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## STRATEGIC MANAGEMENT AND ECONOMICS

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*This essay examines the relationship between strategic management and economics. It introduces the special issue on this same topic by providing a guide to the eight papers contained in the special issue, and it offers the guest editors viewpoints on the contributions of each discipline to the other. The essay notes the major contribution from economics has been primarily from the industrial organization literature, with promises of important gains to be made from the 'new' economics as it breaks away from the neoclassical theory of the firm. Contributions from strategic management to economics are noted. Areas for further research utilizing the relationship between strategic management and economics are also indicated.*

### INTRODUCTION

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 strategic management, 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

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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*).

Our purpose for this special issue of the *Strategic Management Journal* is to examine the state of the current connection between strategic management and economics. This examination will be done in two ways. The first way will be in presenting papers from a larger collection of commissioned papers and commentaries that appear elsewhere<sup>1</sup> and which provide particularly salient examples of the intersection of the two fields. The second way is through this editorial essay which, in addition to providing our interpretations of the papers selected for this special issue, is extended to provide our own views about the connection between strategy and economics. These views are intended to challenge both economists and strategists to recognize each others' contributions, limitations, and the opportunities each faces in connecting theory and application. Along the way we hope that new directions and priorities for research will be surfaced for our readers, whether their primary interest is strategy or economics. Some comment will also be provided on the issue: What (and who) should guide the intellectual development of the strategic management field: strategic thinking and strategists, or economic theory and economists?

Our essay is organized in this way: the next section briefly reviews the evolution of the connection between economics and strategic management. The third section addresses important forces that have induced this connection. The fourth section examines the future of strategic management and economics, high-

lighting salient research issues. The fifth section provides a guide to the papers in this issue. Our summary comments close the essay.

## 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 firms.<sup>2</sup> 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, if not all organizations, are in competition, competition for factor inputs, competition for customers, and ultimately, competition for revenues that cover the costs of their chosen manner of surviving. 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-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

<sup>1</sup> The larger collection of papers appears in *Fundamental Issues in Strategy*, Richard P. Rumelt, Dan Schendel, and David J. Teece, (Eds.), Harvard Business School Press, 1992. This book contains the papers in this special issue, in some cases in extended form. This companion volume extends the discussion of strategic management and economics presented in this special issue, broadens the scope to include other social science disciplines, and provides a wider discussion of research issues facing the field.

<sup>2</sup> 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.

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. The perspectives were dual: that of the firm as a whole, including its performance, and that of the role of the general manager. Together with an intellectual style that stresses pragmatic realism over abstraction, these perspectives remain at the center of the field and distinguish it from other fields with different perspectives, but with similar interests in the same core issues.

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. Depending upon whether one read Selznick's *Leadership in Administration* (1957), Chandler's *Strategy and Structure* (1962), Andrew's material in *Business Policy: Text and Cases* (1965), or Ansoff's *Corporate Strategy* (1965), a company's mission or strategy built upon 'distinctive competence,' constituted the firm's method of expansion, involved a

balanced consideration of the firm's 'strengths and weakness,' and defined its use of 'synergy and competitive advantage' to develop new markets and new products. Ever since the sixties, the strategy metaphor has survived as a central construct of the field, even without the careful definition necessary for research purposes.

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 expansion<sup>3</sup> of consulting firms specializing in strategy, the establishment of professional societies, and the advent of journals publishing material on strategy.<sup>4</sup> 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 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.

<sup>3</sup> 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.

<sup>4</sup> 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.

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.<sup>5</sup> This work demonstrated for the first time the existence of structural heterogeneity within industries, and led to the first hard empirical evidence of the 'strategic groups' under discussion and development at Harvard. More important than the content of the Purdue and Harvard studies, however, was the different empirical nature of the work. In addition to cases used for induction, this new work used difficult data collection, and the rapidly growing power of the computer and multivariate statistical methods capable of handling large data bases, to test hypotheses in a deductive style of research.

This shift in research style ultimately led to questions that case research and simple hypothesis testing could not illuminate. Results were difficult to interpret, lacking any theory in which to embed them. Cumulation of questions occurred, but not of results that led to advice for practitioners, or to tests of theory useful for practice. Hence, the work of the seventies was instrumental in motivating the work of the

eighties and its search for linkages to theory. As importantly, this work and style led to a new generation of researchers better equipped to handle the new style of research and its intellectual demands.

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.<sup>6</sup>

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

<sup>5</sup> See Hatten and Schendel (1977), Hatten, Schendel, and Cooper (1978), and Schendel and Patton (1978).

<sup>6</sup> This view was named and defined by Wernerfelt (1984a). 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.



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.

In looking backward over these three decades, what comes into focus is the search for theoretical explanations of very complex phenomena. A linking occurred for the first time between basic disciplines of the social sciences,<sup>7</sup> especially economics, and practical issues involved in managing the firm. What had begun in the sixties as rather simple concepts that gave insight into phenomena described in cases, ended in the eighties motivating a search for theory with causal and predictive power able to be used in practice.

## 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 (1978) 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 research<sup>8</sup> where the relationship was interpreted as evidence of 'market power.' Why? Because using the structure-conduct-performance paradigm as the driver, market share represented 'structure' ('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 (BCG) 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 shape 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

<sup>7</sup> It should be understood that this special issue of the SMJ focuses on economics and strategy, but theoretical contributions were also forthcoming from other basic disciplines such as psychology, sociology, political science, and anthropology. Indeed, the theoretical linkage search was conducted on a broad scale and was by no means limited to economics alone.

<sup>8</sup> 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.

purposes.<sup>9</sup> 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 (1981) 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 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.<sup>10</sup> 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.<sup>11</sup>

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, for example order of entry, nonimitable differences in quality or efficiency, and of course, luck. By making the assumption, the widely-used study of performance vs. some parameter or other loses much of its value. For example, if 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. Observed differences in performance may actually reflect widespread ignorance about the phenomena being studied. Which risk is being undertaken is not just a matter of preference, but more likely one of conditions, especially in the presence of innovation and change. The general approach to making this judgement is to rule in favor of equilibrium when the underlying assets or positions are frequently traded or contested, when the level of aggregation and the type of data is familiar to actors in the industry, when the data is widely available and frequently reviewed, and when the connections between the data and profits are widely understood.

While equilibrium assumptions often drive out consideration of innovation, change, and heterogeneity, this does not need to be the case. In the neoclassical world, equilibrium meant that profits were everywhere zero, or more generally, that all opportunities had been exploited. But

<sup>9</sup> Their 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.'

<sup>10</sup> See Jacobson (1990). For an intermediate view, see Boulding and Staelin (1990). Schendel and Patton (1978) as part of the Purdue brewing studies provided a simultaneous view of the search for market share, profitability, and growth.

<sup>11</sup> 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 (1984b, 1991).

more sophisticated views now permit more sophisticated equilibria. The basic idea of Nash equilibrium, wherein each actor does the best he or she can with what they individually know and control, especially when coupled with uncertainty, asymmetric information and unequal resource endowments, permits a broad range of intriguing outcomes. For example, one could model the gradual imitation of an innovation as a process in which competitors observe its operation and market results, and then gradually learn what the leader already knows. In such a model large profits would be earned, firms might enter and then exit, and competition would gradually increase. Although the product market would not be in neoclassical equilibrium, the behavior described is still equilibrium behavior in that no one has passed up any opportunities for profit that are *known to them*. Thus, it is possible to describe many aspects of innovation and the profit-creating transient responses they induce with 'equilibrium' models, although a plethora of nonneoclassical assumptions will be required.

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. What is worth recognizing is that the acceptance of this level of economic equilibrium does not nullify strategic management, nor does it imply that one should teach economic theorems rather than management. What it does imply is that professional educators are right in their focus on developing skills in the analysis of the particular rather than the general. In addition, it suggests a framework for

where to look for opportunities and, once identified, a basis for judging their relative merits. Theory alone is insufficient absent intimate, unique knowledge of technical conditions and the ability to position assets and skills to create favorable competitive positions.

### The experience curve

During the 1970s the experience curve doctrine, developed by the Boston Consulting Group,<sup>12</sup> 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;<sup>13</sup> and (4) they explored the competitive implications of the experience effect. An example of the latter is BCG's 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.' (1970: 29)

The idea that cumulative production experience, not scale, could be a primary driver of unit costs implied a value in doing business apart from the immediate profits earned. In the second-half of the seventies, virtually every article, book,

<sup>12</sup> See, *Perspectives on Experience*, The Boston Consulting Group, 1970.

<sup>13</sup> This 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.



and presentation on strategy referred in some way to the experience curve. The idea's power was that it provided an explanation for the sustained dominance of leaders, and for heterogeneity, despite competition. There was also the simple fact that it supported many managers' tastes for pursuing dominance and growth at the expense of current profit.

The impact of the experience curve on the strategic management community extended beyond the overt content or correctness of the doctrine. 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. Finally, among those who sought more precision, there was the need to clarify assumptions about competitive behavior, to more exactly characterize the resulting equilibria,<sup>14</sup> and to empirically estimate the relative importance of scale, industry-experience, and firm-experience effects.<sup>15</sup> Thus, the very act of developing and grappling with the logic of experience-based competition encouraged economic thinking within strategic management.

### 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. Traditional entry-barrier theory yielded the concepts of scale economies and sunk costs; mobility barrier theory stressed the importance of learning and first-mover advantages in making specialized investments in positions within industries. The 'Chicago' tradition supported the notion that high profits were returns to specialized, high-quality resources. Game theory provided models of firms which use preemption, brand crowding, dynamic limit-pricing, signaling, and reputations for toughness to strategically protect market positions. The economics of innovation brought a focus on Schumpeterian competition, intellectual property, and the costs of technology transfer. And evolutionary economics yielded the idea that skills, embedded in organizational routines, resisted imitation and had to be developed anew by each firm.

Within strategic management there has been a great deal of work aimed at synthesizing these ideas into coherent frameworks. The most prominent effort is Porter's (1980, 1985). Taking

<sup>14</sup> Experience-based equilibria are analytically intractable. Spence's [1974] work remains the best analysis, accomplished by ignoring discounting.

<sup>15</sup> Lieberman [1984], studying 37 chemical products, found learning effects much larger than scale economies and showed that they were associated with cumulative output rather than calendar time.

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. Porter catalogued, described, and discussed a wide range of phenomena which interfered with free competition, thus allowing abnormal returns, and suggested how their interaction and relative importance varied across contexts. Porter's (1985) later approach, delineated in *Competitive Advantage*, extended the earlier analysis of competitive strategy to encompass positioning within an industry (or strategic group) so as to achieve sustained competitive advantage. Positing two basic types of firm-specific advantage (cost-based or differentiation-based), Porter argued that advantage could be sustained from a product-market position and a configuration of internal activities that were mutually reinforcing (i.e. strong complementarities amongst activities and the conditions of demand).<sup>16</sup>

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. The creation of such resources is seen as entrepreneurship: strategic management consists of properly identifying the existence and quality of resources, and in building product-market positions and contractual arrangements that most effectively utilize, maintain, and extend these resources. This perspective finds its greatest use in examining heterogeneity within industries, and in the discussion of 'relatedness' among diversified businesses. Nelson (this issue) discusses a recent version of this viewpoint, incorporating learning, that is called the 'dynamic capabilities' approach. Prahalad and Hamel's [1990] recent discussion of core competences is an expression of the resource-based view.

Ghemawat [1991] provides a new attempt at synthesis around the idea of *commitment*. His

view is that the persistence of strategies and of performance both stem from mechanisms which link and bind actions over time. He identifies lock-in, lock-out, lags, and inertia as the key irreversibilities at work and reinterprets a great deal of strategic doctrine in terms of the selection and management of commitments.

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. Depending upon the framework employed, success is now seen as sustained by mobility barriers, entry barriers, market preemption, asset specificity, learning, ambiguity, tacit knowledge, nonimitable resources and skills, the sharing of core competences, and commitment. That 'fit' was correlated with success can be argued, that it is causal cannot be. The fit argument lies in the long line of work that Porter (this issue) describes as a continuing search for causal explanation. Teaching frameworks that suggest the importance of fit are correct so far as they go, but it is the new economic frameworks which establish the causal linkages. That has been learned from the pressure of asking questions from an economics perspective.

### 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 take their toll. During the past 30 years, and especially during the last 20 years, 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,

<sup>16</sup> This argument can be couched in strict equilibrium terms by introducing strategy-specific assets or other sources of first-mover advantage.

violate crucial axioms in the neoclassical model. In various combinations they are the essential ingredients of new subfields 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 subfields 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.<sup>17</sup>

For many economists, the assumption of unlimitedly rational actors is the defining characteristic of their field. Consequently, transaction cost economics, which follows Simon in positing bounded rationality, has had a difficult uphill struggle for recognition and acceptance. The subfield got its start in the mid-1970s as some economists, building on Coase's (1937) seminal work, began to systematically probe questions of firm boundaries and internal organization. Williamson (1975) was the chief architect of a framework that explored the limits or boundaries of both markets and business firms as arrange-

ments for conducting economic activity. His basic point was that transactions should take place in that regime which best economizes on the costs imposed by bounded rationality and opportunism. This framework was explicitly comparative (the relative efficiencies of markets and hierarchies were exposed) and enabled economists for the first time to say something about the *efficiency* properties of different organizational forms. (Previously economists had commonly sought and found monopoly explanations for complex forms of business organization; efficiency explanations were ignored or denigrated.) In addition to comparing markets and hierarchies, transaction cost researchers also began to look at questions of internal structure and the manner in which specific decisions and actions were taken. In particular, the Chandler–Williamson M-form hypothesis raised important issues relating to corporate control. These ideas began to achieve wider acceptance after being supported in a number of empirical studies.<sup>18</sup>

Within strategic management, transaction cost economics is the ground where economic thinking, strategy, and organizational theory meet. Because of its focus on institutional detail, rather than mathematical display, it has a broader audience among noneconomists than other branches of organizational economics. During the 1980s, a considerable amount of work was done in applying the transaction cost framework to issues in organizational structure. In particular, research has been carried out on vertical supply arrangements in a number of industries,<sup>19</sup> the structure of multinational firms (Buckley and Casson, 1976; Teece, 1981; Kogut, 1988), sales force organization (Anderson and Schmittlein, 1984), joint ventures (Hennart, 1988; Pisano, 1990), and franchising (Klein, 1980). Williamson [this issue] provides a useful review of additional applications of interest to strategic management.

<sup>17</sup> See, for example, Joskow's [1988] treatment of price-adjustment clauses in long-term coal contracts.

<sup>18</sup> Armour and Teece [1978] demonstrated returns to the adoption of the M-form as well as showing eventual dissipation of excess returns through imitation; Monteverde and Teece [1982] established that specific assets affected the vertical structure of organizations.

<sup>19</sup> Early contributions were Monteverde and Teece's [1982] study of auto components and Masten's [1988] study of aerospace.

### 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.

Agency theory has developed in two branches. The *principal-agent* literature is chiefly concerned with the design of optimal incentive contracts between principals and their employees or agents. Principal-agent economics is largely mathematical in form and relatively inaccessible to those who have not made investments in its special technology. The standard problem has the agent shirking unless rewards can be properly conditioned on informative signals about effort. The interesting aspect of the problem is that both parties suffer if good measures are not available. A version of the problem that links with strategic management concerns project selection and the design of incentives so that agents will not distort the capital budgeting process.

The second *corporate control* branch of the agency literature is less technical and is concerned with the design of the financial claims and overall governance structure of the firm. It is this branch which is most significant to strategic management. The corporate control hypothesis most familiar to strategic management is Jensen's (1986) 'free cash flow' theory of leverage and takeovers. According to Jensen, in many firms, managers have inappropriately directed free cash flow towards wasteful investments or uses. Two cures to this problem have been proposed: use of high levels of debt to commit managements to payouts and hostile takeovers, which put new management teams in place. What should strike strategic management scholars is that BCG offered precisely this diagnosis for many diversified firms in the early 1970s. According to BCG, most firms mismanaged their portfolios, misusing the funds generated by mature cash-rich businesses ('cows'), usually by continuing to reinvest long after growth opportunities had evaporated.

The corporate control perspective provides a

valuable framework for strategic management research. By recognizing the existence of 'bad' management, identifying remedial instruments, and emphasizing the importance of proper incentive arrangements, it takes a more normative stand than most other subfields of economics. However, scholars working in this area also have the tendency to see all managerial problems as due to incorrect incentives—a tautology for a perspective which assumes away any other sources of dysfunction (e.g. capital markets problems like those discussed by Shleifer and Vishny in this issue, managerial beliefs about cause and effect, management skills in coordination, and the presence or lack of character and self-control).

### Game-theory and the new IO

Three of the papers in this special issue deal with implications of game theory for strategic management, so our remarks here will be brief. Mathematical game theory was invented by von Neumann and Morgenstern (1944) and Nash (1950). However, little progress was made in developing economic applications until the late 1970s. It was probably Spence's (1974) work on market signalling that sparked the modern interest of economists and it was Stanford's 'gang of four,' Kreps, Milgrom, Roberts and Wilson, (1982), who codified the treatment of sequential games with imperfect information.<sup>20</sup>

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 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

<sup>20</sup> Much of the technical foundation they used had been laid by Selten [1965] and Harsanyi [1967].



and reputations. Commitment, as Ghemawat (1991) emphasizes, 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. Thus, for example, a customer's belief (probability) that a seller is of the 'honest' type constitutes the seller's reputation and that reputation can be lost if the seller behaves so as to change the customer's beliefs. Reputations can also describe relationships within the firm, and the collection of employee beliefs and reputations can be called its 'culture.' Given the competitive importance of external reputations, the efficiency properties of internal reputations, and the relative silence of game theorists about how various equilibria are actually achieved, there is clearly much room for contributions, including those from strategic management research.

#### *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. Key to their view is the idea that organizational capacities are based on routines which are not explicitly comprehended, but which are developed and bettered with repetition and practice. This micro-link to learning-by-doing means that the current capability of the firm is a function of history, making it impossible to simply copy best practice even when it is observed.

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. Both frameworks challenge the naive view that firms can change strategies easily, or that such changes will even matter when attempted and made.

#### *The changing climate within business schools*

Business schools have transformed themselves profoundly over the past 30 years. Business schools and their faculty have moved from collecting and transmitting best current practice to developing and communicating theoretical understandings of phenomena connected with 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. The traditional faculty found its scholarship in studying business firms, identifying the best practice they could find, and transmitting what



they learned in the classroom, typically through a case, and the occasional published article. Along the way such faculties were frequently cast in the role of consultants to practicing business managers and many found greater financial reward in such work than they did from their scholarship alone. The new, discipline-based faculty on the other hand found their scholarship inside the academy, in the writings of others similarly placed, and in advancing the theory of their field, often without resort to practice and application of what they learned. Their minds and rewards were concentrated on what they produced inside the academy. Set in motion was a process that retired practice-based scholars in favor of discipline-based ones.

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. The primary measure of excellence became publication in discipline-based journals and acceptance by the community of discipline-based scholars, rather than relevance to practice or contributions to professional education. Discipline-based scholars not only earned internal rewards more easily, they also typically lacked the cushion of consultation that would otherwise allow a greater adaptation to the special circumstances of professional schools. This self reinforcing cycle is still present today.

Throughout most of this period very high growth rates characterized business schools, as they moved from granting about 12,000 to over 70,000 MBA degrees per year, and to many more schools offering the MBA. Well-trained faculties in specialty areas such as marketing, finance, accounting, and other functions were in short supply, especially in the earlier years of greatest growth. To fuel expansion it was a short step to hire disciplined based faculties directly, and worry about their adaptation to applications in business firms later. Some made the transition, some did not, but many who did retained an allegiance to their base disciplines that included seeking publication reputations, not in the field in which they were to profess, but in the

basic discipline in which the faculty member had been trained.

In the world of business, more and more large firms began to create their own management development programs, aimed at filling the gap between the increasingly theoretical MBA education and the needs of practice. In addition, consulting firms grew in scope and sophistication. In many functional areas, including management and strategy, specialist consulting firms replaced business schools as repositories of best practice.

These factors led to an increased proportion of business school faculties 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 faculties 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 faculties 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 vs. verbal reasoning and their interest in theory vs. empiricism. Within the behavioral groups, the split is chiefly between those following organization theory and those taking a managerial process view of strategic management.

Which group has the better idea? Who will dominate? That remains to be seen, but if what the top research-oriented (i.e. Stanford, Northwestern, Chicago, Berkeley, etc.) schools are doing now is any guide you have to bet on those emphasizing contribution of economics, if not total reliance on economists. If, on the other hand, the top European schools or practice is your guide, if what managers listen to makes a difference, those who combine a modicum of economics with a focus on managerial process are clear winners. No matter what you believe will be the outcome of this contest, economics has clearly infused and informed strategic management, not only by the power of its theory to yield insights, but by the transformation of the business school host, and the evolution of strategic management as a field.

However, from the viewpoint of strategic management we see a danger in these trends. We advocate 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 (inhibited?) by those who see no need for (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.

Economics has been chiefly concerned with the performance of markets in the allocation and coordination of resources. By contrast, strategic management is about coordination and resource allocation *inside the firm*. This distinction is crucial and explains why so much of economics is not readily applicable to the study of strategy, and why strategy can inform economics as much as economics can inform strategy. Twenty-five years ago economists, asked how a firm should be managed, would have (and did) argue that subunits should be measured on profit, they should transfer products, services, and capital to one another at marginal cost, and the more internal competition the better. Today, we know that this advice, to run a firm as if it were a set of markets, is ill-founded. Firms replace markets when *nonmarket* means of coordination and commitment are superior. Splendid progress has been made in defining the efficient boundaries of firms—where markets fail and hierarchies are superior—but there are limits to building a theory of management and strategy around market failures. It is up to strategy scholars to flesh out the inverse approach, supplying a coherent theory of effective internal coordination and resource allocation, of entrepreneurship and technical progress, so that markets can be identified as beginning where organizations fail.

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, remain 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 expressed 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. Part of any competitive strategy can be tested against known economic theory and models of competitive reaction; but most business strategies also contain implicit hypotheses concerning organizational behavior, political behavior, technological relationships and trends, and rely on judgements about the perceptions, feelings, and beliefs of customers, suppliers, employees, and competitors. Competitive strategy is integrative—not just because it integrates business functions and helps create patterns of consistent, reinforcing decisions, but also because creating and evaluating business strategies requires insights and judgements based on a broad variety of knowledge bases.

### **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 do not 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 judgements 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. Thus, for example, as industrial organization increasingly becomes infatuated with formal modeling (it didn't until the mid-1970s), it may lose the rich empirical base that made it possible for the Mason/Bain tradition to undergird Porter's work. Put differently, industrial organization may have already made its important contributions to strategy.

An example may help illustrate the very real gap between theory, economic or otherwise, and the need to internalize a vast amount of information pertaining to business practice. A case instructor used to ask 'What are this company's strengths?' Economic reasoning has now helped us understand that what we may mean to ask is 'What firm-specific, nonimitable resources or sustainable market positions are presently under-utilized?' The restatement helps: it is more precise, it provides a definition of 'strength,' and it defends against critics who insist on a discipline base behind university education. But are economists better equipped to answer the question? We suspect not. It is probably much easier to teach these economic concepts to a generalist than it is to teach economists about business.

### 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 the 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. Models have been constructed to examine markets with consumer loyalty, experience effects, producer reputations, complex signaling games, the strategic use of debt, multimarket deterrence, and causal ambiguity. In addition, models have been used to explore joint ventures, venture capital, vertical integration, the appropriability of intellectual capital, governance structures, and many other phenomena. All of this has been informative and provides strategic management with a panoply of useful insights. However, these phenomena have not been *deduced* from these models or from some general theory. Rather, each of these many models has been carefully engineered to deliver the phenomena being studied. The contribution of a good modeler is in finding the least aggressive assumptions that



enable the phenomena in question. Consequently, the new microeconomics is essentially a formal language for expressing knowledge elsewhere obtained. Camerer (this issue) calls this the 'collage problem.'

The 'collage 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.

The new microeconomics is still a developing field and in the future we will see further elaboration of existing frameworks. But we can also confidently expect to hear the clangs of new monkey wrenches being thrown. One already in the air is the strong evidence for persistent biases in human judgement and decision-making. Another which can be anticipated is the fact that managers not only have different information sets, they also differ in their beliefs<sup>21</sup> and in their understandings of the causal mechanisms they face. A third, emphasized by Nelson (this issue), is that firms do not apprehend complete sets of alternatives, but grope forward with but limited understanding of their own capabilities and the opportunities they face.

The implications of this research style for strategic management are several. First, it should be clear that knowledge about what phenomena need be studied is outside its scope. Hence, there remains a central and important role for scholars who identify phenomena worth studying. For example, it is up to strategy and management scholars to convince financial economists that most firms really do budget as if they were equity constrained—only then will useful models of this phenomena appear. Similarly, it is up to strategy researchers to reveal the patterns of global

interdependence and competition—economic modeling will come after the fact. Second, the economist's approach to these phenomena is to show their existence; yet this is rarely sufficient to help in practical strategy work. Yes, it is useful to know that reputational equilibria are enabled when product quality cannot be determined by inspection and warranties are unavailable, but this is of little help to a firm that wants to know whether or not its reputation in the U.S. for workshirts will help it in Eastern Europe. It is up to strategy (or marketing, or other functional fields) to develop the measures, tools, and methods to help in specific situations. Third, each of the economist's models tends to be minimal and independent of the others—they do not integrate into any cohesive theory of the firm. For example, game theorists can model entry deterrence as based on reputations for toughness, as flowing from asset specificity, as responsive to uncertainty about post entry performance, and find that entry is encouraged by opportunities for learning, by the presence of technology options, and by economics of scope involving related products. However, these separate models provide little or no information about which of these phenomena, if any, will predominate in a specific situation, nor do they help much in determining even the rough magnitudes of the wealth impacts each of these phenomena can induce. This lack of specificity not only hinders empirical testing, it renders the professional utility of these concepts dramatically smaller than model builders imagine.

### 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

<sup>21</sup> A belief is a prior probability assignment to an unobservable variable. Interesting beliefs are those which affect decisions yet which are not significantly updated by events.



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.

For this set of reasons we believe the boundaries between strategic management and economics will remain distinct, but proximate and sometimes fuzzy. But the applied nature of strategic management and its extensive scope will require intersection with theory from other social science disciplines as well.

### **A GUIDE TO THE PAPERS**

The eight papers in this special issue each raise or address issues which lie in the terrain between economics and strategic management. The authors are leaders in their fields: Colin Camerer in competitive strategy and the experimental economics of games, Alfred Chandler in business history as well as corporate strategy and structure,

Richard Nelson in the economics of technological change, Michael Porter in competitive strategy, Garth Saloner in game-theoretic industrial organization economics, Andrei Shleifer and Robert Vishny in financial economics and corporate control, and Oliver Williamson in organizational economics. The commentator on Camerer's and Saloner's papers, Steven Postrel, is a contributor to both game-theory and competitive strategy.

It is worth emphasizing that each author was assigned the topic for his paper by the editors. The topics were selected to reveal the state-of-the-art in the connection between economics and strategic management. The happy consequence of having this uniquely talented group respond to our requests is that we obtain an unobstructed view of our subject. Because each author has been involved in the development of the concepts and theories they use and describe, there are no problems of misinterpretation, lack of comprehension, or misinformation.

The very heartening aspect of these papers, especially those written by discipline-based economists, is that no one questions the importance of the issues that are raised in strategic management. Twenty five years ago there would have been no such agreement. Furthermore, there is general agreement that neoclassical microeconomics is woefully inadequate to deal with important issues of strategy. The fracture lines begin to appear over which of the newer economic subfields supply the greatest insights into strategic advantage. Not surprisingly, game theorists tend to bet on game theory. . . and so on.

The alert reader will discern three basic frameworks in these papers (some papers use more than one). The first stresses the centrality of avoiding direct competition and has no great problem with fairly strong rationality and equilibrium assumptions (e.g. Saloner and Camerer, as well as Porter's treatment of the structure of advantage). The second framework stresses the importance of governance and of getting the match right between the technologies to be managed and the system of ownership, administration, planning, and control. The writers using this framework (Chandler, Shleifer and Vishny, and Williamson) mix a static model of efficient arrangement with the willingness to see real firms as making mistakes and learning from them. The third framework stresses the centrality of innovation, learning, and discovery in shaping

advantage (e.g. Nelson, as well as Porter's treatment of the origin of advantage).

### The papers

The development and proper scope and structure of the diversified firm is one of the central issues in our field. Alfred Chandler's original study of this subject was a key stimulus for the development of a scholarly research tradition in strategic management. In this paper he revisits the question, using the events of the last 25 years to inform a new view of the administrative limits of corporate headquarters units. In particular, he examines how continued growth forced the standard M-form organizations of the immediate post-WWII era to a three-tiered structure, and how prosperity (and hubris) led to diversification strategies that overtaxed these structures.

The basic conceptual scheme Chandler brings to this paper is that developed in *Scale and Scope*, (Chandler, 1990). Heavy and technologically complex industries are characterized by inexhaustible technical economies of scale and scope, but the ability of firms to exploit these economies is limited by their entrepreneurial skill in guiding complementary investments and their administrative skill in coordination of the resultant operations. Thus, it is the managerial capabilities of the corporate office that ultimately determines the size, scope, and success of the enterprise. In this paper, Chandler uses Goold and Campbell's [1987] topology of headquarters styles, identifying those using purely financial controls as essentially administrative and those using strategic planning or strategic control methods as performing some entrepreneurial functions. He analyzes the recent histories of British and U.S. firms and concludes that multibusiness companies employing financial controls have been successful only when they have restricted their ownership to firms in services and in simply mature industries. Where industries are mature, but complex and require substantial investments, headquarters units must engage in strategic control. And where complexity is combined with technological advance, headquarters offices must supply entrepreneurially oriented strategic planning.

As in Chandler's other works, many of his conclusions fit easily within an 'economizing' institutional economics framework. Thus, for example, the fact that advancing technology

increases the need for headquarters strategic planning can be seen as induced by the costs of haggling and hold-up that would be borne were the divisions to plan on a decentralized basis. However, Chandler's essential contributions go far beyond this static picture. In reaching his conclusions, Chandler uses the methods he has perfected: the historical analysis of challenge and managerial response. In this paper we do not see firms 'applying' concepts or somehow driven to the efficient response by selection pressure. Instead, we see management getting it wrong, suffering consequences, struggling to understand the nature of their dilemmas, and then, perhaps, creating new structures, policies, and methods to cope with, and perhaps transcend, the problem. Chandler's real message is not that one must get the headquarters design just right, but that those firms which dominate their industries are those which have shown the most resilience and insight in responding to the challenges that their own growth and expansion have generated.

Andrei Shleifer and Robert Vishny investigate some of the same terrain as Chandler—the wave of unrelated diversification followed by a wave of restructuring and retrenchment. Shleifer and Vishny review the available evidence and conclude that unrelated diversification did not improve economic efficiency. Unrelated diversification was carried too far in the 1960s, they argue, because of antitrust enforcement as well as agency problems connected with multidivisional structures: 'The M-form begot the monster of the conglomerate.'

What makes Shleifer and Vishny's paper especially interesting is their treatment of the efficient market hypothesis. Since the stock market responded to conglomerate acquisitions in the 1960s, many researchers have concluded that they created value. This paper argues that the stock market was merely reflecting the *mistaken* beliefs of a majority of investors. Drawing on their research on arbitrage and market fads, Shleifer and Vishny contend that fads persist because it is too costly for the best-informed investors to bet against them.

The boom and bust of conglomerates is a convenient vehicle for this argument, but its implications extend well beyond the issue of conglomeration. Event studies, using stock market residuals, have become a standard way of investigating the 'value' of various policies and

strategies. If these studies do not really measure value, but only what investors think is value, then this whole methodology may be significantly weakened.

Richard Nelson's paper addresses the question of how and why firms differ, an extremely deep question in strategic management. If different firms display different levels of performance or competitive advantage, despite competition, then the reasons for these persistent differences reveal the basis of competitive advantage. In this paper Nelson tackles the especially difficult version of this question: how *discretionary* considerations—such as the strategies and structures adopted by management—help underpin such differences. Although the existence of discretionary differences is comfortable for many students of strategy, it is at odds with neoclassical microeconomic theory, which Nelson sees as 'badly limited' and hence unhelpful to the field of strategy. It is badly limited because it is often too abstract and rarely deals with economic aggregates smaller than the industry, and because economists see the economic problem as basically about getting private incentives right, not about identifying the best things to do, and how to do them. In this regard, Nelson and Williamson see eye-to-eye. Neither has much time (nor do the editors of this special issue) for the long, but gradually eroding tradition in economics which treats firms as black boxes.

Nelson stresses that if economics is to inform fundamental questions in strategy, economists must break away from the assumption of clear and obvious choice sets and correct understanding of consequences of making various choices. He offers a Schumpeterian perspective, one which stresses the importance of fundamental uncertainty, perceptions about feasible paths, and trial and error learning, as a better way to come to grips with firms and firm behavior. More particularly, he argues that it is the differences among firms in their abilities to generate and profit from innovation, not differences in command over particular technologies, that are the basis of durable, difficult-to-imitate differences in firm performance. It is the issue of firms' *capabilities to innovate* which the strategy and competitiveness literature ought to be more forthright in tackling.

Oliver Williamson's paper is a call to arms. The war is against the idea that strategizing is a

source of competitive advantage, and in favor of stressing the importance of economizing. It argues that whereas the field of strategy should be concerned with first-order economizing ('rectangles'), it has imported doctrines from industrial organization economics which are focused on second-order economizing ('triangles'). Williamson contends that if strategic management is to unlock the sources of long-run competitive advantage, and if it is going to rely on economic thinking to assist it, then it ought not to rely so uncritically on economic perspectives which appeal to market power (strategies that restrict product competition) as the source of advantage. Rather, the field should develop more of an efficiency perspective—that being good at what you do and avoiding waste is more important than exploiting switching costs or playing oligopoly games.

Note that Williamson's *economizing* firm is miles away from Porter's *low-cost producer*; the economizer is not necessarily efficient at production, but in the broad range of business functions. For example, the economizer may be very efficient at managing the transition from design to production, or at tailoring products to local tastes. Williamson's position on this issue is at variance with the traditional (economic) assumption that firms are 'on their cost curves.' If firms are assumed to be technically efficient, the problem is simply to determine the level of output. Williamson, by contrast, sees the fundamental challenge as organizing and governing activities so as to eliminate waste.

Because transaction cost economics, which Williamson pioneered, is concerned with first-order economizing, he suggests that it has much to offer the field of strategy. (Of course, there are other approaches which focus on economizing too.) His paper goes on to identify several important insights from transaction cost theory which are relevant to strategy. Transaction costs are the costs of organizing the economic system. Internal structures, managerial control systems, and the positioning of the boundaries of the firm all impact transaction costs. Williamson outlines a framework which helps explain why these costs differ across organization forms and then shows how the framework applies to several issues in strategic management.

Michael Porter has played the key role in shaping the currently dominant perspective on

competitive strategy. That perspective attempts to explain how a particular configuration of activities, resources, and industry characteristics combine to shield a firm's profits from rapid competitive erosion. In this paper Porter makes the point that the dominant perspective explains competitive success at a given point in time, solving what he labels the *cross-sectional problem*, but that the dynamic process by which firms perceive or attain superior market position, what he labels the time series or *longitudinal problem*, is much less developed. His paper attempts to suggest what we know and what we need to know to develop a theory of firm performance linked to managerial choice, initial conditions, and environmental circumstance.

Porter begins with what he labels the chain of causality (Figure 2 in his paper). In his cross-sectional explanation, success flows from advantage inherent in industry structure and relative position. Advantage, in turn, is due to the configuration of activities. The activities provide support to the configuration, in turn, because of drivers (i.e. activity-level sources of advantage). Backing up longitudinally, activity configurations and drivers arise from 'initial conditions' and managerial choices. In the paper he then moves even further back, noting that initial conditions are the result of past managerial choices, luck, and the nature and quality of the local (business) environment. One can, of course, then step back again, seeing the character of the local environment as due to the policy choices made by a variety of institutional leaders and natural physical endowments. This chain of causality map not only helps unify Porter's own theorizing on competitive strategy, it also clarifies the different levels at which explanation can be attempted or equilibrium assumptions applied.

Why do some managements make the right choices in selecting products, industries, and activity configurations? Porter reviews the degree to which game theory, commitment views (Ghemawat, 1991), and the resource-based theory of strategy can provide answers. Not surprisingly, none does the job, but we obtain insights about each approach along the way. Where, then, to turn? Porter's (1990) own current answer is luck and local environment. Drawing on his research in *The Competitive Advantage of Nations*, he argues that managerial insight does not spring up randomly, but is concentrated, in each

industry, in certain locales. In those locations, clusters of competing and supporting firms have grown up which collectively embody a great deal of specialized know-how. One of the most intriguing ideas advanced here, one drawn from *The Competitive Advantage of Nations*, is that strength is frequently the fruit of adversity.

What seems to keep us from making better progress on understanding managerial choice? Porter suggests that a key missing element is a theory of action that is not rooted in choice, but which deals with creating new options and discovering new approaches. In this sense, he joins forces with Nelson who also calls for a model of search and discovery to help inform the discussion of innovation and change.

Three papers in this special issue address the connections between modern game theory and strategic management. Garth Saloner provides a viewpoint on the usefulness of game theoretic modeling in strategic management. His basically positive view is conditioned by two major cautions: there is no evidence of any real-world use of game theory by companies, and game theoretic approaches are 'too hard' to be applied to anything but very simple 'boiled down' models of reality. The second issue may, of course, be the reason for the first and it is interesting to speculate on what consequences would flow from the invention of a game theory 'engine' that quickly and clearly yielded the equilibria of very complex models.

Saloner's enthusiasm for game theoretic models survives these two considerations and is based on their necessity, the 'audit trail' they provide, their metaphorical value, and their growing importance in empirical research. Once you begin to consider the reactions of rivals to one another's moves, he argues, you are doing game analysis, and the current theory is simply the distilled wisdom about the most sensible way to do it. The great value of explicit modeling is the clear record of assumptions and logic—the audit trail—that permits others to verify and modify one's analysis. Saloner dismisses the use of game theory to calculate actual behavior, stressing instead the value of understanding why certain results obtain in certain situations and the possibility of novel insights. As work progresses, he argues, research will build up a mosaic of models, each providing insights about a particular aspect of strategic interaction. Game theory's contribution to stra-



tegic management will be the sum total of the insights this mosaic provides.

One of the most challenging questions Saloner tackles is the reasonableness of the rationality imputed to players in game theory. He points out that in many games, such as Cournot competition, the rationality required is not very great. However, in many modern game models, equilibria are based on quite complex considerations, straining the credulity of the rationality assumption. There is no escape, he suggests, from using judgement on this matter and notes that your own play in a game might be affected by whether your opponent was David Kreps, a fourth grader, an average undergraduate, or the CEO of a typical U.S. firm.

Colin Camerer also addresses the utility of game theory to strategic management. Like Saloner, Camerer is concerned with the sparseness of modern analysis, termed 'no fat' modeling, and with the fact that game analysis is hard. If neoclassical analysis is like eating with a fork, he analogizes, game theory is like using chopsticks. Game theory is not only hard, Camerer stresses, it is also too easy. That is, it is too easy to generate explanations for all sorts of behavior. This happens because behavior is not just determined by preferences, but also by the presence of hidden information.

The heart of Camerer's essay addresses the rationality assumption—is it too demanding to be reasonable? His own laboratory work on games shows that people do not arrive at strategies using the cognitive methods of the theorist. Consequently, theoretical equilibria are usually approached only after repeated play. Nonetheless, through processes of adaptation and/or evolution, theoretical equilibria are approached. Camerer also points out that the strict rationality assumptions of the theorist are sometimes only an analytical convenience; the same equilibria can often be justified with weaker assumptions, though the analysis is more difficult.

Despite these and other difficulties in living with game theory, Camerer favors welcoming it into the strategic management family. Like Saloner, he feels that it is the best way to look at interactions among alert rivals. In addition, Camerer sees opportunities to inform areas of interest to strategic management, such as the properties of collective resources (reputations and capabilities). Finally, he argues that the

problem of too many explanations and too many equilibria provides opportunities for good empirical work to point the way.

Steven Postrel's paper is a comment on Saloner's and Camerer's discussions of game theory and strategy, especially the 'Pandora's Box' problem that the theory has too few constraints on generating explanations of behavior. Using a humorous setting, Postrel shows how a game-theorist could build a model to rationalize unreasonable behavior. His point is that game theory is not really a theory of strategy but is only a methodology for analyzing games. Other than rationality, the substantive theory present in a model is in the assumptions, not in the mechanics.

These then are the papers offered in the special issue. All offer informed and interesting views, and we hope will in their own right inform the reader on boundary conditions, future challenges, and research opportunities that lie in considering economic reasoning on strategic management issues.

## 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, thus far, has 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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