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RESEARCH NOTES AND COMMUNICATIONS

STRATEGY AND THE RESEARCH PROCESS: A COMMENT

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The purpose of this paper is to expand the discussion of the 'state-of-the-science' in strategy research. We critically examine the role that theory plays in strategic research, and describe the principles underlying good theory. From a philosophy of science perspective, we argue that: (1) both inductive and deductive methods are valid ways of generating theory; (2) the falsificationist perspective provides an inadequate model for describing the process of theory testing; and (3) managers, researchers, public policy-makers, the popular press, and the public at large all have important roles to play in the knowledge development process.

In a recent paper in the *Strategic Management Journal*, Montgomery, Wernerfelt, and Balakrishnan (1989) (henceforth referred to as MWB) offer some prescriptions for improving the quality of research in the strategy content area. The purpose of this note is to present an alternative perspective of the issues raised by MWB. In general, we feel that MWB have performed a valuable service to the field by focusing attention on the important issues of theory-building and theory-testing. In re-examining MWB's analysis we emphasize how our propositions provide a different set of recommendations for managers and strategy researchers.

WHAT IS A THEORY ANYWAY?

MWB introduce their paper as an attempt to encourage discussion on 'the state-of-the-science' in strategy research, and assume throughout their arguments that strategy research should strive to become a 'science.' Similarly, much of their paper is concerned with the role of 'theory.' MWB do not clarify what they mean by these terms; perhaps they assume that there is a reasonable degree of unanimity of interpretation of the terms among the community of researchers they address. However, given that there are multiple viewpoints within the philosophy of science literature, we feel that a critical first step in developing the very important questions that their paper and ours address is to specify explicitly our understanding of the nature of 'science' and of the role of 'theory' in scientific development.

In our comment we assume that explanation by law is the essence of science, and that the development of such explanations is the main benefit which strategy researchers seek by becoming 'more scientific.' The role of theory in science is contained in the following consensus definition of 'theory' proposed by Hunt (1983) following Rudner (1966):

> A theory is a *systematically related* set of statements, including some *lawlike generalizations*, that is *empirically testable*. The purpose of theory is to increase scientific understanding through a systematized structure capable of both explaining and predicting phenomena (emphases ours).

This definition incorporates the key elements of the nature of theory as proposed by philosophers of science from different branches of the social sciences as Kaplan (1964)—sociology, Blalock (1969)—statistics, Alderson (1957)—marketing, and Friedman (1953)—economics. We feel therefore that this is a reasonably complete

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specification of the essential criteria and purpose of theory.

With this definition of theory as a starting point we re-examine MWB's propositions. Through this process we suggest some alterations to their propositions as well as develop our own propositions to serve as guideposts for knowledge development in the strategic management area.

THE NATURE OF THEORY-BUILDING

MWB make two normative claims in their Propositions 1 and 2 regarding the preferred method of theory construction, i.e. 'all theory generation should depend on some past observation' and 'all observations should be guided and interpreted through some theory.' Both propositions imply that the reverse can also be true, but are dysfunctional ways of theory-building. MWB seem to argue that theories can be generated in the absence of *any* observations and that data can be interpreted in the absence of *any* theory, i.e. theorizing is possible independent of past experience and data are theoretically neutral. However, a number of philosophers of science (Kaplan, 1964; Churchman, 1971) have shown that these reverse statements *cannot* be true. The mere process of deriving law-like generalizations about a phenomenon involves the experience of the researcher, and all interpretation of data is conducted within the context of a framework imposed by the researchers. Therefore, MWB's first two propositions should be expressed as positive rather than as normative statements, and as such we are in general agreement with them.

However, an interesting issue with regard to the *role* of observations in theory-building remains. Churchman (1971) has pointed out that initial observations have different roles in two methods of theory generation, i.e. the inductive method and the deductive method. The inductive method of the Lockean Inquiring System is 'the process of starting with highly warranted (or well agreed upon) observational statements about specific events and inferring a generalization' (Churchman, 1971: 94). Thus, observations are the very basis of the theory. On the other hand, the deductive mode is 'the process of using a set of assumptions to prove a theorem by some standard set of rules of inference' (Churchman, 1971: 94). In this method the role of initial observations is to provide a basis for speculation about the phenomenon, which is then followed by development of assumptions and the hypothetical model from which generalizations are deduced.

Now, MWB state that their Proposition 1 is 'the basis of empiricism, or what Churchman (1971) calls the Lockean Inquiry System' (p. 190). They therefore seem to imply that all theory generation should be based on the pure inductive method (i.e. the Lockean Inquiring System).

It is useful to view the pure inductive method and the pure deductive method of building theory as representing the extremes of a continuum. In contrast to MWB, we propose that methods falling along all points of the continuum, including the deductive method, represent valid ways to generate theory for strategy researchers. While the inductivist route has had a prime role in the development of strategy theory, there is no clear reason why this must be normatively true. The research question and the phenomenon of interest dictate whether 'more inductively-oriented' techniques with greater emphasis on the role of initial observations, or 'more deductively oriented techniques' are likely to be useful (see Karnani, 1984, for a good example of the productive use of deductive techniques). MWB seem to feel that because operations researchers and economists, who often use deductive techniques, sometimes sacrifice relevance for mathematical elegance, this somehow makes deductive techniques deficient. However, it is not the deductive procedures which underlie the deficiencies in the theories, but rather the failure of these theories to correspond to the norms underlying 'good' theories. These norms are discussed in the next section. Our first proposition is therefore:

Proposition 1: Inductive as well as deductive methods are valid ways of generating theory.

WHAT IS A GOOD THEORY?

MWB's notion of what constitutes 'good theory' is contained in their Proposition 3 and discussion of Assertion 1. Proposition 3 states that 'a theory is better, *ceterus paribus*, (a) if it is refutable and (b) if it is consistent with a body of existing theories' (p. 190). Assertion 1 makes the suggestion that 'well-reasoned' theory should underlie strategy research, and MWB offer a series of examples of 'well-reasoned theory' in their discussion.

We postpone our discussion of the idea that theories are refutable to the next section on testing of theories, since these are intertwined issues. Further, while we agree with many of the substantive conclusions regarding the nature of 'well-reasoned' theory which are implied by the examples following Assertion 1, we feel that the recommendations to researchers based on these examples are incomplete, and their rationale somewhat unclear. In our view the principles of 'well-reasoned' theory should derive directly from a logical examination of the purpose of theory 'to increase scientific understanding through a systematized structure capable of both explaining and predicting phenomena.' These principles are embodied in our Proposition 2.

Proposition 2: Theoretical statements should (a) specify generalized conditional relationships which operate over a wide range of circumstances; (b) be incorporable into the total body of scientific knowledge; (c) be internally consistent.

In discussing this proposition we first explain these principles, and then highlight some practical difficulties of achieving these criteria.

The first part of the proposition expands upon the 'lawlike generalizations' aspect of theorizing. To explain and predict, it is required that we understand the necessary and sufficient conditions which underlie the phenomenon of interest. Thus, theories must contain generalized conditional propositions, defined as statements of 'if–then' relationships which cannot occur by chance. These propositions should identify the forces which are important for a particular phenomenon, and specify the manner of interaction of the forces in influencing the phenomenon. They should also specify the rules governing the type of phenomenon which the theory is expected to explain (typically contained in the 'assumptions' underlying the theory). Only with the specific elucidation of such statements are precise explanations and predictions possible. As an example, consider Panzar and Willig's theoretical statement regarding the phenomenon of multiproduct firms: 'there must be multiproduct firms in competitive equilibrium, where economies of scope are prevalent' (1981: 271). They also point out, following the arguments of Teece (1980), that a key assumption of this theoretical statement is the existence of transactions costs in marketing the services of the quasi-public input which is the underlying source of the economies of scope.

Because of the interdisciplinary and integrative orientation of the field, most phenomena of interest to strategy researchers are highly complex in nature. Therefore, the complete specification of necessary and sufficient conditions, as well as the explicit formulation of assumptions, are extremely difficult tasks. For the most part we deal in partial explanations and the predictive power of our theorizing is limited.

There is an intrinsic tension between the requirement that theoretical statements have 'precise' explanatory and predictive power and that they be applicable to a 'wide' range of circumstances. This issue is of particular concern to strategy researchers, who seek an understanding of strategic behavior at different levels of aggregation (i.e. at the firm level, at the strategic group level, at the industry level, and across industries). Often, predictive precision can be obtained for phenomena at lower levels of aggregation (such as strategic groups) but this may imply sacrificing generalizability (for the operation of the phenomena across industries, for instance).

Do these objections make the task of seeking lawlike generalizations futile? Not at all. We believe that it is a worthwhile endeavor to continue to build mid-range theories, for this can be a useful path to the development of more complete theories. Our view is that a theory is better if it explains a wider range of phenomena while striving to aim for predictive precision. For example, a theory of the relationship between market share and profitability would be 'better' if it isolated the particular circumstances which govern the nature of this relationship at different levels of aggregation.

Parts (b) and (c) of this proposition expand upon the 'systematically related' aspects of theorizing. We propose that there are two kinds of systematic relationships which are important; first, the relationship between theories (referred to in part (b)) and second, the relationships of the elements contained within the theory (referred to in part (c)).

Part (b) of our propositions follows from the reasoning that, in general, isolated lawlike generalizations are less useful for describing and explaining phenomena than are systematically related lawlike generalizations. Our view of incorporating theory into the body of scientific knowledge encompasses not only the pre-existing base of knowledge but also the *potential* base of knowledge. In essence, we propose that a theory should not only have the capability to be integrated into the body of theory already established, but also to suggest and stimulate new scientific enquiry. The usefulness of a theory is in part dependent upon its fruitfulness in generating new research avenues which hold the promise of answering important questions. An example from the field of strategy formulation is Wrigley's (1970) development of the typologies of diversification strategy, which not only provided some answers to existing questions but also laid the foundations of much rich research.

Note that we do not propose that a 'better' theory *must* be 'consistent with a body of existing theories,' as MWB suggest. Their proposition would by implication prevent theories resulting from scientific revolutions (Kuhn, 1962), being categorized as 'better.' Such theories may be unrelated to pre-exisitng theories which have poorer explanatory and predictive power (see Kuhn, p. 157 for some examples), but may have great potential for generating new research directions which are critical to scientific discovery.

Part (c) of our proposition implies that the concepts in each statement of the theory be systematically connected. Even though theory is speculative, it must be subject to rules of construction of formal logic, including careful specification of assumptions. Internal consistency is facilitated by clearly defining the key concepts, explicating their interrelationships, and specifying the relationships between the statements in the theory.

Drawing from Proposition 2, our recommendation to strategy researchers would be to approach their research with commitment to generating scientific knowledge regarding the phenomenon of interest, while following the rules regarding consistency in developing theoretical arguments. Strategy researchers have much to contribute by way of a unique approach to the scientific understanding of business phenomena. However, it is also invaluable to draw upon and link our research to previous theoretical work from the different areas which have examined the phenomena. This is all the more critical since strategy research is by nature integrative across functional disciplines. Concomitantly, it may be counterproductive to suggest (as MWB do) that strategy researchers should be well trained in *one* other discipline to facilitate cross-disciplinary work. While we agree that this may not always be an easy task, we encourage strategy students to become conversant with multiple disciplines of organizational science which attempt to explain the phenomenon under question. Thus, if the issue is to understand the linkage between market share and profitability, it is necessary to understand the antecedent work in marketing and industrial organization economics. We do not suggest that strategy students of the market share–profitability link be fully knowledgeable about the entire fields of marketing and industrial organization economics, but merely those aspects of the fields which assist in developing a better understanding of the phenomenon.

No discussion of the nature of good theorizing can be complete without reference to creativity, which plays a critical role in theory development. A way to stimulate creativity is by re-examining the received tenets of the field towards encouraging the development of new patterns of thinking. To facilitate this type of constructive re-examination, to foster the development of a systematized body of knowledge regarding a phenomenon, and to continuously build their expertise, we encourage strategy researchers to engage in projects with researchers from other functional areas.

FALSIFICATIONISM VERSUS TESTING

Throughout their paper, MWB espouse wholeheartedly an extreme version of the falsificationist view of Karl Popper. This view posits that scientific statements can be falsified by empirical tests, and that such a process can lead researchers closer to the truth (with truth being defined as correspondence with fact). Specifically MWB argue that theories which 'cannot be tested in a way which could lead to falsification ought to be viewed with skepticism' (p. 190). They go on to give an example of a theory which 'stands or falls on the result of a single test' (p. 191).

In concept this view may have some merit; however, it is impractical to put into practice in the context of strategy research. Most present-day philosophers of science do not believe that falsificationism provides a workable methodology for the social sciences (see Suppe, 1974). In order to 'test' a theory, hypotheses (i.e predictive statements which have empirical referents) must be generated and confronted with factual evidence. If the data are found to be inconsistent with the hypotheses, this by itself is not enough to conclude that the underlying theory is false. Instead, it may be that the empirical specification of hypotheses (which necessarily involves operationalization of concepts) does not adequately capture the essence of the theory. Researchers who conduct empirical tests are well aware of the very real difficulties of bridging the gap between the abstract and the empirical. If this extreme falsificationist viewpoint were to be universally adopted, as MWB suggest, there would be an uncomfortably high probability of rejection of adequate theories on invalid grounds. We note that Popper himself rejects the doctrine of 'complete' falsifiability: 'In point of fact, no conclusive disproof of a theory can ever be produced' (1968: 50).

Therefore, inconsistent evidence cannot be said to conclusively 'falsify' a theory. In fact, 'all historically significant theories have agreed with the facts, but only more or less' (Kuhn, 1962: 147). However, such evidence still has a useful role in theory development. Theories begin to be questioned because of the existence of anomalies and counterinstances, which leads to the search for new theories. These new theories in turn have some evidence supporting and other evidence against them. The process of rejection of a theory is always accompanied by qualified acceptance of a competing theory. Theories are not so much refuted in isolation as replaced by better theories. Thus, theories are never 'immediately' falsifiable; however, they may be replaced by better theories and only in this sense are 'ultimately' falsifiable.

In fact, it has been demonstrated that the actual history of scientific advance is rarely in agreement with the Popperian account of falsification. Most useful scientific theories have advanced *in spite* of refutations by empirical data (Anderson, 1984). For example, all the following scientific theories were, at one time or another, in serious danger of drowning in an ocean of anomalies: Copernican astronomy, the theory of oxidation, natural selection, kinetic theory, and continental drift (see Lakatos, 1970). Eventually, these theories survived because of the continued fruitfulness of the theories in generating new lines of scientific enquiry and the development of improved measurement systems which resulted in 'better' interpretation of the data. Thus, the Popperian program of 'conjectures and refutations' finds it difficult to account for the actual growth of scientific knowledge in the face of such historical examples as these (Anderson, 1984).

We agree with MWB that theories are not conclusively verifiable by testing. Data can only provide evidence on whether the particular empirical variables selected to represent the theory have the hypothesized relationship. Thus, if the hypotheses fail to be rejected, the empirical evidence may be said to be consistent with the theory. However, this does not rule out the possibility that the data may still be consistent with other theories. As Friedman points out: 'observed facts are necessarily finite in number; possible hypotheses, infinite' (1953: 9). Further, we once again note that in the field of strategy, as in all the social sciences, data are not neutral; the beliefs contained in the theory will partially shape the way we test the theory and interpret the data. In the face of this restriction, the task of conclusive verification becomes even more difficult.

The skeptic will at this point ask what therefore is the point of testing at all, given that conclusive verification or falsification is difficult. In response, we can only emphasize that the true content of any test is to verify the explanatory power of a set of empirical variables which represent a theory, without any claim for the exclusiveness of the theory. In the context of this role we believe that testing is a useful path for scientists to pursue for two reasons. First, the added weight of empirical evidence helps in the delineation of the range and heterogeneity of facts that the theory takes into account. Second, it helps in delineating the facts that the theory does *not* take into account i.e., anomalies, which are very useful in generating new lines of scientific enquiry.

At this juncture, then, it is not clear what strategy researchers have to gain by adopting a falsificationist perspective. First, such a model does not appear to be one which has guided successful research in other fields of inquiry. Second, such an approach could lead to premature closure, as promising theories are rejected on the basis of a limited number of experiments or surveys. Our approach to theory testing is contained in our Proposition 3.

Proposition 3: Scientific theories of phenomena should generate hypotheses which can be empirically tested. Tests which discriminate between alternative explanations of the phenomenon under question are, ceteris paribus, *better than tests with unspecified null hypotheses.*

This proposition expands upon the second criterion of theory and follows from the basic purpose of theory: to explain and predict, theories must be subject to empirical testing of hypotheses generated by the theory. Hypotheses should be stated using empirical referents which correspond as closely as possible to theoretical concepts and which are intersubjectively certifiable.

While MWB suggest that 'nonstandard operationalizations of variables' (presumably nonstandard with respect to operationalizations used by earlier researchers) constitute a 'troubling issue of craftsmanship' (p. 193), we feel that lack of standardization is not the issue of importance, but rather failure to correspond with the concepts in the theory. In fact, we encourage strategy researchers to actively seek new ways to operationalize concepts to achieve better correspondence between theory and measures. To test for external validity it is necessary to verify a relationship despite substantial variation in methodology. It is therefore important to attempt to obtain equivalent results through an entirely original approach. Finally, given that there are multiple competing explanations for the phenomena of interest to strategy researchers, tests should be designed as far as possible to investigate empirical implications which allow for discrimination among these.

When drawing inferences from research findings, researchers should be careful to specify which of the alternative explanations their findings are consistent with. In drawing implications it is also important for strategy researchers to remain free from biases, i.e. to guard against forming pre-judgments or conclusions prior to the evidence and maintained independently of the evidence (Kaplan, 1964). In many ways these suggestions are counter-parts to careful theory construction; if our recommended criteria for good theory-building are employed, the discipline of employing formal logical techniques will aid in interpreting data accurately.

THE APPLICATION OF SCIENTIFIC KNOWLEDGE

In our view, if strategic management is to become a science it must strive towards 'explaining by law' the phenomena of interest. It is true that the field has traditionally been concerned with generating normative implications for practicing managers. However, since the goal of science is to explain and predict phenomena, the role of positive research must be recognized. Three related issues merit further discussion.

The first issue has to do with the model of science that is appropriate to follow. In our view it is doubtful whether physics, where even ultimate applications often do not constitute a primary factor for directing research efforts, would be the best model. Rather, it must be recognized that strategic management is more an applied discipline (similar, perhaps, to engineering), and therefore concerned with ultimate application of research findings. However, these applications may sometimes take a while to emerge. Therefore, we concur with MWB's argument that direct practical applications should not be required of all papers.

The second issue concerns the identification of the audiences for whom the applications are being generated. Strategy researchers should be encouraged to seek knowledge which is generalizable beyond the confines of their members. To approach the status of a 'science' it is beneficial to examine issues which are valued in the larger community of scholars and practitioners. Strategy research findings may be extremely relevant for public policy-makers, researchers from other disciplines, consultants, the popular press, or the public at large.

The third issue concerns the role of application in the development of the field. MWB provide an interesting discussion of the 'division of labor' among researchers. However, this division may be even broader than MWB suggest. Some philosophers of science suggest that there are two distinct roles necessary for the advancement of knowledge (Manicas and Secord, 1983). It is important for scientists (either 'pure' or applied) to conduct theoretical and empirical research in order to uncover causal structures. Under this model, it is then the role of the technician to apply these decision rules to the situations faced by particular organizations. For instance, a manager or consultant may make strategic plans for a corporation using an analytical framework which is developed from research findings.

Notice that an applied scientist is not equated with a technician. In strategy research, the technician role (attempting to apply generalized knowledge to a specific setting) is perhaps best attempted by managers, consultants, and policy-makers. Under this conception, both applied and 'pure' researchers would devote the majority of their effort toward developing and obtaining evidence for lawlike generalizations. Technicians such as managers would be more concerned with applying these generalizations while simultaneously considering their organizations' history and current operating constraints. Under this framework, managers may not actively think of themselves as participating in the process of science, but they would be playing a crucial role in the development of scientific knowledge. In some sense, then, organizations are the laboratory in which strategy research findings are tested. Cooperation is hindered if the scientist and technician do not have reliable and well-developed channels of communication available. This is an area where *SMJ* has in the past contributed, and should continue to contribute, by providing a vital link between strategic scientists and technicians.

This discussion leads to our fourth proposition:

> *Proposition 4: Both managers and researchers have important roles to play in the knowledge development process. The audience for strategy research includes these two groups, as well as other stakeholders such as public policy-makers, the popular press, and the public at large.*

The application of scientific knowledge to the field is never an easy task. Kaplan (1964) points out potential pitfalls associated with 'validating' scientific knowledge through real-world applications. The application may succeed or fail, but this outcome may be due to reasons external to the theory being applied. The evidence provided by these applications in support of the theory must be carefully assessed in each case (Kaplan, 1964). Multiple perspectives (including managerial perspectives) are necessary to test empirical predictions of scientific theories, and real-world applications are necessary if the ultimate goal of science is to solve human problems.

SUMMARY

In our comment we offer some modifications to the propositions developed by MWB. In particular, we provide more precise definitions of science and theory. Drawing from these definitions we explicate in some detail the principles underlying good theory and propose that both inductive and deductive methods are valid ways of generating theory in strategy research. In addition, we argue that falsificationism is not an appropriate model for guiding research in strategy, for several reasons. Finally, we discuss the role of stakeholder groups other than academic researchers in building theory.

It is important to realize that the theoretical issues raised by MWB are present (though often only implied or assumed) in every empirical article published in the strategy literature. We feel that the strategy field is at an important juncture. Research can continue in a rather unfocused fashion (as well described by MWB), or attempts can be made to coordinate research efforts to conform with certain guidelines. The guidelines we suggest are of necessity somewhat broad. It is important to guard against forming arbitrary rules to guide the research process, because these may in fact hinder the transition to an organized and recognized science. This transition is by nature of a continuous process which is typically protracted and never easy.

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