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RESEARCH NOTES AND COMMUNICATIONS

STRATEGY CONTENT AND THE RESEARCH PROCESS: A CRITIQUE AND COMMENTARY

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The purpose of this paper is to encourage discussion on the 'state-of-the-science' in strategy content research. We present a view of an interactive research process and argue that strategy content research would benefit from (1) more carefully developed theoretical work; (2) more theory-driven data analysis; and (3) less emphasis on the immediate *applicability of results.*

INTRODUCTION

We are concerned that research in the content of strategy is not progressing at the rate that it could be. The purpose of these comments is to give our interpretation of the 'state-of-the-science'; to offer some suggestions for change; and, most importantly, to encourage soul-searching on the research process in the area.

We suspect that readers will differ widely in their response to these comments. Some will strongly support the ideas presented here. Others may strongly disagree. Rather than attempting to present many angles on the topic, we have chosen to present the views we know best, and about which we feel most strongly. Those who see things differently are encouraged to do the same.

Some general prescriptions from the philosophy of science provide a useful starting point. Briefly, we argue that the research process is a continuous expansion of knowledge involving the generation, refutation and application of theories. Since the process never has a well-defined start, and since the three steps complement one another, no one task is more important than any other. Instead, it is imperative that there is a balance and a dialogue between the steps and that each is conducted in a competent manner.

Next, we compare the research process with the actual state of science in strategy content, offering our perspective on where progress has been most rapid, and where progress has been slower. We then offer some specific suggestions for change, directed at researchers and journals in the area.

Before proceeding, we want to clarify that these comments focus on strategy content, research work which addresses the scope of the firm (the combination of markets in which a firm competes) and the ways of competing within individual markets (business-level, competitive strategies). This restricted focus does not necessarily mean that the matters raised here are irrelevant for other areas of strategy research, only that they were not written with those topics in mind.

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AN INTERACTIVE RESEARCH PROCESS

Below, some well-known propositions from the philosophy of science are briefly reviewed. These propositions serve as a backdrop for our specific diagnoses and suggestions in the following sections.

Philosophy in general, and philosophy of science in particular, is characterized by disagreement on almost all issues with which it concerns itself. For example, there are well-publicized disputes over the meaning of truth in the social sciences and the extent to which physical sciences can serve as models for social sciences (see Habermas, 1971; Adorno *et al.*, 1976). We will attempt to sidestep these controversies and argue five propositions which we believe govern good research, and which have received wide support within the scientific community. In presenting these ideas we have had to simplify the issues involved to meet realistic space requirements which prohibit exhaustive discussion. More detailed exposition of these ideas can be found in the primary sources which are referenced, and in Christenson (1973, 1976).

Proposition 1: All theory generation should depend on some past observations.

This proposition is the basic lesson from the downfall of rationalism as developed by Descartes and Leibnitz, the idea that one could reason out the structure of the universe by logic alone, without regard to experience. We theorize only about what has been observed and experienced. This proposition is the basis of empiricism, or what Churchman (1971) calls the Lockean Inquiry System. In strategy content research we may speculate about nonexisting organizational forms, but we only do so because existing forms allow us to conceive of the hypothetical constructs. While this proposition would be more pertinent in a critique of other fields (operations research, and perhaps economics), it is the logical counterpart to the critical result present in Proposition 2.

Proposition 2: All observations should be guided by and interpreted through some theory.

This proposition derives from the falsifications' critique of classical empiricists, who felt that pure observation was sufficient to derive all truths. The intuition behind Proposition 2 is that one's choice of what to observe and what not to observe reflects on *a priori* theory about what variables are believed to be pertinent or important. The shoe size of CEOs or the number of marriages they have had, for example, are not in the PIMS data base. Furthermore, observations gain meaning only when interpreted through a theory. For example, the correlation between industry concentration and firm profitability can be interpreted as causal (a result of collusion) or spurious (e.g. resulting from relationships between share and both other variables). That the same evidence can be interpreted in different ways suggests that it is theories which give meaning to observations. Accordingly, in this view there is no such thing as pure induction—learning from the data alone. (Koopmans, 1947 and Vining, 1949, writing on methodological issues in quantitative economics, provide a particularly interesting debate on this point.) What we observe and infer is guided by what we look for.

Together, Propositions 1 and 2 argue against the views that data 'speak for themselves' and that *a priori* theory—theory arising from no basis in experience—is possible. The challenge at this point is how best to unite theory and experience. To address this issue we present two propositions—one on 'good' theorizing, and one on 'good' testing.

Proposition 3: A theory is better, ceteris paribus, (a) if it is refutable and (b) if it is consistent with a body of existing theories.

The first point is the basic tenet of falsification as presented by Popper (1959), which offers a proposal for demarcating science from metaphysics. Falsificationism, or at least the version attributed to Popper, holds that no theories are conclusively established—there are only refuted and not yet refuted theories. Further, the 'falsifiability' principle contends that theories or 'laws' that cannot be tested in a way which could lead to falsification ought to be viewed with skepticism.

In *Conjectures and Refutations* (1962: 36), Popper offers the following summary:

1. It is easy to obtain confirmations, or verifi-

cations, for nearly every theory—if we look for confirmations.
2. Confirmations should count only if they are the result of risky predictions; that is to say, if unenlightened by the theory in question, we should have expected an event which was incompatible with the theory—an event which would have refuted the theory.

Implicit in these comments is the view that tests of 'risky' theories are better than tests of 'safe' theories. This applies to variable selection, to the expected signs of the variables, and to the functional form of the model. For example, a theory which proposes that

$$X_1 = X_2^a lnX_3 + bX_4, b > 0,$$

is better than a theory which proposes that X_1 depends on some unknown combination of some of the following: X_1, X_2, X_3, . . ., X_{50}. While a theory of the former type stands or falls on the result of a single test, theories of the latter type allow the researcher to try many transformations, specifications, configurations, etc.

The second point has been argued most forcefully by Laudan (1977), who considers the weeding-out of inter-theory conflict a legitimate research activity in itself. Once a sufficiently large body of reasonably consistent theory has been developed, a field has a basis on which to pursue what Kuhn (1962) calls 'normal science'. As long as only small disjointed pieces exist, the community of researchers is generally much less progressive (productive).

Proposition 4: A good test is one that can refute an explicit theory.

This view of testing differentiates between data-driven and theory-driven testing. With reference to the discussion above on what constitutes a good theory, this view maintains that the linkage between a test and theory must be explicit and before-the-fact. Being explicit requires that the statement of theory must be unambiguous. Excessively broad or obscure statements of theory cannot be rigorously tested or disproved. Before-the-fact requires that the hypotheses to be tested, and their foundation in theory, be clearly established prior to data analysis.

With regard to the test itself, issues relating to the appropriateness of the design, and the assumptions and conditions of the models and methods, are critical to the integrity of research. While these issues may appear to be tactical details, they are not. Carelessness in these matters has overturned innumerable empirical studies. The work of Bass and his colleagues (1973, 1975, 1978) on the appropriateness of pooling observations in cross-sectional regressions illustrates this point. These authors demonstrated that, on an erroneous assumption of sample homogeneity, several noted market power studies pooled observations across dissimilar groups of firms. Upon disaggregation, the results only hold true is some subexamples. Hatten and Schendel (1977) demonstrated this point on an intra-industry level in a paper that anticipated the concept of strategic groups.

Proposition 5: The sciences should be undertaken for the sake of ultimate application.

This by now widely accepted point was brought out by the pragmatists such as James and Dewey. Although some people still will claim that we pursue knowledge for its own sake, most philosophers look at the sciences as attempts to find means to achieve various goals. The applications need not be right around the corner, but should be somewhere on the horizon.

This point is very apparent in applied work, yet also is descriptive of more basic research which leads to application through longer periods of time and less direct routing. While applications coming from the latter kind of research may be further in the future, the contributions may also be of a more enduring and fundamental nature.

Propositions 1–5 yield a picture of an interactive research process without a well-defined start or end, in which theory generation, theory refutation, and application develop interdependently. Each researcher is born into this process and works within it to pave the way for others. For the entire process to result in a healthy rate of knowledge accumulation, we need a balance among the three component processes. Too little theory, too much testing, or an overemphasis on immediate application will render the process ineffectual. In particular, if one component is weak, it will be unable to fuel the two others and the entire process will suffer. Beyond that it suggests the benefit of paying scrupulous attention to earlier, related work.

RESEARCH IN STRATEGY CONTENT

As we see it, the road ahead in strategy content research is intricately linked to the issues discussed above. Using Propositions 1–5 as a backdrop, we offer three assertions.

Assertion 1: Well-reasoned theory is instrumental to progress in strategy content research.

In the following pages we make two arguments about the importance of theory in strategy content research. Here, in Assertion 1, we argue that carefully crafted theory can make a direct contribution to the accumulation of knowledge in the field. In Assertion 2 we focus on the relationship between theory and testing.

To illustrate the independent contributions of theory, we begin by discussing three pieces of conceptual work which we believe have made important contributions to strategy research.

1. Richard Caves and Michael Porter's work on mobility barriers (Caves and Porter, 1977) starts from standard economic theory about entry barriers and adds an assumption about firm heterogeneity. This recognition that subgroups exist within an industry is an important departure from standard entry theory and provides a valuable structure for analyzing entry and intra-industry rivalry.
2. Distinctive competence is fundamental in strategic thinking, yet for many years this important concept was poorly understood. There was little systematic knowledge or agreement on what constituted a distinctive competence. Building on the classic work of Posner (1975) and others, Jay Barney (1986) argued that several kinds of strengths *cannot* be sources of distinctive competence. In particular, resources which support distinctive competencies should be rare and such that they cannot be traded in perfect markets.
3. David Teece's work (1980, 1982, 1984) on the scope of the firm gives us a theoretical framework for understanding diversification strategy. Based on the work of Oliver Williamson (1975), Teece developed an efficiency-based theory which demonstrated that economies of scope are not sufficient cause for firm diversification.

Why are these papers exemplary? Three points stand out. First, the authors have a thorough understanding of the base disciplines on which their work builds. Second, the research is well connected with other research (contributes to cumulative progress in the area), and addresses fundamental and important strategy questions. Third, the papers are well crafted in that the conceptual ideas are carefully and specifically developed.

In contrast to the above work, many strategy content publications suffer from serious shortcomings on one or more of these criteria. First, and perhaps most critical, strategy content research is seriously compromised when it incorporates mistreatment of the strong theoretical bases of interfacing disciplines. Such theory is typically developed in carefully crafted steps so as to ensure logical consistency between a set of assumptions and their implications. Small changes in assumptions or parameters can alter dramatically the implications of a model. Research that ignores this point too often applies models outside their appropriate domain (e.g. when results derived under the assumptions of perfect competition are used to make predictions about oligopolistic industries) or incorporates fundamental misunderstandings of the models themselves (e.g. papers that attempt to use the capital asset pricing model but in the process violate the fundamental properties of the model (Wernerfelt, 1985)).

Second, with reference to Laudan's comments on theory development (1977), the strategy content area has been slow to eliminate inter-theory conflict or to build a consistent body of theory relating strategy. In this regard there appears to be a great impulse to redefine variables and to put them into one's own framework of analysis. Hofer and Schendel's description of the area's treatment of the strategy concept (1978: 12–45) well illustrates this point. While as researchers we may prefer to work with our own idiosyncratic models, for the sake of cumulative progress, a more accommodating approach would be better.

Third, while theory sections are rarely absent in strategy papers, a number of procedural problems reflect the field's discomfort with this component of the research process. Recurring problems include: (a) excessively loose statements which are representative of what Popper would call 'riskless' theory; (b) incomplete or partially drawn references to a number of (sometimes conflicting) conceptual arguments; and (c) lengthy reference lists where work is cited but not incorporated meaningfully into a paper's conceptual arguments.

In offering these comments we do not mean to imply that problems of this type are easy to avoid. On the contrary, theoretical work, especially that built on precise foundations in other areas, is very difficult to execute skillfully; at the same time, such extensions often appear to be deceptively straightforward—potentially a dangerous combination of circumstances.

Regardless of the intentions or causes, misapplication and poor craftsmanship in theoretical work is clearly costly to any area of inquiry. As outlined in the previous section, poor theory development hampers the entire research process, challenging the efficacy of testing and the basis for application. In our judgement, theory has become the stepchild of the research process in strategy content work, strangling progress in the area and damaging the field's external reputation.

Assertion 2: Strategy content research progresses when data analysis is well crafted and backed by theory.

Again, we will begin our discussion with examples.

1. Richard Rumelt and Robin Wensley's (1981) work on the correlation between market share and performance made a pivotal contribution to a long stream of work. Based on theories developed by Mancke (1974), Rumelt (1984) and Lippman and Rumelt (1982), the authors specified a model in which two theories can be sharply contrasted: one which holds that the relationship between share and performance is direct, and one which holds that the relationship is spurious. The empirical results sharply rejected the 'direct' hypothesis. Together with the work of Schmalensee (1985) and others, the Rumelt–Wensley paper challenged longstanding beliefs, about the value of market share.
2. Using a unique data base, Marvin Lieberman (1984) tested several competing hypotheses about learning cruves. His finding, that industry output is a more important source of learning than firm output, has many important implications (Lieberman, 1987). In particular, this evidence suggests that, counter to the assumptions of the Boston Consulting Group, learning curve advantages are not proprietary.

Why are these studies notable? To begin, each has a solid theoretical foundation, and each addresses significant problems with a history in the literature. Further, both studies incorporate specific, refutable hypotheses that are well defined in advance of testing and permit the researcher to distinguish among competing theories. The studies are also well crafted in that the variables are reasonably defined, the testing procedures are straightforward and involve a minimum amount of data manipulation, and the assumptions and conditions of the models are consistent with the questions being studied.

Unfortunately, these strengths are often absent in empirical studies of strategy content. As noted in Propositions 3 and 4, good theory is the foundation for good empirical work. Where theory is weak or lacking—or simply safe—the contributions of empirical work are jeopardized. Poorly defined conceptual priors cannot be rigorously tested or disproved. Further, the singling out of a few results from many cannot be accepted as specific confirmations of what were originally vague or open-ended propositions. These problems are particularly characteristic in large-sample statistical studies, although the problems are by no means limited to these studies, nor does all such work suffer in this way.

Secondary issues of craftsmanship are also troubling. Nonstandard operationalizations of variables, idiosyncratic testing procedures, and a lack of care in the interpretation of results serve to compound the problems discussed above.

Assertion 3: In the long run, research in strategy content will generate more useful recommendations if direct managerial applicability is not required of all papers.

As discussed in Proposition 5, we believe that science should be undertaken for the sake of ultimate application. Note that there are two parts of this proposition: first, research is done for the purpose of application; and second, this need not be immediate application. We believe that the strategy field tends to err with respect to the second point. At present there is a very strong bias to force every paper to conclude with a commentary about what the research contributes to current management practice. We disagree with this demand for two reasons: (a) The requirement tends to discourage basic research whose (often high) payoffs are in the more distant future. If we persist with this practice, the field may end up with series of short-term projects and few enduring advancements. (b) The requirement is likely to force inaccurate advice. Researchers pursuing more basic work may be pressured into making premature conclusions prior to the supporting work of more applied scholars. Additionally, many individual pieces of work gain meaning only in the context of a larger pattern of research. In these circumstances drawing implications from a single piece of work is not likely to be fruitful.

In many successful academic disciplines there is a division of labor among researchers. Some develop basic theories, some test them, and some translate them into application. The field of finance provides a good example of this point. While it is beyond dispute that sophisticated financial theory has had a significant influence on practice, it is very rare that a paper in, for instance, *The Journal of Financial Economics* contains a paragraph on managerial implications.

The impact of financial research on practice is particularly interesting when compared with the experience of the strategy field. Long considered to be an area which championed application, strategy research, in our judgement, has had surprisingly little impact on practice. In fact, the influence has tended to flow in the opposite direction, where practice invents, and teaching disseminates.

A prominent exception to this pattern is Michael Porter's very influential work on competitive strategy (1980). Porter's book is an outgrowth of extensive basic research, both theoretical and empirical. In contrast, many ephemeral management texts purport to develop, test, and apply theory in one piece. Given its long roots, and the number of researchers who have participated in the process, the relative richness of Porter's volume is not surprising.

In concluding our discussion of Assertion 3 we want to return to the concept of balance in the research process. We feel that Assertion 3 is justified by the current state of strategy content research. We are arguing for a change in degree, not in kind, for a change where more basic work can coexist with more applied work. We believe such a change ultimately will best serve the field's traditional problem-solving orientation.

The remainder of this paper is an attempt to develop recommendations which might help address the problems and the opportunities described above.

SUGGESTIONS FOR CHANGE

Any change in the field ultimately comes down to the willingness and effort of individual scholars. We therefore begin our comments at the level of the individual researcher.

In our view it is important for all of us to execute or promote more theoretical work. In addition to larger steps we need to place more value on small but meaningful extensions of existing theory. While it may appear that the area should emphasize major theoretical developments, these are likely to be rare and difficult to develop with the necessary attention to detail.

When extending theory it is important to consider the development of a larger body of work. Research which advances the field by displacing or extending earlier work is likely to be more valuable than research which stands in isolation. Note that these points are pleas for more theoretical work as well as pleas for a different kind of theoretical work.

On the empirical side, we need to be much less tolerant of missing, weak, or safe theory specification in our own work and in the work of othes. In line with our earlier comments we need to be more alert to procedural slackness in empirical work—loosely defined variables, unspecified functional forms, mechanical variable selection, and wide-ranging hunts for significance. A more active and public dialogue, including published comments, rejoinders and criticisms, could be a powerful force in this regard.

While application is desirable, and lack of it has caused some areas (economics or operations research, perhaps) to go astray, as argued in Assertion 3, to insist on the immediate applicability of all research is not conducive to progress. Accordingly, we believe that the implicit requirement that strategy papers should conclude with a list of managerial implications should be dropped.

In addition to the efforts exerted by individual scholars, our journals can play a key role in improving the quality of the research process. The journals serve to reward and showcase good work, and also contribute to the quality of all submitted manuscripts through the refereeing process. This means that the selection of editors and reviewers is very important.

It is our contention that the current practice of many journals publishing strategy work, whereby all papers are reviewed by a small editorial board and evaluated by one editor, is dysfunctional. No one can be an expert on all aspects of the strategy field. Accordingly, we believe that these journals would benefit from moving toward a system similar to that of *Management Science*. Under such a system a specialist area editor would be responsible for identifying referees and would have final evaluative authority for a manuscript. This practice should produce a closer alignment between the expertise and interests of editors, referees, and paper content, hopefully leading to better reports.

In addition, the procedure outlined above should allow occasional use of referees from other disciplines (e.g. finance, marketing, sociology). Apart from serving a function similar to that of the cognate member of a dissertation committee, these outsiders could also expose authors and editors to the methodological standards of their professions *prior* to publication. Even if we do not accept all their judgements, this practice should be an eye-opening experience.

Journals face an obvious risk in these proposals. If carried too far, we may be left with a heterogeneous mixture of papers from a number of disciplines. As a result the field may lose its core identity. We have two reactions to this risk. First, the balance of forces is presently very skewed towards 'unitarian' practice and its associated problems. We believe some movement away from this pole would be beneficial. Second, anarchy need not reign in a decentralized process. If implemented cautiously and managed carefully, such a system could greatly enhance the publication process.

The selection and training of Ph.D students is another important topic to consider. This is a place where we as current scholars can transcend our own shortcomings, by attempting to give our intellectual children better training for the rigors of research than we ourselves had.

The breadth of the policy area and its linkages to many other disciplines necessitates a sophisticated grasp of those areas as well as our own. In this situation there is a clear danger of educating people who are amateurs at many things, but professionals at none. While we do not want our students to be economists, sociologists, marketers, or organization theorists, *per se*, we want them to be good enough to draw upon those fields without embarrassment.

It is, in our opinion, not possible to master strategy and yet have a rigorous research-level knowledge of several interfacing disciplines. Therefore, we believe that each strategy student needs to develop some sort of specialization. Each must have breadth enough to teach core policy courses, but each should also have sufficient depth to do research at the juncture with one other discipline. To address this issue, we believe it is necessary to require some degree of doctoral-level specialization on the part of each student. Some schools address this need by administering minor and major preliminary examinations, or by requiring that a certain number of graduate credit hours be taken in an interfacing discipline. While these efforts are in the right direction, the evidence suggests that they need to be strengthened.

Strategy students should take fewer masters-level survey courses and favor more specialized doctoral seminars which provide in-depth exposure to an area's research traditions. A suitable acid test for this experience could be a student's ability to publish part of his/her dissertation in a journal primarily associated with an interfacing area. These points, of course, also apply to those of us well beyond the dissertation stage.

CONCLUSIONS

We have made somewhat radical, but well-meaning, comments about current trends in strategy-content research. As others before us who have raised issues or expressed concerns (Camerer, 1985; Camerer and Fahey, 1988; Leontiades, 1982; Mitroff and Mason, 1982; Schendel and Cool, 1988; Wensley, 1982), we hope that this paper will help in the never-ending process of sorting the wheat from the chaff, in our research and in our discussions.

In conclusion, let us acknowledge that the issues facing the area could, to some extent, be expected. Strategy is a young field; the lack of theory is apparent in many social sciences; and data mining takes place in most spheres of inquiry (Lovell, 1983). However, this does not mean that these circumstances are desirable or unavoidable.

Two particular conditions and a derived reaction have, in our opinion, led the field to its current position. First, strategy began as a capstone course, intended to integrate the functional areas. Thus, the field's domain was very broadly defined, making theory generation difficult. Second, in the 'capstone' conception, the field was given the role of translating the insights from various functional areas into action. This created the impression of an area without an independent body of content. As the strategy field began to develop its own research tradition, it had to struggle with history. Further, a legitimacy question arose which caused the field to move rapidly toward extensive large-sample empirical research, which presumably was beyond reproach. In our opinion the pendulum swung too far in that direction, leaving theory behind.

While advocating that the area should devote more attention to theory, we would like to clarify that we do not endorse an unqualified imitation of the way things are done in other areas, notably economics. It is possible that falsification in that field has been given too low a status relative to theoretical generality, making application to real-life settings difficult if not impossible. Our thesis is that the strategy field could benefit from a more substantial theoretical perspective but, importantly, one that is complemented by empirical testing and translation into practice.

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