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PRECIS OF
STRATEGIC MANAGEMENT AND ECONOMICS

Richard P. Rumelt, Dan Schendel, and David J. Teece

The last decade has witnessed a minor revolution in strategic management research and writing. As never before, academics have adopted the language and logic of economics. This change is owed to the increased use of economics by strategy scholars and to the increased ability of economists, armed with new tools and richer theories, to attack problems of central interest to strategic management. Thus, during this past decade we have seen strategy scholars reaching out to use or reformulate economic theory, as in Porter's (1980) influential treatment of industry structure. In the other direction, we have seen some economists positioning their work as relevant to general managers, as in Jensen's (1989) views on corporate control and Williamson's (1975, 1985) analysis of the efficiency properties of the institutions of capitalism.

Although there can be little doubt that economic thinking is reshaping strategic management, opinion is divided as to the usefulness of this trend. Within the strategic management field, there is a growing group who cross over between the fields, but maintain an understanding of their distinct strengths and weaknesses. However, there are also some who see economics as the "solution" to the strategy problem (or, perhaps, to the "tenure" problem), rejecting the field's traditional preoccupation with situational complexity and managerial processes. Finally, there are some who strongly oppose the confluence, seeing economics as "imperialistic," as taking undue credit for formalizing that which was already known by others, and as insensitive to aspects of the human situation other than the rational pursuit of gain. Within economics, the situation is simpler: there are those who follow and appreciate the contributions of strategic management research, but there is a much larger group who are unaware of traditions outside of economics and apprehend business management only through their own constructs (and an occasional reading of the *Wall Street Journal*).

A BRIEF HISTORY OF ECONOMICS WITHIN STRATEGIC MANAGEMENT

Strategic management, often called "policy" or nowadays simply "strategy", is about the direction of organizations, and most often, business firms¹. It includes those subjects which are of primary concern to senior management, or to anyone seeking reasons for the success and failure among organizations. Firms have choices to make if they are to survive. Those which are strategic include: the selection of goals, the choice of products and services to offer; the design and configuration of policies determining how the firm positions itself to compete in product-

¹ We will use a variety of terms interchangeably and assume throughout the reader will interchange them easily as well. Such alternatives as firm/organization/enterprise; product/service; policy/strategy/strategic management; administrative structure/organization structure/management process are examples of terms and concepts we use more or less interchangeably for sake of variety and convenience, and we trust, with no loss of generality.

markets (e.g. competitive strategy); the choice of an appropriate level of scope and diversity; and the design of organization structure, administrative systems and policies used to define and coordinate work. It is a basic proposition of the strategy field that these choices have critical influence on the success or failure of the enterprise, and, that they must be integrated. It is the integration (or reinforcing pattern) among these choices that makes the set a strategy.

Strategic management as a field of inquiry is firmly grounded in practice and exists because of the importance of its subject. The strategic direction of business organizations is at the heart of wealth creation in modern industrial society. The field has not, like political science, grown from ancient roots in philosophy, nor does it, like parts of economics, attract scholars because of the elegance of its theoretical underpinnings. Rather, like medicine or engineering, it exists because it is worthwhile to codify, teach, and expand what is known about the skilled performance of roles and tasks that are a necessary part of our civilization. While its origins lie in practice and codification, its advancement as a field increasingly depends upon building theory that helps explain and predict organizational success and failure. In the sense of expansion, codification, and teaching, theory is necessary, tested theory capable of prediction desirable, and the search and creation of both to better practice, absolutely at the heart of the field. Society is served by efficient, well-adapted organizations and strategic management is concerned with delivering them through the study of their creation, success, and survival, as well as with understanding their failure, its costs, and its lessons.

Strategic management has a rich tradition and long history as a teaching area in business schools, a history virtually as long as that of business schools themselves. Prior to the 1960s, the underlying metaphor of the (teaching) field was that of functional integration. Under this metaphor, the value-added by what was then called "business policy" came from integration of specialized knowledge within broader perspectives.

A new metaphor was introduced in the 1960s, that of "strategy." Strategy was seen as more than just coordination or integration of functions—it embodied the joint selection of the product-market arenas in which the firm would compete, and the key policies defining how it would compete. Strategy was not necessarily a single decision or a primal action, but was a collection of related, reinforcing, resource-allocating decisions and implementing actions.

Where the sixties gave rise to basic concepts, the decade of the 1970s brought their development and application to practice, and in turn gave rise to research in the field as we now know it. The seventies were marked by the rapid expansion² of consulting firms specializing in strategy, the establishment of professional societies, and the advent of journals publishing material on strategy.³ Three forces helped strategy flourish in the 1970s. First, the hostility and instability of the environment of the seventies led to a disenchantment with "planning" and the search for methods of adapting to and taking advantage of the unexpected. The strategy doctrines of the seventies offered an alternative: building and protecting specialized strengths that weather

² It should be noted that The Boston Consulting Group, the first of the firms specializing in strategy, and the firm that spun off many similar firms, was started by Bruce Henderson in the early sixties.

³ Technically, journals specializing in strategy such as this one, began publication in the eighties. However, the agreement to launch the SMJ was made in 1978. The Strategic Management Society started in 1981, but other groups such as the North American Society of Corporate Planners, Division of Business Policy and Planning of the Academy of Management, The Planning College of TIMS, and others can be traced to the seventies.

change and expressing those strengths in new products and services as markets shift. The second important force was the continued expansion and further development of strategy consulting practices based on analytical tools and concepts. The Boston Consulting Group pioneered in this regard, creating the "experience curve" and deriving the "growth-share matrix." The third key force at work was the maturation and predominance of the diversified firm. Top management began to see their corporations as portfolios of business units and their primary responsibility as capital allocation among business units. The new systems that evolved, dubbed "strategic management," forced business managers to define their plans and goals in competitive terms and generated a brisk demand for strategic tools and strategy analysis.

Until the seventies, academic strategy research consisted chiefly of clinical case studies of actual situations, with generalizations sought through induction. Although this style of research continues to play an important role, the seventies saw the rise of a new research style, one based in deductive methods, the falsification philosophy of Popper, and the multivariate statistical methods characteristic of econometrics. Almost simultaneously, three different streams of work were changing the face of the field. Two of these streams were conducted at Harvard, the third at Purdue University. At the Harvard Business School, students of Bruce Scott built on Chandler's (1962) pioneering work and inaugurated a stream of research on diversification and firm performance. At the Harvard Department of Economics, Richard Caves' students began to modify traditional Mason/Bain studies of structure and performance to include differing positions of firms within industries, inaugurating the study of "strategic groups" within industries. Meanwhile, at Purdue University, Dan Schendel, together with his and Arnold Cooper's students, began the so-called "brewing" studies which explored the empirical links between organizational resource choices, interpreted as "strategy," and firm performance.⁴

During the 1980s, owing to the changes noted, the pace of change accelerated; economic thinking moved closer to center stage in strategic management as disciplines were examined for theoretical motivation for the empirical work then building. The most influential contribution of the decade from economics was undoubtedly Porter's *Competitive Strategy* (1980). In a remarkably short time, Porter's applications of mobility barriers, industry analysis, and generic strategies became broadly accepted and used in teaching, consultation, and many research projects.

Whereas Porter's approach to strategy built on the structure-conduct-performance tradition, which studied market power, there was another tradition, associated with the University of Chicago, which saw industry structure as reflecting efficiency outcomes rather than market power. In this tradition differences in performance tend to signal differences in resource endowments. In addition, another new stream of thought began to emphasize the importance of unique, difficult-to-imitate resources in sustaining performance. Within strategic management, these approaches have flowed together and have been dubbed the *resource-based view* of the firm.⁵

⁴ See, Hatten and Schendel (1978), Hatten, Schendel and Cooper (1976), and Schendel and Patten (1978).

⁵ This view was named and defined by Wernerfelt (1984). Additional contributions were made by Teece (1982), Lippman and Rumelt (1982), Rumelt (1984, 1987), Barney (1986), and Teece, Pisano, and Shuen (1990). Grant (1991) reviews the subject and Conner (1991) provides a comprehensive evaluation.

In addition to these broad perspectives developed within the field, during the 1980s strategy scholars dramatically increased their use of economic theory and their sophistication in doing so as the examples that follow indicate. The event-study methods of financial economics were used to investigate strategic and organizational change as well as the strategic fit of acquisitions. New security-market performance measures were applied to old questions of diversification and performance, market share and performance, as well as new areas of inquiry. Transaction-cost viewpoints on scope and integration were adopted and new theories of the efficiency of social bonding advanced. Studies of innovation began to use the language and logic of rents and appropriability, and research in venture capital responded to the agency and adverse selection problems characteristic of that activity. Agency theory perspectives have been used in the study of firm size, diversification, top-management compensation, and growth. The new game-theoretic approach to industrial organization has informed studies of producer reputations, entry and exit, technological change, and the adoption of standards.

WHY ECONOMICS IN STRATEGIC MANAGEMENT?

Why has the "content" side of strategic management come to draw so heavily on economics? The trend cannot have been driven by practice; very few, if any, of the unregulated firms in the U.S. employ microeconomists to analyze strategies or help chart strategic direction. It cannot have been driven by teaching; most strategic management courses continue to rely on cases that are more integrative than analytic. We contend that the infusion of economic thinking has been driven by five forces or events, all connected with the research program of strategic management. They are (1) the need to interpret performance data, (2) the experience curve, (3) the problem of persistent profit, (4) the changing nature of economics, and (5) the changing climate within business schools. Each of the forces or events has shaped the connection between economics and strategic management and each continues to pose practical and intellectual challenges that will shape future developments.

The Need to Interpret Performance Data

In the early 1970s strategy researchers began to look systematically at corporate performance data, particularly return on investment, in attempts to link results to managerial action. Fruhan's (1972) study of the airline industry, Rumelt's (1974) study of diversification strategy, Hatten, Schendel and Cooper's (1976) brewing industry study, Biggadike's (1979) study of entry and diversification, and the PIMS studies were the early examples of this new style of research. The problem implicit in each of these studies was that of interpreting the observed performance differentials. What meaning should be ascribed to performance differences between groups, or to variables that correlate with performance? The need to find an adequate answer to these questions was one of the forces engendering economic thinking among strategy researchers.

The story of the market-share effect provides a good illustration of this dynamic. The empirical association between market share and profitability was first discerned in IO economics research⁶ where the relationship was interpreted as evidence of "market power." Why? Because using the structure-conduct-performance paradigm as the driver, market share represented "struc-

⁶ Imel and Helmberger (1971), Shepherd (1972), and Gale (1972) all address this phenomena. In the marketing literature there were also models proposed and studied that linked market share to profitability, but without much attention paid to the underlying theoretical issues involved.

ture" ("conduct" was implicit) and supernormal returns were interpreted as poor social "performance." Within the strategic management community, the market-share issue was raised by the Boston Consulting Group and sharpened by the PIMS studies, carried out on the first business-level data base available for economic research. The leading role both BCG and PIMS gave to market share helped shaped thought about strategic management in the late 1970s. The viewpoint they espoused saw market-share as an asset that could be "bought" and "sold" for strategic purposes.⁷ BCG advised its clients to "invest" in share in growing industries (where competitive reaction was either absent or dulled) and "harvest" share in declining industries. PIMS researchers and consultants went further and told managers they could increase share, and thus profit, by redefining their markets (i.e., redefine their competitors and presumably their share position).

In 1979, Rumelt and Wensley (1980) began an empirical study using PIMS data that was designed to estimate the "cost" of gaining market share. Their motivation was discomfort with the consultants' advice to gain share in growing markets (or new industries, etc.). The advice seemed to be too much of a "free lunch." Were there really be simple rules of strategy that could always be expected to pay off? Expecting to find the cost of share-gains to be at least their worth in each context, they were quite surprised to find no cost to share-gains. Changes in share and changes in profitability were positively related in every context examined. *It was not possible to interpret this result without extensive forays into economic theory and advanced econometrics.* In the end, they adopted the assumption that share changes were properly "priced" and interpreted their results as implying that the share-profit association was causally spurious. Instead, an unobserved stochastic process (i.e., luck, good management) was jointly driving both share and profitability. Subsequent empirical research has generally supported their view.⁸ The market share issue also stimulated efforts to model competitive equilibria in which share and profitability are associated. Note that most of this work has been carried out within strategic management rather than by economists.⁹

The market share story exemplifies an argument over data analysis and equilibrium which continues in new forms today. Simply stated, equilibrium means that all actors have exploited the opportunities they face. Thus, competitive equilibrium rules out, (by assumption), the possibility that differences in firm wealth can be attributed to differences in freely variable strategy choices, or easily reversible decisions. Instead, observed differences in wealth must be attributed to phenomena that are uncontrollable or unpredictable, e.g., order of entry, non-imitable differences in quality or efficiency, and of course, luck. By making the assumption, the widely-used study of performance versus some parameter or other loses much of its value. For example, if

⁷Their views were also echoed by some economists. Shepherd [1979: 185] claimed that "present market share ... will yield a given profit rate.... The firm can maintain that profit rate. Or it can raise it now, while yielding up some of its market share to other firms. Or it can 'invest' present profits in building up a higher future market share."

⁸ See Jacobson (1990). For an intermediate view, see Boulding and Staelin (1990). Schendel and Patten (1976) as part of the Purdue brewing studies provided a simultaneous view of the search for market share, profitability, and growth.

⁹ Lippman and Rumelt's (1982) theory of uncertain imitability generates this sort of equilibrium as does the differentiated oligopoly modeled by Karnani (1985). Elegant models in which market share "matters" have been developed by Wernerfelt (1984, 1991)

the world is in equilibrium, the fact that growing industries are more profitable does not mean that one should invest in growing industries. Instead, the assumption of equilibrium leads the researcher to presume that the observed profitability is balanced by the expectation of future losses, risk, or is sustained by impediments to entry, or is a reputation-based premium, or is otherwise balanced by unseen scarcity and cost.

Equilibrium assumptions are the cornerstone of most economic thinking and are the most straightforward way of modeling competition. Researchers who eschew equilibrium assumptions risk gross errors in the causal interpretation of data. On the other hand, the risk in adopting an equilibrium assumption is that it may be unwarranted.

An example of an equilibrium assumption of use in strategic management is that of "no rule for riches"—that there can be no general rules for generating wealth. There is no substitute for judgement in deciding whether or not this exclusion should be applied to a particular context (that is, deciding how general is "general"). Interestingly, this equilibrium assumption rationalizes traditional case-based situational analysis that has been the hallmark of strategic management instruction. If there are no general rules for riches, then a strategy based on generally available information and unspecialized resources should be rejected. Opportunities worth undertaking must be rooted in the particulars of the situation. They must flow from special information possessed by the firm or its managers, from the special resources, skills, and market positions that the firm possesses. Viewed in this light, traditional case analysis is a legitimate search for opportunity.

The Experience Curve

During the 1970s the experience curve doctrine, developed by the Boston Consulting Group,¹⁰ was a powerful force within strategic management. Although the idea that some costs followed a learning-by-doing pattern had been around since the 1920s, it was largely ignored by economists because it was a theoretical nuisance; it destroyed the ability of standard models to reach equilibrium. BCG added four critical ingredients: (1) They argued that the pattern applied not just to direct labor, but to all deflated cost elements of value added; this expanded version of the learning curve was called the *experience* curve; (2) they provided convincing data showing experience effects in a broad variety of industries; (3) they argued that experience-based cost reduction was not restricted to the early stages of production, but continued indefinitely;¹¹ and (4) they explored the competitive implications of the experience effect. An example of the latter is BCG's (1970: 29) suggestion that "there is no naturally stable relationship with competitors on any product until some one competitor has a commanding market share of the normal market for that product and until the product's growth slows. Furthermore, under stable conditions, the profitability of each competitor should be a function of his accumulated experience with that product."

¹⁰ See, *Perspective in Experience*, The Boston Consulting Group, 1970.

¹¹This was a critical issue. Scherer's [1970: 74] contemporaneous industrial organization text dismissed the importance of learning-by-doing in mass production industries because "the rate of cost reduction evidently declines as cumulative output rises beyond several thousand units." Interestingly, the second revised edition, published in 1980, abandoned the disclaimer and treated learning-by-doing as an important phenomena, citing BCG, among others.

The experience curve was the first wedge driven in the split that widened between the study of management process and the study of competitive action and market outcomes. In a field which had traditionally seen the firm as embedded within an "environment," the experience curve focused attention on the actions of alert rivals. Most importantly, the logic of the experience curve engendered a taste for a microeconomic style of explanation: For the first time there was a simple, parsimonious account of what competitive advantage was, how it was gained, and where it should be sought. Adding piquancy was the fact that the logic of experience-based competition was not imported from economics, but was instead developed within strategic management and then exported to economics.

The Problem of Persistent Profit

One of the key empirical observations made by traditional strategy case research was that firms within the same industry differ from one another, and that there seems to be an inertia associated with these differences. Some firms simply do better than others, and they do so consistently. Indeed, it is the fact of these differences that was the origin of the strategy concept. In standard neoclassical economics, competition should erode the extra profits earned by successful firms, leaving each firm just enough to pay factor costs. Yet empirical studies show that if you do well today, you tend to do well tomorrow; good results persist.

One of the factors in the 1970s that drove strategy researchers to search for theoretical explanations for persistent performance differences was the enormous success and legitimacy of the capital asset pricing model (CAPM). Developed by financial economists, the CAPM not only had practical usefulness, it gave great strength to the idea that markets were *efficient*. Consequently, an intellectual climate developed in the academy which tended to presume efficiency in all markets, even product-markets, and aggressively challenged assertions to the contrary. The experience curve doctrine provided a partial response to this challenge, but it clearly was not the whole story.

In searching for explanations for enduring success it was natural to reach for relevant economic theory. The most obvious theory was that of industrial organization economics and its various explanations for abnormal returns. Within strategic management, the most prominent effort is Porter's (1980, 1985). Taking the basic ideas of the Mason/Bain structure-conduct-performance paradigm, Porter changed the perspective from that of the industry to that of the firm, and formulated what had been learned from this perspective into a theory of competitive strategy.

A second effort at synthesis is the resource-based view of strategy. This view shifts attention away from product-market barriers to competition, and towards factor-market impediments to resource flows. Identifying abnormal returns as rents to unique resource combinations, rather than market power, this perspective emphasizes the importance of specialized, difficult-to-imitate resources.

In summary, the single most significant impact of economics in strategic management has been to radically alter explanations of success. Where the traditional frameworks had success follow leadership, clarity of purpose, and a general notion of "fit" between the enterprise and its environment, the new framework focused on the impediments to the elimination of abnormal returns.

The Changing Nature of Economics

The economist's neoclassical model of the firm, enshrined in textbooks, was a smoothly running machine in a world without secrets, without frictions or uncertainty, and without a temporal dimension. That such a theory, so obviously divorced from the most elementary conditions of real firms, should continue to be taught in most business schools as the "theory of the firm" is a truly amazing victory of doctrine over reality. This era may, however, finally be coming to an end as the cumulative impact of new insights takes their toll. During the past thirty years, and especially during the last twenty, at least five substantial monkey wrenches have been thrown into what was a smoothly running machine. They are called *uncertainty*, *information asymmetry*, *bounded rationality*, *opportunism*, and *asset specificity*. Each of these phenomena, taken alone, violate crucial axioms in the neoclassical model. In various combinations they are the essential ingredients of new sub-fields within economics. Transaction cost economics rests primarily on the conjunction of bounded rationality, asset specificity, and opportunism. Agency theory rests on the combination of opportunism and information asymmetry. The new game-theoretic industrial organization derives much of its punch from asymmetries in information and/or in the timing of irreversible expenditures (asset specificity). The evolutionary theory of the firm and of technological change rests chiefly on uncertainty and bounded rationality. Each of these new sub-fields has generated insights and research themes that are important to strategic management. Each is briefly treated in turn.

Transaction Cost Economics.

Of all the new subfields of economics, the transactions cost branch of organizational economics has the greatest affinity with strategic management. The links derive, in part, from common interests in organizational form, including a shared concern with the Chandler-Williamson M-form hypothesis. They also derive from a common intellectual style which legitimizes inquiry into the reasons for specific institutional details. The clinical studies conducted by strategy researchers and business historians are grist for the transaction cost mill. A theory which seeks to explain why one particular clause appears in a contract is clearly of great interest to strategic management scholars, who have a definite taste for disaggregation.¹²

Agency Theory

Agency theory concerns the design of incentive agreements and the allocation of decision rights among individuals with conflicting preferences or interests. Although it deals with the employment transaction, agency theory is not compatible with transaction cost theory. Whereas transaction cost economics begins with the assertion that one cannot write enforceable contracts that cover all contingencies, agency theorists make no such presumption, and instead seek the optimal form of such a contract.

Game-Theory and the New IO.

Modern game theory raises deep questions about the nature of rational behavior. The idea that a rational individual is one who maximizes utility in the face of available information is simply not sufficient to generate "sensible" equilibria in many noncooperative games with asymmetric information. To obtain "sensible" equilibria, actors must be assigned beliefs about

¹²See, for example, Joskow's [1988] treatment of price-adjustment clauses in long-term coal contracts.

what others' beliefs will be in the event of irrational acts. Research into the technical and philosophical foundations of game theory has, at present, little to do directly with strategic management, but much to do with the future of economics as the science of "rational" behavior.

Game theory as applied to industrial organization has two basic themes of most interest to strategic management: commitment strategies and reputations. Commitment can be seen as central to strategy. Among the commitment games that have been analyzed are those involving investment in specific assets and excess capacity, research and development with and without spillovers, horizontal merger, and financial structure. Reputations arise in games where a firm or actor can have various "types" and others must form beliefs about which type is the true one.

Evolutionary Economics

There has been a long-standing analogy drawn between biological competition (and resulting evolution) and economic competition, with both fields often pointing towards the other to ground ideas. Making the analogy concrete, however, has largely been the work of Nelson and Winter (1982), who married the concepts of tacit knowledge and routines to the dynamics of Schumpeterian competition. In their framework, firms compete primarily through a struggle to improve or innovate. In this struggle, firms grope towards better methods with only a partial understanding of the causal structure of their own capabilities and of the technological opportunity set.

Because evolutionary economics posits a firm which cannot change its strategy or its structure easily or quickly, the field has a very close affinity to population ecology views in organization theory. Researchers interested in the evolution of populations tend to work in the sociology tradition, while those more interested in the evolution of firm capabilities and technical progress tend to work in the economics tradition.

The Changing Climate Within Business Schools

Business schools have transformed themselves profoundly over the past thirty years. That transformation has moved business schools and their faculty from acting as collectors and transmitters of the best current practice to development and transmission of theoretical understanding of pervasive phenomena and issues surrounding the practice of management, principally the management of complex business firms. This transformation, which occurred for larger reasons, has influenced the strategy field and its connection to economics in important ways. There are several reasons why that transformation has occurred: the impetus of the Ford Foundation and Carnegie Foundation; university hiring and promotion practices, the rise of consulting firms as repositories of best practice, and the relative proximity of economics departments. Without these changes collectively, the field as we know it would be different, and economics involvement in strategy would have been less.

In the late fifties, the so-called Gordon and Howell (1959), and the Pierson (1959) reports were published, both critiquing the business schools of their day. The criticisms were many and the changes they prompted were extensive, but one of the most far-reaching recommendations was that business schools needed to be infused with rigor, methods, and content of basic disciplines: mathematics, economics, sociology, and psychology. This recommendation was avidly followed, with the result that a good many economists, psychologists, and others trained solely in the basic social science disciplines found employment in business schools alongside traditional, professionally-oriented faculty members.

In time, probably longer than anticipated, the discipline based preference in hiring and promotion led to a stronger and stronger presence of discipline based scholars, including economists. Indeed, some newer business schools, and some older ones as well, were organized with the economics departments as part of their faculty. As business schools became more discipline based, their standards for hiring and promotion came into alignment with the social sciences.

These factors led to an increased proportion of business school faculty either trained in economics directly, or importantly influenced by the standards common to discipline based scholars. Unforeseen by Gordon, Howell, and Pierson was the changing character of economics, and other social sciences. Less and less concerned with empiricism, economics became increasingly concerned with working out the internal logic of its theoretical structure and less and less concerned with describing real institutions. This trend continues today, with "advanced" departments of economics offering Ph. D. programs in which price-theory is considered applied and not even covered during the first year of study.

These changes in business schools forced those interested in strategic management to "take sides," and adopt a discipline. Early on, the typical faculty member in strategic management (then called business policy) was recruited from those with experience and high rank in a functional area (e.g., marketing). The switch required was to that of the total enterprise and its general management function. The increased discipline base of business schools made this switch more difficult, and many schools began to hire young faculty and expect them to move up through the ranks on the merit of work done in strategy. To move through the system in this "new" field was especially difficult, as it tended to lack the infrastructure peculiar to promotion needs: patrons, senior faculty who had been through the system; journals, venues for exchange of views. Additionally, it had a case-based tradition of research increasingly shunned by the academy. Consequently, groups interested in general management and strategy began to take either organization theory or economics as their base discipline.

Throughout the 1970s it appeared that organization theory was the discipline of choice for strategy groups. However, this balance was reversed in the 1980s, largely due to the success of Porter's approach to strategy. While some schools and their strategy faculty retained an essentially behaviorally focused group, many others moved to economics based views. Like economics itself, economic-based strategy groups now also differentiate themselves on their commitment to mathematical modeling versus verbal reasoning and their interest in theory versus empiricism. Within the behavioral groups, the split is chiefly between those following organization theory and those taking a managerial process view of strategic management.

From the viewpoint of strategic management we see a danger in these trends. We have advocated a balanced view of the field, perhaps tipped slightly in favor of tests of theoretical constructs by practice and application. If the balance, as it has at some schools, goes too far toward theory or toward a single discipline base such as economics, there is no counterweight from practice and application likely in either research or teaching. Likewise, if the balance tips too far toward managerial process or even best practice, as it has at other schools, there are no theoretical constructions to accumulate and build for the good of the field. Either unbalanced outcome is bad. In our view, balance requires both theory and application, in their fullest and finest representations, in our research, in our teaching, and in our faculty. That such balanced views represented by portfolios of scholars, some at the discipline end, others at the practice end, do not exist, especially at our best schools, is a sad comment on the lack of administrative leadership

and faculty understanding that exists about strategic management, its content, and its challenges. Simon's [1967] description of the problem of running a professional school has special relevance to strategic management:

Organizing a professional school . . . is very much like mixing oil with water. . . . Left to themselves, oil and water will separate again [p. 16]. . . . A professional school administration—the dean and senior faculty—have an unceasing task of fighting the natural increase of entropy, of preventing the system from moving toward the equilibrium it would otherwise seek. When the school is no longer able, by continual activity, to maintain the gradients that differentiate it from the environment, it reaches that equilibrium with the world which is death. In the professional school., "death" means mediocrity and inability to fulfill its special functions [p. 12].

Unfortunately, strategic management is too often inhabited by those who see no need for (or fear?) the balance we advocate.

THE FUTURE OF THE CONNECTION BETWEEN ECONOMICS AND STRATEGIC MANAGEMENT

We believe that strategic management has clearly profited from the infusion of economic thinking. There is no question that the presumption of equilibrium and the specification of alert rivals, rather than an amorphous "environment," has generated valuable new frameworks, new insights, and greatly sharpened thinking among strategy scholars. Nevertheless, it is vital also to recognize that this infusion has come only after the weakening of orthodoxy within economics. For decades economics impeded research into strategy by committing its intellectual capital and influence to static analysis, an almost exclusive focus on price competition, the suppression of entrepreneurship, a too stylized treatment of markets, hyper-rationality assumptions, and the cavalier treatment of know-how. Had orthodoxy weakened sooner, strategy would have had the benefits from useful economic thinking earlier. That orthodoxy weakened was perhaps partially a result of research in strategic management.

The most interesting issue regards the future of the competitive strategy portion of strategic management. It is this subfield which has turned most wholeheartedly towards the use of economic reasoning and models. If the trend continues, does the competitive strategy subject matter have an independent future, or will it become just a branch of applied economics? There are two reasons for concern about this. The first is parochial: The field's most elementary wisdom suggests that competing head on with economics departments in their own domain is a losing strategy. The second has to do with the internal integrity of the field. To split off part of a problem for separate inquiry is to presume its independence from other elements of the problem. Yet the sources of success and failure in firms, and therefore the proper concerns of general management, remains an issue of debate (see, for example, Williamson's argument in this issue). It would be a great loss if the study of competitive strategy became divorced from the other elements of strategic management.

We believe that competitive strategy will remain an integral part of strategic management and that its connection with economics will evolve and take on new forms in the future. We believe that fears of "absorption" will not be realized for these reasons: (1) strategy is not "applied" economics; (2) economists will not learn about business; (3) microeconomics is a collage and apparently cannot provide a coherent integrated theory of the firm or of management; (4) that

which is strategically critical changes over time; and (5) organizational capability, not market exchange, may increasingly assume center stage in strategic management research.

Strategy Is Not Applied Microeconomics

We assert this because it is patently clear that skilled practitioners do not develop or implement business or corporate strategies by "applying" economics or any other discipline. There are economists who argue that this only proves that practitioners are not very skilled after all, but such a response is neither social science, which studies natural order, nor good professionalism, which seeks to solve, rather than ignore, the problems of practitioners. We do not deny that economic analysis may be useful to a strategist, but so may demography, law, social psychology, and an understanding of political trends, as well as an appreciation for product design, process technology, and the physical sciences underlying the business.

Economists Will Not Learn About Business

Economics has a strong doctrinal component that resists displacement. Strategic management, by its nature and audience, is pragmatic. If certain approaches don't shed light on business practices, or if practitioners deny their validity, the proclivity of the strategy field will be, and should be, to reject them. In addition, we believe that economics will not delve very deeply into business practices to generate new theory. This belief is based on judgments about long-term trends in academia. As Simon (1969: 56) commented on academic tastes, "why would anyone in a university stoop to teach or learn about designing machines or planning market strategies when he could concern himself with solid-state physics? The answer has been clear: he usually wouldn't." Having become as mathematical as physics, and more axiomatic, mainstream economics will not learn enough about business and management to challenge strategic management in its domain.

Microeconomics is a Collage

The upshot of all the ferment in economics is that with regard to issues of most concern to strategic management, the neoclassical theory of firm is no longer a contender. However, there is no new "theory of the firm" to replace it. Instead, there are areas of inquiry characterized by the assumptions that are acceptable in building models and by the phenomena to be explained. There is excitement and vitality in the new economics because the range of phenomena that can be explained has been dramatically enlarged. However, there is also confusion over the loss of the old determinism. With the old theory of the firm, everyone knew how to price—you just set marginal revenue equal to marginal cost. But now price can signal quality to customers and price may tell a potential entrant something about the profits to be made. With the old theory of the firm, a topic like "corporate culture" was outside the realm of consideration, and classified with faith healing and voodoo. But now it is clear that there can be many types of social equilibria among the actors within a firm, with the equilibria depending upon sets of beliefs and history, and that these equilibria have radically different efficiency properties. More generally, it used to be that given a technology, the neoclassical theory delivered a prediction about the allocation of resources. But now one has to specify the technology, the information sets of the actors, including their beliefs, and the order of play and one still usually obtains many possible equilibria. The descriptive power of the new economics has been paid for by the loss of determinism.

The limitation of the new microeconomics is that it *explains* rather than *predicts*. That is, it tends to consist of a series of models, each of which has been purposefully engineered to capture

and illustrate a particular phenomena. Consequently, the new microeconomics is essentially a formal language for expressing knowledge elsewhere obtained.

The problem is simply that formal theorizing has collapsed to examples. Consequently, part of the intellectual structure of the new microeconomics is evolving to look more like strategic management. Any scholar working in strategic management must be aware of the traditional economist's normal reaction to most of the work in our field: "The subject is interesting, but there is no tight theory—it looks like a bunch of lists." But the new economics, taken as a whole, is a "bunch of lists." More precisely, it delivers a large number of tightly reasoned submodels, but no strong guidance as to which will be important in a particular situation.

What is Strategic Changes Over Time

What is strategic, changes as time and discovery alter the basis of competition. These changes arise, in part, because of technological, legal, social, and political changes. They also arise because education and research disseminate knowledge, reducing the degree to which a particular issue can be a source of advantage. The rise of Japanese competition, for example, has substantially altered the research agenda for strategy scholars. By contrast, little or no accommodation to such changes is seen in microeconomics. Business school deans like to argue that their research programs, though abstract, constitute the practices of tomorrow. The opposite is closer to the truth. Yesterday's business strategies are the subject of today's research in strategic management (e.g., takeovers and LBOs, Kaizen), and economics is just beginning to theorize about phenomena that developed half a century ago (e.g., separation of ownership and control, the diversified firm, national advantages). Today's strategic issues (e.g., the growth of new "network" empires in Europe and Asia, time-based competition) are only dimly perceived by anyone within the academy.

Advantage May Be Internal

Both theoretical and empirical research into the sources of advantage has begun to point to organizational capabilities, rather than product-market positions or tactics, as the enduring sources of advantage. If this is so, our investigations will increasingly take us into domains where economics is presently at its weakest—inside the firm. There are bids by transaction cost economics and agency theory to become "organization science," and we can expect new and important insights from these fields. However, their comparative advantage is the analysis of individual responses to incentives. If behavior turns on interacting expectations, beliefs and routines, and if diagnosis, problem solving, and the coordination of knowledge rather than effort are central, then economic views of organization will continue to be useful, but also will be only one part of the story.

SUMMARY AND CONCLUSIONS

We have tried to show the relationship between economics and strategic management in this essay. It is more than some admit, and less than some would hope. We have tried to show that economics and strategic management are not the same thing, in research or in practice. We have tried to indicate that it is the new economics that offers the most promise, but it is old economics in the form of industrial organization that has thus far made the greatest contribution. There can be little question that the development of the strategic management field has benefited from the influence of economics, but the influence is not unidirectional either.

Where do we go from here? One trend that has recently emerged and deserves mention is the new attention to internal organization. Strategic management is increasingly concerned with understanding the administrative processes that select and coordinate the firm's activities. The capabilities of the firm, and the asset structures that accumulate, appear central to advantage and success. The assets that matter do not appear purely physical or separable. The conjunction of physical and intangible assets results from innovative managerial choice and action not easily duplicated. About such matters the new economics cited and discussed here, both in the papers, and this essay, are just beginning to have something to say. However, in this new and complex realm, economics will be only one of the logical systems in use. Where organizational relationships turn on exchange and on individual incentives, various economic approaches will have much to say. Where the coordination and accumulation of knowledge is key, and where patterns of belief and attitude are important, other disciplines will have more to say.

Along with the internal turn taken by research, comes increasing concern over dynamic explanation. Game-theory brings a fanatical attention to sequences of action and reaction, history provides stories of challenge and response, innovation is inherently dynamic, and so are the processes whereby skillful managers make sense of and respond to an evolving environment. In the more practice-oriented side of the field there is great interest in time-based competition and in the interplay between product-market strategy and the development of organizational capabilities.

More important than these trends in subject matter is the gradual enlargement of strategic management to include discipline-based scholars who share our interest in understanding the direction of enterprises. Caution in this regard is only reasonable. Strategic management scholars are small in number and struggle to maintain integration amongst frameworks and between theory and practice; most disciplines are populous and tend to compete, rather than cooperate, with other disciplines. Nonetheless, intellectual and social mechanisms must be found to make the very best of the discipline-based scholars welcome in strategic management. Their participation and variety are key to the long-run survival of our field.

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