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# INTRODUCTION TO THE SPECIAL ISSUE: PERSPECTIVES ON THEORY BUILDING IN STRATEGIC MANAGEMENT

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## TAKING STOCK OF THE STRATEGIC MANAGEMENT FIELD

It is proper that a discipline, whether nascent or mature, should take stock of its progress once in a while, and strategic management is no exception. For example, Camerer (1985) believes that the field is plagued by confusion about its basic concepts and research domains, a view echoed in more recent reviews (*e.g.* Summer *et al.*, 1990) and amplified by concerns about whether strategic management researchers should offer normative viewpoints and recommendations for practitioners (*e.g.* Bettis, 1991). In addition, writers such as Mintzberg (1990) and Montgomery *et al.* (1989) argue that too much historical research in strategy has suffered from an inadequate theory base and sometimes mindless data-mining and 'number crunching'.

However, we take a much more positive view of our conceptual 'fuzziness' than Camerer and others appear inclined to. Lack of conceptual clarity in the strategy field may have been a handicap, or perhaps merely a natural progression, but it has not been so serious as to hinder communication among scholars. Students of strategy have so far managed to communicate through an intuitively shared consensus as to what strategy is, or ought to be, all about. Significant progress was made as long as 15 years ago, at the 1977 University of Pittsburgh conference on strategic management, in identifying and defining the broad contours of the field (Schendel and Hofer, 1979). Further, conferences such as the University of Texas–Arlington International symposium entitled 'Strategic Management Frontiers' (Grant, 1988) and the recent conferences of the Strategic Management Society (Chakravarthy and Doz, 1992; Rumelt *et al.*, 1991) also demonstrate clearly researchers' ongoing attempts to map more precisely the strategy concept and the methodological issues involved in strategic management research.

Therefore, while researchers sometimes quibble about missions, goals, and objectives, it is possible to identify a commonly shared view of strategy (at least in the for-profit sector) as something an organization needs or uses in order to 'win', or establish its 'legitimacy' in a world of competitive rivalry and numerous challenges to managerial autonomy. At a minimum, strategy is what makes a firm unique, a winner, or a survivor. Its presence or use does not ensure viability, but its absence leads to failure. In Rumelt's (1979) terms, strategy is what a firm uses to create and maintain an 'asymmetric' advantage in its product markets.

Another widely-accepted view is that strategy reflects a pattern in a stream of conscious managerial decisions (Miles and Snow, 1978; Mintzberg, 1978) aimed at ensuring organizational adaptation. MacCrimmon (1986) also develops a strategy model called the ends-means-conditions model which incorporates game theoretic concepts. In this model, conditions are envisaged as a mediating influence between ends and means and thus modify the conventional ends/means definitions of strategy (Andrews, 1971; Chandler, 1962). How much greater precision do we need in defining the concept of central interest to the field?

The fields Camerer offers us as models to emulate, particularly economics and psychology, are no less plagued by controversy and schisms than ours, and neither addresses the competitive reality of the method and substance of managerial decisions. For example, how much more precise are economists when they discuss 'innovation' or 'regulation', or psychologists when they talk about 'intelligence'? As Mahoney observes in this issue, the economist Joan Robinson (1956) once noted, 'There is no advantage (and much error) in making definitions of words more precise than the subject matter they refer to'.

## STRATEGIC MANAGEMENT AS THEORETICAL PLURALISM

Rather than urging strategy research to retreat to a state of disciplinary isolation or specialization, it is more sensible to adopt the viewpoint that phenomena studied in strategic management research often can be viewed through more than one lens (Thomas and Carroll, 1992). Some of the more widely drawn-upon perspectives include industrial and organizational economics, organizational behaviour, and psychology. Generally, each perspective can capture a part of a given strategic management phenomenon but, like the parable of three blind men feeling an elephant, an integrated understanding is rarely obtained. Despite the potential benefits of an integrative perspective, there is a need for much progress in synthesizing the various theories. The primary difficulties stem from incompatible assumptions and differences in units of analysis.

If an integrative approach is impossible, as Allison (1971) suggested, there is much to be gained by treating the diverse perspectives as alternative conceptual lenses that may well lead to different inferences from the same data. Researchers who advocate one to the exclusion of others are guilty of dogmatism, which is the last thing we need in the strategic management field. In the spirit of a balanced policy dialogue and theoretical pluralism perspective (Bowman, 1990; Thomas, 1984, 1989) we follow Huff (1981, p. 87) in arguing that 'the culture clash between genuinely different points of view can be an important basis for the development of knowledge.' Such clashes stimulate thought and may help extend existing theories or develop new ones.

In addition to eclectic theoretical efforts, empirical research may be used to support or reject theoretical overlaps. The collection of new data may facilitate inductive reasoning that may help extend and integrate existing theories or develop new ones. Unfortunately, many empiricists avoid multi-method research because it requires more effort. Also, discrepancies across methods often arise. Many analysts wrestle endlessly with such discrepancies under the assumption that failing to find perfect convergence indicates a flaw in the design or an error in the analyses.

While it is often desirable to observe similarities across methods (*e.g.* to support theoretical relationships empirically), it might also be useful to emphasize the dissimilarities across methods to underscore the need for multimethod research. Rather than framing the discrepancies as 'noise' or 'errors', it might be fruitful to frame them as diagnostic information that could further efforts to integrate the various perspectives. This essentially reverses what is often considered as the foreground and the background. Such discrepancies may be the most informative data of all. For example, they may point to factors that sway cognitive and organizational processes away from economic rationality.

Of course there are many other possible causes for such discrepancies, and strategy researchers will have to be clever to tease out these factors. As economists often suggest, 'If it were an easy problem to solve, someone would have done it already.' Yet strategy researchers should not shy away from such problems.

Researchers in neighbouring fields like economics, organizational behaviour, and psychology generally must focus their efforts within their respective fields. Evaluations for tenure and pay within a given department and, to some extent, academic acceptance, tend to be influenced more by publications within journals recognized as relevant by members of that department. Forced into a relatively narrow strategy for research, researchers may be less inclined to expend time and effort to gain exposure to, and publish, in other streams of research.

In contrast, strategic management is the field that claims to integrate these and other streams of research. Its outlets for research, as evidenced by this issue, typically make a very clear and open call for integrative work. By the very nature of the field, researchers in strategic management are routinely exposed to a variety of perspectives. This constant exposure should prime the pump for multilectic (if not truly integrative) thinking and is a potential source of competitive advantage for strategic management that other fields may not possess.

Strategic management is viewed sometimes as an applied area that simply imports theories from a variety of more theoretically-oriented fields (Schendel, 1991, p. 2). Perhaps another metaphor can be developed that better illustrates the potential of strategic management research. Economics can be likened to Newtonian physics in that it studies certain forces impinging on an object (*e.g.* an organization or industry). Organizational behaviour and theory can be likened to chemical engineering in that it focuses on the properties of the objects or the medium being acted upon (*e.g* an organization or industry) and often endeavours to redesign or create novel forms with more desirable properties. Strategic management can be likened to fluid dynamics – the medium and the forces acting upon/within it are inherently intertwined and cannot be meaningfully separated. This alludes to the fundamental inseparability of the content and process sides of strategic management (*i.e.* formulation and implementation).

So, while economic incentives may still be the best single predictor of business transactions, deals and decisions are negotiated and made by people. The conduct within a market may be drawn toward economic rationality, but cognitive and social forces may produce friction inhibiting the move toward economically rational patterns. These forces may even drive the patterns in economically irrational directions. Returning to the metaphor of fluid dynamics, an organization or institution (*e.g.* an industry) is like a medium of varying viscosity that generally flows in a predictable direction (*i.e.* toward economical rationality) but, due to the nature of the medium and its surroundings, it is predisposed to the formation of eddies and backwashes which temporarily alter, impede, and even reverse this flow.

## THE CONTRIBUTION OF PAPERS IN THIS ISSUE

The articles in this issue reflect, in each case, elements of theoretical pluralism in addressing areas of research in the strategy field. We offer here a brief commentary on the important issues in each paper. While each is unique, they are bound together by evidence of theoretical pluralism and such recurring themes as the uncertainty and dynamism of organizational and economic activity; the uniqueness and heterogeneity of circumstance, strategy, resources, and performance; and the use of multiple lenses, perspectives, and bases.

First, Spender's article centring on entrepreneurship emphasizes the subject material of strategy research, not methods. The boundaries of the field are shifting to see how human behaviour underlies theoretical abstractions. Strategic management research is, to date, mostly about allocative entrepreneurship, not creative entrepreneurship, yet creativity is the basic response to the reality of uncertainty. Overcoming uncertainty adds value; this is the creative entrepreneurship that is the purpose of organizations, and strategy is about the management of this creativity. Productive research directions in strategy are generated by expanding the concept of uncertainty. Spender is influenced strongly by Schumpeter and by the traditional domain of leadership themes in strategic management. It is a creative attempt to marry economic and behavioural perspectives (a theme seen in some of the subsequent articles in this issue).

Second, Aharoni views strategy as the ability to define and enact uniqueness, which echoes Spender's emphasis on the value of creative entrepreneurial efforts to address uncertainty. He examines strategy's now-fashionable resource-based view of the firm. His key question is a critical issue for both practitioners and researchers in its focus on why firms differ and the identification/determination of their unique advantages. Strategy research should focus, not on industries, but on firm uniqueness and how to predict success based on those unique actions and characteristics (often called 'core competences', such as Prahalad and Hamel, 1990). To do so, strategy research needs to look more at outliers and place an emphasis on process, politics, technological change, and dynamics, suggesting that Schumpeterian dynamic theory may be more useful as a starting point to strategy than industrial organization.

Third, the article by Bogner and Thomas develops an integrated model of competitive groups by building on the economics-oriented work of strategic groups (McGee and Thomas, 1986a,b) and on the cognitive modelling behavioural stream (Porac and Thomas, 1990). It notes that strategy formulation literature incorporates both economic and behavioural ideas, but that research on competitive groups has developed these as essentially separate streams. Both explain patterns and suggest behaviours. The article looks at divergences and incompatibility between the two in developing an integrative model which draws on organization theory and considers the influence of various forms of change. Finally, it discusses the attractiveness of causal modelling as an alternative approach.

Fourth, the article by Fiegenbaum and Thomas is thematically related to that by Bogner. It makes a rare analysis of firm and strategic group dynamics using an adaptive, process-oriented model of strategy (itself a combination of behavioural and economic models). It demonstrates the strong inertia that surrounds a firm's strategic recipe and which makes it difficult for firms to change. It also suggests, and provides, the linkages for greater firm-level modelling and the cognitive modelling of competition. Finally, it argues for linking individual level theory with group level theory, and proposes a range of theoretical perspectives and analytical techniques which might be fruitfully applied in research on strategic group dynamics.

Fifth, Schoemaker's article develops, and then adds to, an Allisonian view of the world by analysing a series of multiple lenses – unitary rational, organizational, political, and contextual – in a schema which juxtaposes individual goal congruency with co-ordinative efficiency. It suggests that the latter two variables are important in developing meta-theory for strategic decisions. The article urges synthesis and pluralism in the integration of organization and cognition research with that of the economically-rational approach. It emphasizes the contextual perspective which brings to the forefront organizational unpredictability and its underpinnings of complexity and change. This perspective elevates environmental particulars to prominence as the guiding forces behind strategic decisions.

Sixth, Zajac and Olsen distinguish between transaction costs (Williamson, 1985) and transaction value in analysing interorganizational relationships. They identify two key limitations of transaction cost theory – namely, neglect of organizational interdependence and neglect of the processual component of exchange relationships – to look at the creation, maximization, and allocation of value in the interactions between firms. The article argues that organizational interactions seek to raise value, not simply reduce costs. Again, like a number of the other articles in this issue, it seeks to merge behavioural and structural issues in the development of a richer view of interorganizational strategy. Clearly, this type of analysis might be extended both downward into the theory of internal decision and organizing, and upward to the strategic group level of patterns of group action and evolution.

Seventh, Singh provides an extremely thorough review of research on corporate restructuring and notes the diversity in questions asked and theories employed. Drawing on financial and industrial organization literature, he argues that work in restructuring should be extended toward a fuller consideration of processual and field-based issues and highlights the value to theory-building in the restructuring domain of incorporating institutional detail, multiple theories, and eclectic methods.

The eighth and final article, by Mahoney, argues strongly against the logical positivism implicit in much strategy research. A cogent exploration of the dangers and limitations of positivism for strategy, it echoes the views in Schoemaker's article on multiple perspectives. Philosophical and methodological perspectives are incorporated to build an argument for theoretical pluralism and for drawing other fields into the conversation of strategy.

## CONCLUSION

As the articles in this issue illustrate, theories from a number of disciplines (*e.g.* economics, organizational behaviour, psychology) have been used to understand phenomena in strategic management research. Each perspective provides a unique set of insights, but there has not been sufficient success in putting the pieces of the puzzle together.

Firms often do not follow economically rational courses of action even when the economic incentives are relatively obvious, and it is often noted that psychological and socio-political forces probably combine to cause that deviation. In other words, the firm or its decision-makers are following non-economic, or only partly economic, rationality. However, there are few if any attempts to formalize how those factors combine. The details of the interactions must be clearly specified in order to predict when and how firms will deviate from economically rational behaviour.

Similarly, it is unclear how psychological and socio-political forces influence firms when economic rationality does not dictate an obvious course of action. Integrating these factors would make a major contribution to understanding strategic actions in such situations as emergent markets (where industry boundaries and structure are ambiguous), markets with significant technological or socio-political constraints or change, and oligopolies (in which firms typically are faced with conflicting incentives to co-operate and compete).

It is unlikely that interdisciplinary theories will be developed in fields which are relatively more narrowly defined. Interdisciplinary research should be one of the primary distinctive competences of strategic management as a field, and there seems to be sufficient demand for interdisciplinary research within this field to make such ventures possible and fruitful. Further, it is reasonable to assume that neighbouring fields would find it useful to import interdisciplinary theories. Efforts to develop, and to disseminate, such theory are a natural role for strategic management research.

The value to be added by strategic management can be put into business terms by conceiving technology as knowledge structures (Thomas and

McGee, 1989). The licensing of technology can reduce start-up costs (*e.g.* R&D expenditures) for the receiving organization. Technology can be applied directly or combined with other technologies in novel ways within receiving organizations as they develop their own innovations. In an academic environment, exporting interdisciplinary theories to more sharply-defined fields can allow their researchers to expand the boundaries of knowledge with substantially less effort and greater benefit; the technology (knowledge structures) for linking their known body of theories to a wealth of theories in other disciplines can be provided by strategic management researchers. From an entrepreneurial perspective, this represents a remarkable opportunity for strategic management as a field.

Another way of approaching this issue is to compare the development of a scientific field to the economic development of nations. Strategic management as a field may wish to consider the way it chooses to develop in light of the post-war economic development of Japan. That nation made a conscious, organized effort to seek out and import technologies and to use, integrate, experiment with, and improve them. In the process, Japan learned to put technologies to uses more varied and more productive than did their originators and learned to develop its own technology as well. A major criticism of Japan was that it could only copy others' technologies and lacked the capacity for innovation. That broadly-held perception persisted long after facts had proved otherwise. Similarly, strategic management has been labelled as an applied field incapable of significant contributions to theory, yet we believe the field can disprove that belief. Strategic management certainly is by its nature a practitioner-oriented field, yet the continuing effort to generate integrative theory is beginning to provide a more holistic view of firms and industries and their dynamics, thereby not only helping to improve the effectiveness of managers and broadening our own understanding, but providing theoretical material for other fields to explore as well.

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