

What Constitutes a Theoretical Contribution?

DAVID A. WHETTEN

University of Illinois

Since becoming editor of *AMR*, I have tried to find a simple way to communicate the necessary ingredients of a theoretical contribution. There are several excellent treatises on the subject, but they typically involve terms and concepts that are difficult to incorporate into everyday communications with authors and reviewers. My experience has been that available frameworks are as likely to obfuscate, as they are to clarify, meaning. Besides exposure to the works of Kaplan, Dubin, and others varies widely across the Academy.

This article is a rudimentary effort to fill this gap: The intent is not to create a new conceptualization of theory, but rather to propose several simple concepts for discussing the theory-development process. It is a personal reflection, which has emerged out of my daily editorial activities. My motivation is to ease the communication problems regarding expectations and standards, which result from the absence of a broadly accepted framework for discussing the merits of conceptual writing in the organizational sciences.

Finally, my comments should not be interpreted either as official *AMR* dogma or ironclad rules governing the evaluation process. Each submitted paper is unique, and it is judged on its own merits; however, my thinking has clearly been influenced by the hundreds of communications I have read during the first half of my editorship.

This article is organized around three key questions: (a) What are the building blocks of theory development? (b) What is a legitimate value-added contribution to theory develop-

ment? and (c) What factors are considered in judging conceptual papers? The first section describes the constituent elements of a theory. The second section uses this framework to establish standards for the theory-development process. The third section summarizes the expectations of reviewers regarding the substantive contribution and appropriateness of *AMR* papers.

What Are the Building Blocks of Theory Development?

According to theory-development authorities (e.g., Dubin, 1978), a complete theory must contain four essential elements, which are described in the following paragraphs.

What. Which factors (variables, constructs, concepts) logically should be considered as part of the explanation of the social or individual phenomena of interest? Two criteria exist for judging the extent to which we have included the "right" factors: comprehensiveness (i.e., are all relevant factors included?) and parsimony (i.e., should some factors be deleted because they add little additional value to our understanding?).

When authors begin to map out the conceptual landscape of a topic they should err in favor of including too many factors, recognizing that over time their ideas will be refined. It is generally easier to delete unnecessary or invalid elements than it is to justify additions. However, this should not be interpreted as license to throw in the kitchen sink. Sensitivity to the competing virtues of parsimony and comprehensiveness is the hallmark of a good theorist.

How. Having identified a set of factors, the researcher's next question is, How are they related? Operationally this involves using "arrows" to connect the "boxes." Such a step adds order to the conceptualization by explicitly delineating patterns. In addition, it typically introduces causality. Although the researcher may be unable to adequately test these links, restrictions in methods do not invalidate the inherent causal nature of theory.

Together the What and How elements constitute the domain or subject of the theory. The more complex the set of relationships under consideration, the more useful it is to graphically depict them. Not all theoretical treatises must contain figures with boxes and arrows, but a visual representation often clarifies the author's thinking and increases the reader's comprehension. In particular, formal models aid theory developers and users to assess the balance between parsimony and completeness.

Why. What are the underlying psychological, economic, or social dynamics that justify the selection of factors and the proposed causal relationships? This rationale constitutes the theory's assumptions—the theoretical glue that welds the model together. (Like Dubin, I do not distinguish between a model and a theory.)

The central question addressed here is: Why should colleagues give credence to this particular representation of the phenomena? The answer lies in the logic underlying the model. The soundness of fundamental views of human nature, organizational requisites, or societal processes provide the basis for judging the reasonableness of the proposed conceptualization.

During the theory-development process, logic replaces data as the basis for evaluation. Theorists must convince others that their propositions make sense if they hope to have an impact on the practice of research. If the theoretical model is a useful guide for research, by definition, all the relationships in the model have not been tested. If all links have been empirically verified, the model is ready for the classroom and is of

little value in the laboratory. The mission of a theory-development journal is to challenge and extend existing knowledge, not simply to rewrite it. Therefore, authors should push back the boundaries of our knowledge by providing compelling and logical justifications for altered views. This requires explaining the Whys underlying the reconstituted Whats and Hows.

Why research is conducted has important implications for the link between theory development and empirical research. Combining the Hows and the Whats produces the typical model, from which testable propositions can be derived. (The primary difference between propositions and hypotheses is that propositions involve concepts, whereas hypotheses require measures.) Technically, these statements (e.g., A is caused by B) can be tested without understanding the Whys underlying the model. However, this tends to lead to empirically, rather than theoretically, dominated discussions of the implications of a study's results. As a field, when we have insufficient understanding of why we collectively started an investigative journey, or what theoretical direction we are following, then our discourse tends to degenerate into heated methodological debates over how fast we are traveling. To avoid vacuous discussions, propositions should be well grounded in the Whys, as well as the Hows and the Whats.

To summarize thus far: What and How describe; only Why explains. What and How provide a framework for interpreting patterns, or discrepancies, in our empirical observations. This is an important distinction because data, whether qualitative or quantitative, characterize; theory supplies the explanation for the characteristics. Therefore, we must make sure that what is passing as good theory includes a plausible, cogent explanation for why we should expect certain relationships in our data. Together these three elements provide the essential ingredients of a simple theory: description and explanation.

An additional comment about the use of prop-

ositions is in order. Not all bona fide theoretical contributions require propositions, and all papers need not follow the same format. However, when the purpose of a paper is to present a new theoretical position or to call into question the fundamental structure of an existing theory, researchable propositions are very useful. They force the author to think about the concrete applications of new or revised thinking, and they increase the likelihood that subsequent research will constitute valid tests of the author's core arguments. If propositions are used, they should be limited to specifying the logically deduced implications for research of a theoretical argument. (Some authors mistakenly use propositions to summarize a body of literature.)

Who, Where, When. These conditions place limitations on the propositions generated from a theoretical model. These temporal and contextual factors set the boundaries of generalizability, and as such constitute the range of the theory. Scholars who study the effects of time and context on people and events keep asking nagging questions like, Would your predictions hold in Japan, with a blue-collar population, or across time periods? Unfortunately, few theorists explicitly focus on the contextual limits of their propositions. In their efforts to understand a social phenomenon they tend to consider it only in familiar surroundings and at one point in time.

Although it is unfair to expect that theorists should be sensitive to all possible boundary constraints, clearly there is value in conducting some simple mental tests of the generalizability of core propositions. For example, theorists should be encouraged to think about whether their theoretical effects vary over time, either because other time-dependent variables are theoretically important or because the theoretical effect is unstable for some reason.

Sensitivity to context is especially important for theories based on experience. According to the contextualist perspective (Gergen, 1982), meaning is derived from context. That is, we understand what is going on by appreciating where and when it is happening. Observations

are embedded and must be understood within a context. Therefore, authors of inductively generated theories have a particular responsibility for discussing limits of generalizability.

Although it is important for theorists to be sensitive to context, the Who, Where, and When of a theory are typically discovered through subsequent tests of the initial, rudimentary theoretical statement (What, How, Why). In the process of testing these ideas in various settings, we discover the inherent limiting conditions. In the absence of this breadth of experimental evidence, we must be realistic regarding the extent of a theorist's foreknowledge of all the possible limitations on a theory's applicability.

What Is a Legitimate. Value-Added Contribution to Theory Development?

Most organizational scholars are not going to generate a new theory from scratch. Instead, they generally work on improving what already exists. In that context, it is often difficult to judge what constitutes enough of a contribution to warrant publication in a theory journal like *AMR*. Nevertheless, the constituent elements of social theories described in the preceding section suggest a set of criteria for making editorial judgments.

What and How. Although, in principle, it is possible to make an important theoretical contribution by simply adding or subtracting factors (Whats) from an existing model, this process seldom satisfies reviewers. The additions or deletions typically proposed are not of sufficient magnitude to substantially alter the core logic of the existing model.

One way to demonstrate the value of a proposed change in a list of factors is to identify how this change affects the accepted relationships between the variables (Hows). Just as a list of variables does not constitute a theory, so the addition of a new variable to an existing list should not be mistaken as a theoretical contribution. Relationships, not lists, are the domain of the-

ory. As Poincare (1983) so aptly noted, "Science is facts, just as houses are made of stone.... But a pile of stones is not a house, and a collection of facts is not necessarily science." Therefore, theoretical insights come from demonstrating how the addition of a new variable significantly alters our understanding of the phenomena by reorganizing our causal maps. For example, the addition of "growth-need strength" to job-design theories transformed extant views and altered research practice (Hackman & Lawler, 1971).

Important changes in a theory's What and How are frequently stimulated by surprising research results. In the process of gathering either quantitative or qualitative data, scholars are often confronted with an inconsistency between their observations and conventional wisdom. Although contrary results are frequently discounted by theorists on the basis of measurement error, ongoing challenges to outmoded thinking about motivation (Organ, 1988) demonstrate that sufficient data can be persuasive.

Why. This is probably the most fruitful, but also the most difficult avenue of theory development. It commonly involves borrowing a perspective from other fields, which encourages altering our metaphors and gestalts in ways that challenge the underlying rationales supporting accepted theories. This profound challenge to our views of human nature, group development, organizational transactions, and so forth, generally precipitates a broad reconceptualization of affected theories.

This aspect of conceptual development is particularly critical, and generally overlooked. Theories often are challenged because their assumptions have been proven unrealistic (generally by work imported from other areas). Although it is just as difficult to build consensus around paradigmatic truth as around empirical fact, nonetheless, recent macro theoretical developments involving ecology and economics demonstrate the salience of this approach (Hannan & Freeman, 1989; Ouchi & Barney, 1986).

Who, When, Where. Generally, it is insuffi-

cient to point out limitations in current conceptions of a theory's range of application. For example, discovering that a mainstream personnel selection model has low predictive validity in a military setting does not by itself constitute a theoretical contribution. In addition, theorists need to understand why this anomaly exists, so that they can revise the How and What of the model to accommodate this new information.

Conversely, applying an old model to a new setting and showing that it works as expected is not instructive by itself. This conclusion has theoretical merit only if something about the new setting suggests the theory shouldn't work under those conditions. In other words, it is preferable to investigate qualitative changes in the boundaries of a theory (applications under qualitatively different conditions), rather than mere quantitative expansions. Two examples of this approach are Maruyama's examination of Western theories of management in the context of Eastern culture (1984) and Whetten's examination of growth-oriented organizational theories under conditions of decline (1980).

The common element in advancing theory development by applying it in new settings is the need for a theoretical feedback loop. Theorists need to learn something new about the theory itself as a result of working with it under different conditions. That is, new applications should improve the tool, not merely reaffirm its utility.

Three broad themes underlie this section: First, proposed improvements addressing only a single element of an existing theory are seldom judged to be sufficient. Therefore, a general rule of thumb is that critiques should focus on multiple elements of the theory. This approach adds the qualities of completeness and thoroughness to theoretical work.

Second, theoretical critiques should marshal compelling evidence. This evidence can be logical (e.g., the theory is not internally consistent), empirical (its predictions are inconsistent with the data accumulated from several studies), or epistemological (its assumptions are invalid-given information from another field).

Third, in general, theoretical critiques should propose remedies or alternatives. Although we can think of classic critiques in the history of science that stood on their own merits, the typical debate in our field is less clear cut. Consequently, critics should share responsibility for crafting improved conceptualizations. Otherwise, it is difficult to know whether the original is indeed inferior, or simply the best we can do in a very complex world.

What Factors Are Considered in Judging Conceptual Papers?

Thus far we have examined the inherent merits of a theoretical argument. In addition, reviewers consider other factors, including clarity of expression, impact on research, timeliness, and relevance.

The following list of seven key questions, roughly in the order of frequency in which they are invoked, summarizes the concerns raised most frequently by our reviewers. These questions cover both the substantive issues discussed in the first two sections as well as several formatting concerns. Together they constitute a summary answer to the broad question, What constitutes a publishable theory paper?

1. *What's new?* Does the paper make a significant, value-added contribution to current thinking? Reviewers are not necessarily looking for totally new theories. However, modifications or extensions of current theories should alter scholars' extant views in important ways. Proposed changes can be calibrated in terms of scope and degree. Scope tends to reflect the level of theorizing (general versus middle level), while degree reflects the radicalness of the proposal. In general, scope (how much of the field is impacted) is less important in determining the merits of a contribution than is degree (how different is this from current thinking).

2. *So what? Will* the theory likely change the practice of organizational science in this area? Are linkages to research evident (either explic-

itly laid out, or easily, reliably deduced)? Does the paper go beyond making token statements about the value of testing or using these ideas? Are solutions proposed for remedying alleged deficiencies in current theories? These questions are less appropriate for the rare, highly conceptual papers aimed at changing the way organizational scholars think, in general. However, the purpose of the standard theoretical paper should be to alter research practice, not simply to tweak a conceptual model in ways that are of little consequence.

3. *Why so?* Are the underlying logic and supporting evidence compelling? Are the author's assumptions explicit? Are the author's views believable? Theory development papers should be built on a foundation of convincing argumentation and grounded in reasonable, explicit views of human nature and organizational practice.

4. *Well done?* Does the paper reflect seasoned thinking, conveying completeness and thoroughness? Are multiple theoretical elements (What, How, Why, When-Where-Who) covered, giving the paper a conceptually well-rounded, rather than a superficial, quality? Do the arguments reflect a broad, current understanding of the subject? If propositions are included, are they used properly? Does the argument have any glaring logical flaws? Does it appear that the author has developed these thoughts over an extended period of time, informed by extensive peer input?

5. *Done well?* Is the paper well written? Does it flow logically? Are the central ideas easily accessed? Is it enjoyable to read? Is the paper long enough to cover the subject but short enough to be interesting? Does the paper's appearance reflect high professional standards? Are the paper's format and content consistent with the specifications in the Notice to Contributors?

6. *Why now?* Is this topic of contemporary interest to scholars in this area? Will it likely advance current discussions, stimulate new discussions, or revitalize old discussions? Review-

ers give low marks to papers they perceive are redundant, unconnected, or antiquated.

7. Who cares? What percentage of academic readers are interested in this topic? A paper may be technically adequate but inherently uninteresting to most of our broad audience. Papers written on topics with narrow appeal are typically held to a higher standard for Criteria 1 and 2; that is, they are expected to make a more significant contribution to current thinking and research practice. In general, even highly specialized papers should be linked to core management or organizational concepts and prob-

lems. Otherwise, they are more appropriate for a discipline-based journal.

In conclusion, the theory-development process and criteria for judging theoretical contributions need to be broadly understood and accepted so that editors and contributors can communicate effectively. Hopefully this brief article will facilitate that process. I urge readers to assist in the further development of frameworks for describing and enhancing these important scholarly activities. Papers on the process of building new and improving current theories are always welcome.

References

- Dubin, R. (1978) Theory development. New York: Free Press.
- Gergin, K. (1982) *Toward transformation in social knowledge*. New York: Springer-Verlag.
- Hackman, J. R., & Lawler, E. E. (1971) Employee reactions to job characteristics. *Journal of Applied Psychology Monograph*, 55, 259-286.
- Hannan, M. T., & Freeman, J. (1989) *Organizational ecology*. Cambridge, MA: Harvard University Press.
- Maruyama, M. (1984) Alternatives, concepts of management: Insights from Asia and Africa. *Asia Pacific Journal of Management*, 1, 100.
- Organ, D. W. (1988) A restatement of the satisfaction-performance hypothesis. *Journal of Management*, 14, 547-558.
- Quichi, W. G., & Barney, J. B. (1986) *Organizational economics*. San Francisco: Jossey-Bass.
- Poincare, J. H. (1903) *La science et l' hypothese* [The science and the hypothesis]. Paris: E. Flammarion.
- Whetten, D. A. (1980) Organizational decline: A neglected topic in organizational science. *Academy of Management Review*, 5, 577-588.

I would like to thank Andrew Van de Ven, Richard Klimoski, Kim Cameron, Huseyin Leblebici, Graham Astley, Jack Brittain, Arthur Bedeian, Donald Hellriegel, and Steven Kerr for their suggestions and comments.