizing as mechanistic, with little appreciation of the often intuitive, blind, wasteful, serendipitous, creative quality of the process. Nor do their descriptions make clear the choice points in the process where theorists can act differently and produce theories of better quality. Most existing descriptions of the theorizing process assume that validation is the ultimate test of a theory and that theorizing itself is more credible the more closely it simulates external validation at every step. Thus, a dual concern with accurate representation and close correspondence between concepts and operations is evident virtually from the start in any theorizing activity. These concerns can be counterproductive to theory generation.

Most descriptions of theory construction sound very much like conventional linear descriptions of problem solving (e.g., Jackson, 1975), which is unfortunate in at least two ways. First, as Bourgeois took pains to make clear, theory building involves simultaneous parallel processing, not sequential thinking. One might go even further and argue that when theorizing *is* modeled after linear problem solving, the outcomes are unremarkable. Second, when theorizing is equated with problem solving, the theorizing is dominated by the question, Does this conjecture solve the problem? That construction is unduly narrow because theorizing does not always originate in response to a problem (Ziman, 1987), and the single criterion of a solution is inadequate to cover other reasons why a conjecture might be selectively retained in theorizing (e.g., plausability, coherence, elegance, simplicity, usefulness).

Rather than adopt problem solving as the model of the theorizing process, researchers should view theory construction as sensemaking (e.g., Astley, 1985). Dubin (1976) pointed the way to this usage when he remarked that "a theory tries to make sense out of the observable world by ordering the relationships among elements that constitute the theorist's focus of attention in the real world" (p. 26). The problem of sensemaking for theorists occurs precisely because the correspondence between concepts and observables is so loose (Gergen, 1986), because the system being studied is open rather than closed (Henshel, 1971), and because the dissemination of earlier sensemaking alters the relationships that theorists are currently trying to order (MacIntyre, 1985).

Disciplined Imagination

When theorists build theory, they design, conduct, and interpret imaginary experiments. In doing so, their activities resemble the three processes of evolution: variation, selection, and retention. Because the theorist rather than nature intentionally guides the evolutionary process, theorizing is more like artificial selection than natural selection, and theorizing becomes more like natural selection the more the process is dominated by validation and empiricism.

The close parallel between theory building and evolutionary processes can be illustrated with the example of marine navigation by radar. The context for this event involves a ship navigating at night along a waterway filled with actual tugs, barges, rocks, and phantom objects that sometimes show up on radar screens. The problem is to avoid collisions with real objects. To solve this problem, variations in the form of radar emissions simulate possible routes among the objects. The selection criteria by which routes are evaluated consist of radar emissions that bounce back from what are presumed to be actual objects. The outcome of the process is selective retention of routes that avoid echoes. Various routes are simulated, and most of the simulated routes encounter echoes and are rejected, but a handful encounter nothing and are retained. These evolutionary processes are guided by representations of the environment, not by the environment itself. The radar emissions are a substitute for actually moving through the environment. The echoes from these emissions are substitutes for real objects, and the selection of echo-free routes is a substitute for actually moving around objects when they are encountered.

Theory building involves an analogous process. There is a context which, for the sake of this illustration, will be marine navigation at night using radar. However, the context now takes the form of a representation built from interviews, accident reports, firsthand observation, and intuitions. The problem is why marine captains who use radar often collide with the objects they see on their screen, including other ships that also have radar (Perrow, 1984, p. 214). To solve this problem, variations in the form of conjectures simulate possible scenarios that could explain the collisions, such as lack of ability, poor equipment design, visual illusions, stress, fatigue, and insurance settlements. The selection criteria by which a conjecture is selected or rejected include judgments of whether it is interesting, plausible, consistent, or appropriate. The outcome of this selection process is a conjecture that is retained or rejected by the theorist. An example of a conjecture that might be retained is the suggestion that captains who view ambiguous signs on their screens interpret those signs in the way that poses the least danger and assumes the safest reality possible (Perrow, 1984, p. 217). Given their preference for safe interpretations, captains don't take evasive action until it is too late.

Again, the key point is that the process is guided by representations. The selection criteria are especially noteworthy because this is where the theorizing process comes closest to the issue of validation and its substitutes. The criterion that selected among radar emissions was the presence or absence of echoes which were treated as surrogates for real collisions. The criterion that selected among thought trials was the presence or absence of an affective judgment (e.g., that's interesting, that's absurd) which was treated as a surrogate for confirmation or disconfirmation. Neither the echo nor the judgment provides direct evidence of real objects or valid ideas. Nevertheless, subsequent actions unfold as if the evidence were direct. The navigational route is altered, the interesting conjecture is cumulated with other conjectures, and in both cases there is selective retention of variations that satisfy one or more criteria.

Objects are more likely to be avoided and theoretical problems are more likely to be solved when the problem is represented more accurately and in greater detail with assumptions made more explicit, as a greater number of heterogeneous variations are generated, and as more selection criteria, of greater diversity, are applied more consistently to the variations that are generated. Alterations in these dimensions of the process of evolution and theory building should have a significant effect on the quality of the outcomes generated by these processes.

Problem Statements

The occasion for theorizing has variously been described as a puzzling story (Polanyi, 1989), problem (Lastrucci, 1963), question (Turner, 1987), strategic research material (Merton, 1987), specified ignorance (Merton, 1987), or an anomaly (Schank, 1988). Regardless of how the trigger to theorizing is described, it consists of some description that can vary in fineness of detail, accuracy, and explicitness of assumptions which it incorporates. The theoretical problem that trial and error thinking tries to solve is equivalent to the adaptation problem that trial and error locomotion tries to solve. A conjecture that solves a theoretical problem is equivalent to an action that discovers a niche. In both cases, the likelihood of a solution is determined in part by the way the environment is represented or perceived.

The two cases differ, however, in a crucial way. Theorists both choose the form of the problem statement and declare when their thought trials have solved the problem they pose, a sequence that resembles artificial selection. Theorists are both the source of variation and the source of selection. People searching for niches also may choose the form of their problem statements, but the environment declares which of their trials, if any, are solutions. This latter sequence resembles natural selection because the source of variation differs from the source of selection. Despite this difference, solutions in both cases are more likely to be discovered where the representations are fuller. Whether the problem is to find an explanation or a competitive advantage, fuller descriptions suggest a greater number of possibilities.

The problem statements that drive the theorizing process are more complex than they appear to be. Not only do they contain an anomaly to be explained, but they also contain a set of assumptions that can be confirmed or disconfirmed, a set of domain words that can be connected differently, details that can be generalized, a text that can be sorted into form words and substance words, an implied story whose plot may be implausible, and answers to questions not yet asked. This richness links the problem to the theorist's past experience and allows for more criteria than validation to become relevant to the process (see Kaplan's related discussion of theoretical coherence, 1964, pp. 312–319).

The choice of problem statements in organizational theorizing is complicated by the fact that organizational theorists, unlike theorists in other fields, are constrained in their choices. The result is that they often work on problems that are wide in scope, but limited in detail, inaccurate in their representation, and vague regarding the assumptions involved. The nature of the restriction is evident in the following description.

Natural scientists pick problems they can solve, work for colleague approbation rather than lay approbation, collaborate with people who share their interests and values, and seldom worry about what others think. The world of the social scientist, poet, theologian, and engineer is dramatically different. These people choose problems because they urgently need solution, whether they have the tools to solve them or not.

Now comes the crucial question: Which group would you expect to solve problems at a more rapid rate? Campbell (1986) put the issue this way:

> Since scientists have to live in the larger society and are supported by it in their scientific activity, it becomes probable that science works best on beliefs about which powerful economic, political, and religious authorities are indifferent. . . . Thus static electricity (rubbing cats' fur on amber) and magnetism were optimal foci of scientific growth. (p. 127)

By their very nature the problems imposed on organizational theorists involve so many assumptions and such a mixture of accuracy and inaccuracy that virtually all conjectures and all selection criteria remain plausible and nothing gets rejected or highlighted.

In this context the counsel to move toward theories of the middle range (e.g., Pinder & Moore, 1980; Merton, 1967; Weick, 1974) or toward theories that are nearly theories (Mohr, 1982) makes a different kind of sense. Middle range theories are solutions to problems that contain a limited number of assumptions and considerable accuracy and detail in the problem specification. The scope of the problem is also of manageable size. To look for theories of the middle range is to prefigure problems in such a way that the number of opportunities to discover solutions is increased without becoming infinite.

In this context it also makes sense to emphasize that we should pay just as much attention to problems defined by theorists as to those defined by practitioners. (See Evered and Louis, 1981, for a related discussion of insider-outsider perspectives.) While theorists may attack tractable rather than "relevant" problems, the outcomes they generate remain available as solutions to practitioner problems not yet identified. If theorizing resembles artificial selection, then theorists control both environmental selection and the criteria for survival of conjectures. This means that even though they may choose to study issues about which powerful people feel strongly, they need not. The thrust of Campbell's remarks is that they'll be better theorists if they don't.

Thought Trials

When faced with a problem, the theorist generates conjectures about ways to solve it. These conjectures, usually in the form of if-then sentences, vary at least in the number of trials generated and the heterogeneity between trials. In general, a theorizing process characterized by a greater number of diverse conjectures produces better theory than a process characterized by a smaller number of homogeneous conjectures.

The key property is heterogeneity among thought trials. The advantage of blind-variation, after which thought trials are modeled, is that the process can be "smarter" than the people who run it. If one thought trial has a minimal effect on the generation of the next thought trial, then a broader range of possibilities is tried. Given the tendency of humans to exhibit grooved, habituated, redundant thinking (Steinbruner, 1974), this requirement of good theorizing is among the most difficult to meet regularly.

There are, however, ways to increase independence among thought trials. Campbell (1962) argued that one implication of Simon's work with problem solving is that variation with a strong classification system is preferable to variation without strong classification (p. 66). A strong classification system, one in which an event clearly falls in one and only one category, is especially helpful when solutions are expected to be nonrandomly distributed. Classifications suggest when thought trials may be variations on the same theme rather than variations on different themes.

This point can be illustrated with Astley and Van de Ven's (1983) influential differentiation of organizational theory into four quadrants: system-structural (Q1), strategic choice (Q2), natural selection (Q3), collective-action (Q4). Variation in thought trials within one quadrant should be associated with fewer breakthroughs than would variations that originate in more than one quadrant. Thus, population ecologists (Q3) or social constructionists (Q2) who work within their paradigm should generate less interesting theories that are less important because the thought trials have dependencies among them. Blind alleys will be searched longer and more deeply when classification is weak or ignored than when it is strong and heeded. Those who argue for dialectical oppositions (Astley & Van de Ven, 1983), the cultivation of paradox (Quinn & Cameron, 1988), conceptualization at more than one level of analysis (Staw, Sandelands, & Dutton, 1981), and micro-macro linkages (Knorr-Cetina & Cicourel, 1981) can be viewed as people suggesting that heterogeneous thought trials are more likely than homogeneous thought trials to solve theoretical problems.

Independence among thought trials can be achieved by other means than strong classification systems. Any device that short circuits memory, foresight, or preference in the generation of thought trials increases the independence of these trials.

A good example is Crovitz's (1970) finding that in any scientific writing, such as the abstracts of articles published in *Science*, there are two kinds of words: words that might appear in any abstract (y words) and words such as substantive nouns that are specific to particular articles (x words). The ratio of x words to y words suggests how much jargon the article contains. Furthermore, if theorists delete the x words and keep the y words, they have a generic structure for theorizing. Once the content words have been removed from an argument, theorists are left with a perfectly good set of blanks into which their own nouns can be inserted. Those nouns will be put into relations with one another independent of the theorists' own preferences.

A structure of form words used by Theorist A can generate thought trials that could not be imagined by Theorist B, who thinks using a different structure. To insert the ideas of one structure into the forms of another structure is to increase both the heterogeneity among thought trials and the probability of an original solution. Weick and Orton (1986) experimented with this tactic using content from Staw, Sandelands, and Dutton's (1981) article on threat-rigidity cycles and found that a variety of sensible assertions could be made for topics totally different from those involving stress. It is unlikely that similar assertions would have been made by people working within those topics.

Given the argument that independence among thought trials is crucial for generating good theory, it is interesting to re-examine Lave and March's (1975) suggestion that theoretical propositions can be generated by increasing the generality of specific observations.

Lave and March observed that "Little men often start an argument in the presence of big men" (p. 65). Suppose the theoretical question is, Why does that happen? Potential answers can be generated by enlarging the initial statement and making it more general. Specifically, people need to make the nouns *men* and *argument* and the verb *start* more general, in ways that include the original nouns and verbs as special cases. Thus, Lave and March changed *little men* to *little people* to *physically disadvantaged* to *inequalities among people* and they changed *start an argument* into *act verbally aggressive* and then into *aggression*. With these changes they arrived at an inductive abstraction which reads, "Among people, inequalities in one domain lead to aggression in another."

Lave and March deliberately increased the dependencies among their thought trials by using words that include the earlier words to move toward a higher level of generality with respect to a single problem. In doing so, however, they treated *people* (male and female humans) as a synonym for *men*, a substitution which feminist scholars (e.g., Harding, 1986; Spivak, 1987) would question. Thus, the thought trial, in becoming more general, also becomes less capable of solving the problem of why little white males start arguments in the presence of big white males. The thought trial is less able to solve the theoretical problem because it is generated within a weak classification scheme (people are pretty much alike) that is insensitive to differences that might allow for more accurate understanding of the determinants of aggression.

The point here is not a point about gender, but about classification and independence. Heterogeneity among thought trials is more difficult to achieve than it might appear. Kuhn (1962), in fact, made his reputation on this very difficulty, namely, that thought trials tend toward homogeneity and create paradigms. Heterogeneity within paradigms is rare. Because preference and experience exert such a strong effect in the generation of thought trials, and because these effects are in the direction of homogeneity rather than heterogeneity, better theorizing necessitates devices that force more independence among thought trials. These devices include heterogeneous research teams (Weick, 1983), eclecticism, generalists, classification which identifies underexploited combinations, and randomizing devices.

Selection Criteria

Self-conscious manipulation of the selection process is the hallmark of theory construction. As with thought trials, both the number and diversity of the selection criteria make a difference. The greater the number of diverse criteria applied to a conjecture, the higher the probability that those conjectures which are selected will result in good theory. Furthermore, selection criteria must be applied consistently or theorists will be left with an assortment of conjectures that are just as fragmentary as those they started with. Every conjecture can satisfy some criterion. Thus, if criteria are altered each time a conjecture is tested, few conjectures will be rejected and little understanding will cumulate. The point about consistency is important because theorists have an investment in their ideas. They prefer to be right rather than wrong, bright rather than dull, clever rather than pedestrian. Those understandable preferences can lead to subtle shifts in criteria in the interest of preserving ideas unrelated to a problem. (Recall Bourgeois's category "metaphysical elaboration.") Thus, the theory-construction process improves when many diverse selection criteria are applied consistently.

Alternative portraits of the nature of selection during theory construction are suggested by the following two quotations:

> Given the laboratory for rejecting hypotheses, science will develop most rapidly when the widest range of guesses is being tried. If the testing process is very expensive, then vicarious testing through theoretical integrative efforts utilizing the empirical base of folk-wisdom, may be used as a preliminary, but it cannot carry the whole load and eventually experimental confrontation is required. (Campbell, 1961, p. 21)

> Consider two social worlds. In one, mankind is at the edge of an intellectual mastery of it, approaching a capability to lay out a structured set of propositions describing it with scientific precision. In the other, it is far, far from such a mastery; and man's incompetence is compounded by social change through social learning, which is itself accelerated by such accomplishments as can be credited to social science. His set of structured scientific propositions can never keep up with social change. In the first of these two worlds, the task of social science might be argued to be first and foremost that of achieving scientific validity. In the second, social science has to grasp (by every available method) at whatever limited understandings can be achieved; and they include the understandings of better conceptualization, better formulation of questions, simple reporting, illuminating speculation, rival unvalidated or untested hypotheses, among other possibilities. Hence the task of social science is to pursue all these understandings with caution about prematurely assigning priority to any one above the other. (Lindblom, 1987, p. 514)

The issue being debated is an issue of selection. In an earlier example to explain why ships collide at night, it was argued that a reaction such as "that's interesting" was sufficient to selectively retain a conjecture, independent of additional efforts to verify it. Eventual attempts at verification may occur sometime later but, for reasons discussed by Lindblom (1987), Gergen (1986), Henshel (1971) and others, the value of a theory does not ride on the outcome of those tests. The reason it does not is that validation is not the key task of social science. It might be if we could do it, but we can't—and neither can economists (Lindblom, 1987, pp. 516–517).

If validation is not a criterion for retaining conjectures, this means at least two things. First, the criteria used in place of validation must be explored carefully since the theorist, not the environment, now controls the survival of conjectures. Second, the contribution of social science does not lie in validated knowledge, but rather in the suggestion of relationships and connections that had previously not been suspected, relationships that change actions and perspectives. As Lindblom (1987) observed, "of all our valid knowledge of the social world, most of it seems to have been the product of lay rather than professional inquiry. . . . A typical situation in social science is that scientific inquiry only modestly raises the validity of a lay proposition by qualifying it" (p. 517).

If valid knowledge is difficult, if not impossible to attain in social science, then this puts theorizing and selection in a different light. Theorizing is no longer just a preliminary to the real work of verification, but instead it may involve a major portion of whatever verification is possible within the social sciences. In fact, if there is an inherent loose coupling between scientific concepts and the reality to which they refer (Gergan, 1986), then the only place where those ideas may be tested adequately would be in the imagined worlds of mental experiments, laboratory experiments, or computer simulations.

The generic selection criterion that seems to operate most often in theorizing and that substitutes for validation is the judgment, "that's plausible." The centrality of plausibility to the theorizing process can be understood in the following way. When theorists apply selection criteria to their conjectures, they ask whether the conjecture is interesting, obvious, connected, believable, beautiful, or real, in the context of the problem they are trying to solve. When they ask these questions of the conjecture, the criterion that lies behind the question incorporates considerable past experience with related problems which the theorist brings to bear on the conjecture.

The question in the Hawthorne studies asks, Why does decreased illumination lead to increased productivity? The conjecture, "Perhaps the people producing the output are receiving more attention than they are used to," may be tested against the criterion, "Is that conjecture interesting?" When the theorist asks if the "attention" conjecture is interesting, he or she actually tests the conjecture of increased attention against many more observations than those observed at Hawthorne. The conjecture is being tested against the theorist's prior experience that has been edited down into assumptions that are activated when the theorist asks, Is this conjecture interesting?

The assumption is a distillation of past experience. When that assumption is applied to a specific conjecture, the assumption tests the conjecture just as if an experiment had been run. When a conjecture is tested against an assumption, the outcome of that test is signified by one of four reactions: *that's interesting* (assumption of moderate strength is disconfirmed), *that's absurd* (strong assumption is disconfirmed), *that's irrelevant* (no assumption is activated), and *that's obvious* (a strong assumption is confirmed). Those four reactions are the equivalent of significance tests, and they serve as substitutes for validity. The judgment *that's interesting* selects a conjecture for retention and further use. That judgment is neither capricious nor arbitrary because it is made relative to a standard that incorporates the results of earlier tests. That standard takes the form of an assumption, and the conjecture is compared with this standard during theorizing.

Thus, plausibility is a substitute for validity. The process of theory testing by an experimental test is mimicked by the process of conjecture testing by an assumption test. In both cases, interesting outcomes are retained, while absurd, irrelevant, or obvious outcomes are dropped.

If theorizing is driven by concerns of plausibility rather than concerns of validity, it would follow that conjectures generated during theory construction are selected based on judgments of their plausibility, which can be assessed by a variety of selection criteria.

That's Interesting. "Interest" as a selection criterion has been discussed most fully by Davis (1971), but it has also been discussed by Mohr (1982), Wicker (1985), Daft (1983), Lundberg (1976), and Schank (1988). This criterion has been given prominence because it is tied more closely to past experience and prior tests than people realize. Davis's discussion of how to write interesting theories is often read as an invitation to engage in opportunistic, flashy theorizing.

However, another way to read his discussion is as a description of a tool that aids in diagnosing the adequacy of past understanding and the relevance of that understanding to current theoretical problems. It is as if the person proposes a conjecture, experiences a feeling of interest, and then uses that reaction as a clue to dig deeper and uncover what assumption has been disconfirmed, what data that assumption was based on, and what the implications for current understanding are of those newly awakened doubts. Whenever one reacts with the feeling *that's interesting*, that reaction is a clue that current experience has been tested against past experience, and the past understanding has been found inadequate.

An understanding of the criterion of *interest* explains part of the emotional side of theorizing. Theorists are usually pleased when their assumptions are disconfirmed, whereas nontheorists are worried when their assumptions are disconfirmed. A disconfirmed assumption is an opportunity for a theorist to learn something new, to discover something unexpected, to generate renewed interest in an old question, to mystify something that had previously seemed settled, to heighten intellectual stimulation, to get recognition, and to alleviate boredom. However, a disconfirmed assumption is a problem for a nontheorist because it suggests that past experience is potentially misleading as a guide for subsequent action and that coping may be more difficult.

A disconfirmed assumption interrupts a layman's well-organized activities and plans, but it accelerates the completion of the theorist's well-organized activities and plans. Those differential effects suggest that each should experience quite different emotional reactions to the experience of disconfirmed assumptions. Mandler (1975) and Berscheid (1983) argued that interruptions generate negative affect, especially when resumption of the interrupted activity is difficult, alternative ways to fulfill the plan are unavailable, the interrupted sequence is tightly organized, and the time interval during which the interruption lasts is lengthy. They also argued that positive affect occurs when obstacles are removed, when the attainment of plans is accelerated, and when organized responses can run more smoothly.

Thus, theorists should like disconfirmed assumptions because they accelerate the completion of their intention to build interesting theory, but nontheorists should dislike disconfirmed assumptions because they delay the completion of their intentions. However, once a theorist has a strong investment in a perspective, that investment should be expressed in plans to expand the audience for the theory and in well-organized responses that demonstrate and apply the theory. Once those changes occur, then disconfirmation should be felt as an interruption, and strong negative feelings should be exhibited. Generalists, people with moderately strong attachments to many ideas, should be hard to interrupt and, once interrupted, should have weaker, shorter negative reactions since they have alternative paths to realize their plans. Specialists, people with stronger attachments to fewer ideas, should be easier to interrupt and, once interrupted, should have stronger, more sustained negative reactions because they have fewer alternative pathways to realize their plans. Generalists should be the upbeat, positive people in the profession while specialists should be their grouchy, negative counterparts.

That's obvious. In the context of Davis's system, the reaction *that's obvious* is often the occasion to drop a conjecture from further consideration. Outside Davis's system, however, the judgment *that's obvious* has other meaning.

Homans (1964), for example, argued that major premises in sociological theory often go unnoticed and unstated because they seem simple and obvious. He noted the following:

> In sociology as well as in history, it is our major premises that we are most apt to leave unstated, particularly when they are psychological. We leave them unstated not only because they are obvious, but also because they are so obvious that we cannot bring ourselves to take them seriously. In the social sciences, unlike other sciences, the general laws are the ones men have always known most about, though they have not always formulated them as a psychologist would—and so they can hardly believe that they *are* general laws. Laws are things that have to be discovered; something lying around in plain sight comes too cheap to be a law. (p. 968)

Thus the reaction *that's obvious* may be a clue to significance as well as a clue to triviality.

Furthermore, what is obvious to one person clearly may be novel to someone else. Thus, the reaction *that's obvious* may trigger the question, For whom might this not be obvious? The search for an answer to this question might help establish the boundary conditions (Dubin, 1976) inside which a conjecture will hold true but outside of which it won't.

That's Connected. Crovitz (1970) developed the relational algorithm as a device to generate novel solutions to sticky problems. He extracted all 42 relational words that were used in the 850-word language system called Basic English and proposed that meaning essentially is established when one item is placed in one of these 42 relations to another item. Thus, in a situation with a speaker and an audience, the event is very different if we have a situation in which a person speaks down to an audience, up to an audience, about an audience, behind an audience, without an audience, among an audience, beneath an audience, or over an audience. When faced with a theoretical problem, a theorist can generate thought trials by selecting pairs of domain words from the problem (e.g., captain, radar) and then put them together with all possible relational words to generate conjectures about why the problem occurs.

However, the relational algorithm also embodies an important selection criterion: Is this event connected to that event? Theorists often assume that events are unrelated and reactions of interest often result when unexpected connections are discovered (Davis, 1971). To discover an unexpected connection is to discover a new set of implications. Thus, while the relational algorithm is a valuable device to create independence among thought trials—it puts domain words into relations that the theorist forgot about—it also activates the selection criterion of connections. The assumption that events are unrelated is disconfirmed when people discover they are connected and the reaction *that's interesting* serves as a clue to retain the conjecture.

That's Believable. Many problems that spur theory construction originate in some form of narrative (Polanyi, 1989; Polkinghorne, 1988). This is not surprising since, as James said, "To say that all human thinking is essentially of two kinds—reasoning on the one hand, and narrative, descriptive, contemplative thinking on the other—is to say only what every reader's experience will corroborate" (cited in Bruner, 1986, p. xiii). The standards by which narratives are judged differ from those used to judge arguments (Weick & Browning, 1986). Stories convince, not because they are truthful, but because they are lifelike, coherent, believable, and because they have verisimilitude (Robinson, 1981). Causality, for example, is handled differently in arguments and narratives. Bruner explained:

> The term *then* functions differently in the logical proposition "if *x*, then *y*" and in the narrative *recit* "The king died and then the queen died." One leads to a search for universal truth conditions, the other for particular connections between two events—mortal grief, suicide, foul play. (1986, pp. 11–12)

To judge a conjecture believable, in the context of a story, is to assess the degree to which it makes the story one starts with into a prototypical story; "a prototypical story identifies a protagonist, a predicament, attempts to resolve the predicament, the outcomes of such attempts, and the reactions of the protagonists to the situation" (Robinson & Hawpe, 1986, p. 112). If a conjecture strengthens one of these five elements in the story that spurs theory construction, or if it supplies an element that is missing, then the conjecture is more likely to be retained. If, however, the conjecture neither strengthens nor completes, then it is likely to be rejected.

That's Beautiful. Theorists sometimes use aesthetic criteria such as beauty to select conjectures (e.g., Mohr, 1982, p. 23), although this criterion is more likely to be found in mathematical theory construction. Representative is Poincaré's interesting conjecture that wishful thinking may err in the direction of the beautiful idea:

> The useful combinations [in mathematical invention] are precisely the most beautiful, I mean those best able to charm this special sensibility that all mathematicians know . . . when a sudden illumination seizes upon the mind of the mathematician, it usually happens that it does not deceive him, but it also sometimes happens, as I have said, that it does not stand the test of verification; well, we almost always notice that this false idea, had it been true, would have gratified our natural feeling for mathematical elegance. (cited in Campbell, 1962, p. 62)

There is no reason to believe this experience is enjoyed only by mathematicians, and Lave and March (1975) suggested that elegant models in the social sciences have the capacity to generate the same feeling (pp. 61–73).

That's Real. Problem statements, thought trials, and selection criteria constructed by theorists to aid theory construction are representations of a specific, material, external referent. Many theorists do not place as much emphasis on representation and suggest, instead, that ideas are selected by a more direct access to the real world in the selection process. Mach has said that a mental *copy* of the world makes deduction possible, and Campbell (1986) has said of the selection process, "Validity must come from the contribution of the referent of belief to the selection processes" (p. 118). Kaplan (1964), with typical clarity, noted that "science is governed by the reality principle, its thought checked and controlled by the things it thinks about" (p. 312).

There are intense debates about the degree to which the concepts of science correspond to the "things" it thinks about (e.g., Gergen, 1986; Needham, 1983), but for those who favor a tighter correspondence, and perhaps also for those with vivid, accurate, and detailed problem statements, the criterion *that's real* is a viable selector.

The criterion *that's real* invokes a combination of experience, practice, and convention to select among conjectures, whereas earlier criteria such as *interest* rely more heavily on imagined realities as selectors. That difference is potentially important when theorizing is considered as a largely internal, private process. Theorists can imagine as well as select realities that merely serve the interests of powerful organizational actors and not be aware that this is happening. It is a thin line from *that's interesting* to *that's in my best interest*, from *that's obvious* to *that's what managers want*, from *that's believable* to *that's what managers want to hear*, and from *that's real* to *that's the power system I want*.

While a reality check might catch these subtle, self-serving translations of theoretical analysis into theoretical advocacy, these translations also can be partially caught by disciplined application of evolutionary rules of thumb to thought trials. Myths can sneak through reality checks as well as imagined realities. Neither set of criteria is sufficient by itself.

There obviously are many more selection criteria than those discussed here. In the last analysis, the selection involved in theory construction is a process of editing, winnowing, and sifting. Goodfield described the style of theorizing used by a young experimental biologist named Anna, and in doing so illustrated the delicate sieve involved in theoretical selection:

> When a suggestion is first broached in science, however tentatively, it can be, and often is bolstered by little pieces of information which up to that point may well have seemed extraneous. These can now be picked up and cemented in place. Indeed, Anna once gave a lecture called "the Stained Glass Window Lecture," explaining that all scientists have these little pieces of colored glass, intriguing bits of information or facts which they didn't quite know what to do with. They leave them lying around until, prompted by a new idea or a new piece of information, they mentally sift and select the ones that may help the pattern. (quoted in John-Steiner, 1985, p. 186)

All sifting is not the same. As Campbell (1962, p. 65) added, "the likelihood of a productive thought increases with the wider variety of reasons one has for judging a given outcome 'interesting'. " Sifting with a greater number of distinct criteria, a process which Campbell calls "opportunistic multipurposedness," should produce theories that are more important.

Implications

The view that theory construction involves imagination disciplined by the processes of artificial selection has a variety of implications and raises a number of questions. Having made the process of theory construction more explicit, it now becomes clearer that theory construction can be modified at the step where the problem is stated (make assumptions more explicit, make representation more accurate, make representation more detailed), at the step where thought trials are formulated (increase number of trials generated, increase heterogeneity of trials generated), and at the step where criteria select among thought trials (apply criteria more consistently, apply more criteria simultaneously, apply more diverse criteria).

The difficulty in making these improvements is that many of them require independence among activities within a step and independence between steps. Needless to say, that is difficult, but not impossible, when all activities take place in the mind of the same theorist. Humans are serial information processors, able to compartmentalize, and willing to forget. Humans also can use devices that increase independence, devices such as the relational algorithm and strong classification systems, and social arrangements in the interest of independence. The minute a theorist goes public with his or her ideas, new points of view are introduced and dependencies decrease. Social arrangements such as research teams often serve the same function.

Suppose, however, that dependencies remain. That need not be fatal if the theorist begins with an explicit starting point or can recover retrospectively an approximation of that starting point. Retrieval of the starting point allows other people to begin at the same place and see where their thinking leads them.

Aside from the issue that the process of theory construction is always threatened by dependencies among thought trials, representations and mental selectors are a crucial component of theory construction and should not be taken lightly. That is especially true for organizational problems. Organizations are complex, dynamic, and difficult to observe, which means that whenever we think about them, the thinking will be guided by indirect evidence and visualizations of what they may be like, often captured in metaphors. That is not to apologize for the materials used in theory building. Rather, it emphasizes that theorists depend on pictures, maps, and metaphors to grasp the object of study. Theorists have no choice, but can be more deliberate in the formation of these images and more respectful of representations and efforts to improve them. Metaphors are not just catchy phrases designed to dazzle an audience. Instead, they are one of the few tools to create compact descriptions of complex phenomena. The fact that theory construction makes full use of representations is its strength, not its weakness.

The assessment *that's interesting* has figured prominently throughout, because it has been viewed as a substitute for validity. An assessment of interest represents the terminal stage of a substantial comparison between previous experience summarized into an assumption and a current experience summarized into a conjecture which questions that summary. The reaction *that's interesting* essentially signifies that an assumption has been falsified.

The preceding arguments suggest that the theorist is overloaded by demands to run a miniature evolutionary system in a head that suffers from bounded rationality. That load reaffirms the value of working toward theories of the middle range.

Much as theorists may resist the notion, most theory construction depends on conjectures, preserved in well-crafted sentences, that are tested in substitute environments by people who have a stake in the outcome of the test and may be tempted to bias that outcome. This is the drama that lies behind trial and error thinking and it lies close to the surface in much theory construction. However, it is a manageable drama.

The choice is not whether to do mental testing. Instead, the choice is how well this less than ideal procedure can be used to improve the quality of theoretical thinking. To build better theory, theorists have to "think better." That empty platitude takes on more substance when better thinking is interpreted to mean a more informed and deliberate use of a simulated evolutionary system.

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