FUNDAMENTAL ISSUES IN STRATEGY

A RESEARCH AGENDA

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History of Strategic Management

Strategic management, often called "policy" or nowadays simply "strategy," is about the direction of organizations, and most often, business firms. It includes those subjects of primary concern to senior management, or to anyone seeking reasons for 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. Because of competition, firms have choices to make if they are to survive. Those that 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 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.

Strategic management as an academic field is much younger than its actual practice. While its date of conception (not to mention parentage) is somewhat uncertain, the academic field of strategic management is certainly a child of the 1960s. As such, it is now entering upon its "thirty-something" decade, a time of self-examination and of coming into its own. The premise of this book, and of the conference which preceded it, is that academic strategic management is indeed ready to come into its own, through the identification and clarification of the fundamental issues of scientific interest that distinguish it as a field of scholarly inquiry.

As an introduction to those fundamental issues, we briefly review the history of the academic field. If the child is indeed father to the man, a look back will help put the fundamental issues we identify into perspective, provide context, and clarify terms.

The Business Policy Course and Faculty

Strategic management, as a field of study, originated as a teaching area in business schools. The first business school, The Wharton School, was established at the University of Pennsylvania over 100 years ago. Harvard established its school some years later, but soon assumed a leadership role in business education. As far as can be determined, it was at Harvard that the first business policy course was taught.

The Harvard curriculum was built out of so-called functional courses, corresponding to business functions like accounting, marketing, and manufacturing. The policy course "integrated" what the student had learned in the functional courses, serving as a capstone to the core curriculum. True to Harvard tradition, the case method was employed. The course was not burdened with teaching substantive mechanisms of achieving integration—it simply presented administrative problems faced by senior executives that naturally required a multifunctional perspective.

Harvard served as an important model for other business and management schools, leading many to imitate its design. Business policy as a capstone course became a standard part of the curriculum across the United States. Indeed, today, the Association of American Collegiate Schools of Business (AACSB) includes instruction in business policy among its guideline requirements for accreditation. Tellingly, though, the AACSB has left "business policy" open to very broad interpretation.

As an integrative capstone course, business policy may have had some measure of prestige, but it had no prescribed content. No received theory grounded in the professional norms of a business function, or in the basic disciplines of the social sciences, needed to be taught. Historically, the course was often staffed by full professors, experienced teachers thought to have developed a broad view of business, or by adjunct professors, often former general managers with the wisdom of experience to transmit. With no theory to teach, any discipline or experience base seemed satisfactory, and indeed, eclecticism and a holistic view were valued.

Relegation of strategic management to a capstone course in business policy had serious structural consequences for the development of strategic management as a scholarly field of inquiry, however, and probably stifled its emergence for many years. A single, capstone course permitted no development of follow-on courses, and in turn limited the scope for expanding consideration of the subject. Since the teachers used to staff the course were either already full professors or were adjunct teachers without hope of tenure or interest in full rank, there was no evolutionary career path from assistant to full professor. The career paths to tenure and advanced rank remained in functional areas of business (e.g., marketing, finance) or in the traditional social science disciplines (e.g., economics, organization theory).

Faculty for the business policy course made their intellectual homes elsewhere. If serious research was done, it was done for the basic disciplines in which faculty members were trained. Moreover, the acceptability of using senior faculty members from a variety of disciplines often meant, as a practical matter, that the business policy course became something of a burying ground for academic white elephants long in the tusk but short on remaining research potential.

Some work had to be done, of course, to support course development. This scholarship tended to result in cases and notes of value to teaching, not articles and books of interest to other researchers, and it generated little theory or academic debate. One of the positive legacies of this practice, however, was extensive institutional knowledge and rich general descriptions of practice as observed from case writing and consulting.

In important respects, the field of strategic management only began to develop in terms of research accumulation in the early 1970s. It was then that the first faculty were hired as "policy" teachers, as pioneers expected to find their way up the promotion system to gain tenure in their own field, that of strategic management, by developing research records. Then the questioning of constructs and attention to tools and techniques of scientific research began in earnest. Then, too, professional associations and journals began to be needed.

Journals and Professional Societies

Any field develops around an infrastructure of journals and professional meetings, through which results are argued and disseminated. Initially, strategic management took advantage of the infrastructure built in the 1950s and 1960s to accommodate the increased interest in business and administrative organization. Before 1970, there were no distinct professional societies or publications devoted to the field.

The early history of strategic management showed remarkable interest in "planning," and although it proved short-lived, enthusiasm for planning dominated strategic management in the late 1960s and early 1970s. Consequently, some of the earliest efforts to organize societies and publications centered on planning per se. The Institute of Management Sciences developed a College on Planning, which attracted a small band of workers interested in formal models of the firm useful for planning long-range operation of organizations. The college did not flourish and was ultimately abandoned. The Planning Executives Institute (PEI) devoted its energy to budgeting and short-term financial planning, and was not truly interested in strategy concepts. PEI later was to combine with a practitioner group, the North American Society of Corporate Planners, to form The Planning Forum, which continues to operate throughout North America. Planning societies devoted to improving the state of practice for professional planners formed in various countries in Europe and elsewhere. Some, such as the Strategic Planning Society of the United Kingdom, are quite large.

Publications like *Long Range Planning* and *The Planning Review* were among the early outlets for work in strategy. Early work in strategy also found its way into general management journals such as the *Harvard Business Review, Sloan Management Review, Journal of Business, Business Horizons,* and *California Management Review*. Academic journals devoted to management topics, such as *Administrative Science Quarterly, Academy of Management Journal,* and *Management Science,* also provided important opportunities to publish scholarly research.

About 1969, the Academy of Management elected to form professional divisions to better reflect the specialized interests of its members. One of the first divisions formed, in 1971, was the Business Policy and Planning Division, since renamed the Business Policy and Strategy Division to reflect the ascendance of strategy concepts and the decline of planning. The division became an important base of support for the establishment of tenure-track strategy faculty.

Although interest in planning per se fell off, the number of practitioners and academics interested in topics of strategic management grew steadily through the 1970s. The growth of interest and support was reflected in the 1980s in the formation of societies and publications specialized to strategic management. In 1980, two journals devoted exclusively to strategy commenced publication: the *Strategic Management Journal* (*SMJ*) and the *Journal of Business Strategy,* the former oriented to academic research and the latter to practice. Both flourished, and *SMJ* rapidly became the leading research journal in the field, as reported in the Social Science Citation Index.

Also in the early 1980s, the Strategic Management Society was formed as an organization of practitioners, consultants, and academics interested in developing the field. Unlike predecessor organizations, it held international meetings and tried to attract members from all over the world. With members from about 50 countries, it sponsors meetings around the world and a publication (*SMJ*) devoted to advancing scholarship in the field.

The Development of Strategy Theory and Research

Precursors. The prehistory of strategic management as an academic field lies in studies of economic organization and bureaucracy. The vigorous interaction with economics and the study of organization, which characterizes the field today, reflects its origins in these disciplines. Work in a wide variety of areas contributed to a single task vital to the emergence of strategic management: preparing the ground for concepts of strategy.

Mainstream economic theory—price theory—has traditionally ignored the role of managers and left little scope for strategic choice in economic affairs. From the time of Adam Smith down to the present day, economists have sought to show that a completely decentralized economic system, coordinated only through market prices, could and would be efficient. Little attention has been given to why private firms might make use of managerial hierarchies to plan and coordinate. Institutional settings and arrangements have been largely abstracted away. The varied character and capabilities of actual business organizations have not been considered. The firm of economic theory observes market prices and then makes an efficient choice of output quantities; all firms are essentially alike, having the same access to information and technology, and the decisions they make are essentially rational and predictable, virtually compelled by cost and demand conditions.

While conventional economic theory did not recognize most of the choices open to managers of firms, work in a wide variety of areas—more than can be cited here—served to establish a basis for studying the role of management and the possibilities for strategic choices, before strategic management began to emerge as a field of study.

In the mid-twentieth century Taylor (1947) initiated a "science of work" in an organizational setting, beginning the effort to understand what economists might call "technical efficiency" or "x-efficiency." Taylor's enduring contribution was the conviction, and demonstration, that "natural" practices could be improved through careful observation and analysis.

Barnard (1938) elevated the analysis of organizational work from Taylor's shop floor to the executive ranks in his classic work, *Functions of the Executive*. He stressed the difference between managerial work directed at making the organization efficient, and work that made the organization effective, a distinction critical to the concept of strategy. Simon (1947) extended Barnard's ideas in his attempt to build a framework for analyzing administration. Selznick (1957) explored the roles of institutional commitment and introduced the idea of an organization's "distinctive competence."

The difficult economic conditions of the 1930s raised many questions about capitalism and the real efficiency of business. During this period, theories of imperfect competition were developed by Robinson ([1933] 1959) and Chamberlin (1933). Schumpeter's (1934) innovative entrepreneur and agent of creative destruction provided an alternative to the static concept of competitive efficiency favored by most economists. Later, Frank Knight's (1965) work on the risk-bearing function of entrepreneurs laid an early foundation for much of what we now call organizational economics.

Despite the increasing interest in business organization from the turn of the century on, there was no sustained debate about business strategy and its role in the success of firms. The founder of McKinsey & Co. evidently wrote about strategy in the 1930s. Newman (1951) used the concept of strategy to differentiate certain important work of the manager from the day-to-day work of running an organization. However, it was not until the 1960s that a field of interest could be seen forming.

1960s: Birth. The birth of strategic management in the 1960s took place against a background of tremendous ferment in organization theory. Universalistic principles and maxims of administration were being overthrown in favor of concepts of contingent design. March and Simon (1958) had developed the cybernetic or information-processing metaphor for management structure. Cyert and March (1963) had laid out a behavioral theory of the firm. Open systems theories and approaches, which tended to suggest that organizations were somewhat akin to organisms in a natural environment, had a major influence. Burns and Stalker (1961) contrasted organic and mechanistic types of management organization. Woodward (1965) showed how production process technology influenced organizational structure. Thompson (1967), in a bold, propositional inventory, and Lawrence and Lorsch (1967), in a more empirical work, proposed that managerial organization was contingent on "environmental uncertainty." The 1960s were a propitious time to introduce concepts of strategic adaptation by organizations.

The birth of the field of strategic management can be traced to three works of the 1960s: Alfred Chandler's *Strategy and Structure* (1962); Igor Ansoff's *Corporate Strategy* (1965); and the Harvard textbook, *Business Policy: Text and Cases* (Learned et al., 1965), the declarative text of which is attributed to Kenneth Andrews.

The foundation of strategic management as a field may very well be traced to the 1962 publication of Chandler's *Strategy and Structure*. Chandler's seminal book, subtitled "Chapters in the History of the Industrial Enterprise," was about the growth of large businesses and explored how their administrative structures had been adapted to accommodate that growth. In the process of telling the story of growth and administrative change at General Motors, Sears, Standard Oil of New Jersey (Exxon), and DuPont, Chandler showed how executives at these companies discovered and developed roles for themselves in making long-term decisions about the direction of their enterprises and then made investments and modified organizational structure to make those strategies work. A compelling and fascinating story of economic innovation, organizational behavior, and managerial achievement, it naturally attracted much interest and attention. Most important for the field, Chandler showed executives doing strategic management work and achieving remarkable performance outcomes. Moreover, he showed a process of administrative change within organizations that involved shifts in strategic direction, rather than adjustments for simple efficiency.

In formulating a thesis to summarize his findings, Chandler found it convenient to define two terms, strategy and structure:

> The thesis that different organization forms result from different types of growth can be stated more precisely if the planning and carrying out of such growth is considered a strategy, and the organization devised to administer these enlarged activities and resources, a structure. Strategy can be defined as the determination of the basic long-term goals and objectives of an enterprise, and the adoption of courses of action and the allocation of resources necessary for carrying out these goals. (pp. 15–16)

The concept of strategy used by Chandler was a handy way of characterizing the relationship among a set of managerial purposes and choices, and was explicitly distinct from a structure.

Andrews, in his text for Business Policy, accepted the strategy idea from Chandler, but added Selznick's "distinctive competence" and the notion of an uncertain environment to which management and the firm had to adapt. In Andrews's view, the environment, through constant change, gave rise to opportunities and threats, and the organization's strengths and weaknesses were adapted to avoid the threats and take advantage of the opportunities. An internal appraisal of strengths and weaknesses led to identification of distinctive competencies; an external appraisal of environmental threats and opportunities led to identification of potential success factors. These twin appraisals were the foundation for strategy formulation, a process analytically (if not practically) distinct from strategy implementation. Andrews conceived of strategy as akin to identity, defining it as "the pattern of objectives, purposes, or goals and major policies and plans for achieving these goals, stated in such a way as to define what business the company is in or is to be in and the kind of company it is or is to be" (p. 15). Andrews considered it a "matter of indifference" whether to include selection of goals as well as the deployment of resources in pursuit of goals as part of strategy. He regarded strategy formulation as "analytically objective," while implementation was "primarily administrative."

H. Igor Ansoff was general manager of the Lockheed Electronics Company (New Jersey) and developed his strategy concepts out of frustration with planning as the naive extrapolation of past trends. Ansoff was more explicitly interested in understanding what was meant by "strategy" and took some care to develop his ideas. He accepted that the objective for the firm ought to be to maximize economic return, which he distinguished from accounting return. For Ansoff, strategy provided a "common thread" for five component choices: (1) product-market scope; (2) growth vector (the direction in which scope was changing, e.g., the emphasis on old versus new products or markets); (3) competitive advantage (unique opportunities in terms of product or market attributes); (4) synergy internally generated by a combination of capabilities or competencies; and (5) the make or buy decision. In tracing this common thread of strategy through its components, Ansoff emphasized the potential for success arising from mutual reinforcement among the components.

In retrospect, it seems that Ansoff was more interested in what we would today call *corporate* strategy, while Andrews was more focused on *business* strategy. Ansoff's more elaborate analysis of the *concept* of strategy was also reflected in a more elaborate view of the *process* of creating strategy, a difference in emphasis that promoted the cause of strategic planning. Both Ansoff and Andrews, however, had gone beyond the traditional Business Policy course metaphor of functional integration.

These three authors—Chandler, Andrews, and Ansoff—gave first form to the basic concepts of strategic management. Nearly all of the ideas and issues that concern us today can be found in at least embryonic form in these key writings of the 1960s. It should be acknowledged, though, that their audience was primarily students and professors. None of these three authors directly and immediately influenced practice. Many of the changes in practice that occurred by the late 1960s can instead be traced to the influence of consulting firms. Leadership in this arena was provided by the exemplar of strategy consulting firms, The Boston Consulting Group.

Founded by Bruce Henderson in the mid-1960s, The Boston Consulting Group (BCG) had a major impact on the strategy field. Although a great deal of BCG's practice consisted of "segmentation studies" which simply reanalyzed cost and profit data using economic concepts rather than accounting measures, BCG became best known for its two related conceptual inventions: the experience curve, and the growth-share matrix. In brief, experience curve theory maintained that whoever captured market share early, whoever gained the most experience in production would end up with the lowest cost (assuming efficient operational management practice), and whoever had the lowest cost would have the highest margin. With the highest margin came cash flow and an ability to withstand competition and whatever actions it required. Within a few years, such reasoning led to the growth-share matrix, whose terminology of cash cows, dogs, stars, and question marks became famous and widely used.

The rapid expansion of the firm, successful spin-offs like Bain, and imitation by old-line rivals like McKinsey, attests to the influence that BCG had on practice. In contrast to many other consultants to top management, who emphasized long-term planning without much attention to strategy, BCG made strategic conception central. The experience curve and growth-share matrix drew a sharp, clear line between operational decision making and corporate strategy, highlighting the latter. The corporate strategist was encouraged to assume that efficient operations management would achieve the cost reductions projected along the experience curve and to make corporate investment decisions and plans accordingly. Moreover, the dynamic aspect of competition implied by the experience curve clearly called for strategic behavior: preemption of rivals with commitment today was necessary for success tomorrow.

Together with the work of Chandler, Andrews, and Ansoff, the developments at The Boston Consulting Group gave a powerful new thrust to managerial work and responsibility during the 1960s. The entrepreneurial responsibility of management was recognized—not just an act performed at birth—but as a continuing, pervasive responsibility to consider the long-run, dynamic direction of the firm, even while maintaining routine and efficient operations.

1970s: Transition toward a research orientation. While the work of Andrews and Ansoff, and of others who aimed to provide material for the policy course, expanded consideration of strategy concepts, there was no early reflection on the normative character of the statements made. Experiential, case-based evidence lay behind the writing, but there was little analysis, and no evidence was offered to satisfy a critical reader. Chandler's work, supported by historical methods, was much less aggressively normative and prescriptive, of course, but was still essentially inductive in character. At best, these first works offered a set of constructs and propositions about how strategies formed and how they affected the performance of business enterprises. Systematic observation, deductive analysis and modeling, and careful empirical testing had to wait for the 1970s.

As strategic management began to advance in the direction of positive science in the 1970s, a dichotomy developed between those pursuing essentially descriptive studies of how strategies were formed and implemented (process) and those seeking to understand the relationship between strategic choice and performance (content). It is not clear that the work of Chandler, Andrews, or Ansoff fits easily into either category. The work of all three appears to have implications for both content and process. The seeds of division between process and content may have been sown by distinctions made for rhetorical or expository reasons. Andrews, for example, wrote: "Corporate strategy has two equally important aspects, interrelated in life but separated to the extent practicable in our study of the concept. The first of these is formulation; the second is implementation" (p. 17).

The convenience of analytically isolating strategic choice remains difficult to reconcile with the reality of organizational, strategy-making processes. However, today we are seeing signs of a reconciliation that promises new gains. The developments yielded by recent research efforts suggest that processes to formulate strategy themselves have asset-generating capability.

Late in the 1960s and early in the 1970s, concepts of strategic and long-term planning played important roles in the field. This movement owed much to the diffusion of war-based planning experience in the corporate world. For example, George Steiner had learned planning by working on materials allocation in Washington during World War II, by serving as director of policy in the Office of Defense Mobilization during the Korean War, and then as chief economist at Lockheed (1953–1954). Later, as an academic, he wrote extensively on formal long-range planning processes. Much of this literature was descriptive of selected industry practices and was strongly prescriptive, never attempting to be analytical or empirical.

The prominence of long-range planning, and then strategic planning, failed to survive the economic turmoil that began with the oil embargo of 1973 and continued with the advent of floating exchange rates, high inflation, and increasing international competition; organizations learned from practical experience that simple extrapolations of history and cadres of professional planners failed to lead to innovation, adaptation to change, or even survival. Planning processes too easily degenerated into goal-setting exercises, failing to embody any real understanding of competitive advantage. Moreover, when more sophisticated planning process designs were advanced, problems of execution or implementation increased. An organizational process to both create and execute strategy proved to be poor at conception and not influential enough to make a substantial difference in implementation.

Careful observation of actual organizational decision making gave rise to more subtle conceptions of process, in which strategies were arrived at indirectly and, to some degree, unintentionally. Uncertainty ex ante led to tentativeness, search and serial trial, and some learning—a somewhat chaotic process—which, with a certain amount of luck, might accumulate to a strategy, which could be named and described as coherent only ex post. Lindblom's (1959) "muddling through," Quinn's (1980) "logical incrementalism," and Mintzberg and Waters's (1978) "emergent strategy" all attempted to gain insight into organizational processes which produced strategy as a somewhat unintended outcome.

Attempts to understand and test the connection between strategy and performance also began in the 1970s. In this work, three streams ought to be highlighted. One, centered at Harvard and following on Chandler, generated and tested propositions about corporate growth and diversification strategies. A second, focusing on business strategies, began with the so-called brewing studies at Purdue. The third, also at Harvard, used an industrial organization economics perspective to study business strategy, and culminated in Michael Porter's work on analyzing competitive strategy and competitive advantage.

The brewing studies done in the early 1970s at Purdue examined the strategies and performance of major U.S. brewers over time. Their goal was to explore the proposition that performance was a function of strategy and environment. The brewers were chosen because they represented a group of mostly undiversified firms, and because, due to product taxation and heavy regulation, good data were available for representing the constructs (i.e., strategy and environment) and functional form of the relationship.

The brewing study results (Hatten and Schendel, 1977; Hatten, Schendel, and Cooper, 1978), were generally consistent with the notion that strategy, in addition to "environment," mattered, so that a "better" strategy, relative to competitors, was associated with better performance. The studies also revealed the considerable heterogeneity in strategy and performance that can exist within a single industry; the differences were far greater than was generally presumed in industrial organization economics and, indeed, in most management and strategy thinking. These differences led to very interesting research on strategic groups and to further explanations of performance differences based on concepts of competitive advantage.

The brewing studies demonstrated that the strategy construct could be represented by measurable variables, and that empirical evidence supported the usefulness of the strategy construct itself. What had been derived on the basis of experiential, inductive methods had been supported by more objective, deductive methods of research. This represented a new departure in research philosophy for the field, and changed the direction of research in the field in ways that were more significant than the findings themselves.

At about the same time as the brewing studies, enthusiasm for Chandler's historical work had inspired further interest in empirically demonstrating a relationship between growth strategy, organization form, and the expected performance of the enterprise. This work led ultimately to important findings concerning the forms of diversification that improved performance and those that did not. Wrigley (1970), working under Bruce Scott, did the first work in trying to classify diversification strategies. Other dissertation work followed, conducted in a variety of national economies: Channon (1973) studied the United Kingdom; Pavan (1972) studied Italy; Thanheiser (1972) studied Germany; and Pooley-Dias (1972) studied France. Rumelt (1974) pushed this stream of work even further, contributing more discriminating measures of diversification and testing the impact of diversification strategy and organizational structure on performance. Like the brewing studies, this work was at least as significant for introducing new methodological approaches to the field as for its findings.

In a third major departure for studies of the relationship of performance and strategy, Porter (1980) imported into the strategy field the concepts developed over the years in industrial organization (IO) economics. Using a large number of case studies as a factual base, Porter employed IO concepts concerning market power and profitability to build a general, cross-sectional framework for explaining individual firm performance. Until Porter, firms in strategic management had been seen as adapting to general, even rather vague environments.

Porter's "Five Forces" framework substituted a structured, competitive economic environment, in which the ability to bargain effectively in the face of an "extended rivalry" of competing firms, customers, and suppliers determined profit performance. By making managerial choice in an explicitly economic environment the focal point of analysis, Porter succeeded in turning IO economics on its head. Its traditional role was to identify socially wasteful sources of "monopoly" profits, but Porter instead used the framework to define and explain the strategies available to firms in their quest for survival and profit. Drawing on his extensive case study research, he catalogued, described, and discussed a wide range of phenomena that interfered with free competition and thus allowed abnormal returns, and he suggested how their interaction and relative importance varied across contexts.

Porter's work opened an important bridge to IO economics across which traveled more than the Structure-Conduct-Performance paradigm he employed himself. The "Chicago" critique of traditional entry barrier theory, which supported the alternative view that high profits were returns to specialized, high-quality resources or capabilities, became an important inspiration for the resource-based theory of the firm. Game theory modeling in industrial organization found applications in strategic management, too.

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 other new areas of inquiry. Transaction cost viewpoints on scope and integration were adopted and new theories of the efficiency of social bonding were advanced. Studies of innovation began to use the language and logic of economic 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.

At the same time, research on the strategy process continued apace. Interestingly, the most vital new ideas were generated by those studying global firms. In the 1980s, the increasing globalization of the world's economy was leading students of general management to look ever more carefully at how large multinational corporations directed and coordinated their myriad resources and activities. An important early work was Stopford and Wells (1972). The new framework is first seen in the dissertations of the key authors: Prahalad (1975), Doz (1976), Bartlett (1979), and Ghoshal (1986). Their insights began to challenge the received wisdom about structure and process. In particular, the need to consider functional, product, and geographical bases for specialization forced thinking beyond the increasingly stale product-function dichotomy. The emerging framework represents management as needing to maintain "differentiation" in some activities to achieve gains from specialization or administrative isolation, and tight integration in other areas to achieve economies of scale and focus.[1] In addition, management is seen as actively managing a complex system of linkages among activities, which enables critical coordination and facilitates organizational learning.

In looking back over these three decades, what comes into focus is the search, sometimes in vain, for theoretical explanations of very complex phenomena. The purpose has been to understand real-world phenomena and establish a base for making useful prescriptions. For the first time, basic disciplines of the social sciences, especially economics, have been linked with practical issues involved in managing the firm. What began in the 1960s as rather simple concepts of strategy intended to give insight into the phenomena described in cases has evolved into a serious search for intellectual foundations with explanatory and predictive power.

Developments and Trends in Allied Disciplines

During the past decade strategic management research has increasingly relied on the theories and methods of economics

1 See Chapter 17 of this volume.

and organizational sociology, as well as (but to a lesser extent) on political science and psychology. As a consequence, the boundaries that mark the strategy field have been blurred. Not so long ago there was no doubt about what "policy" or "strategy" research was, and certainly there was no difficulty in separating it from work in economics or other disciplines. Now those distinctions are less clear.

Given these trends, it is important to understand the fundamental questions being asked in the allied disciplines and to be aware of the changes sweeping these fields. This understanding will serve two purposes. First, it will help us to place strategic management's fundamental questions in context, and to see how strategic management is related to, yet differentiated from, its allied disciplines. Second, the blurring of boundaries makes it important to examine the interrelationships between strategy and its allied disciplines. As the strategy field begins to adopt the language and tools of agency theory, population ecology, behavioral decision theory, political science, and so on, and as scholars in those fields realize that the strategy field addresses truly important and significant questions and issues, how should the relationship between strategy and its allied disciplines be viewed, and in particular, what does it mean for a research agenda for strategy? Indeed, does strategy have an independent research future, or should it merely wait for research developments to occur and then give attention to their application? Even more pointedly, does strategy have a future as a field at top research universities, or will its subject matter be taught and researched by economists, sociologists, political scientists, and psychologists?

Economics

In the beginning, economics was centrally concerned with understanding what governed the efficient generation of goods and services and what determined the distribution of wealth in society. However, in this century classical economics, now called microeconomics, has been driven more by its internal logic than by external phenomena in need of explanation. The overriding fundamental question has been, What phenomena can be explained by models that assume that human action is rational? And even in cases where no human agent has made a coherent choice, economists ask, Which institutions can be explained by assuming they were designed and stuctured by a rational actor? This program of research, begun by the Enlightenment thinkers of the eighteenth century, has reached its full flower in the economics departments of U.S. universities during the last 30 years. Indeed, Gary Becker's recent Nobel Prize was awarded for his work on extending economic reasoning to the family and beyond—his boldest work asserts that drug addiction can be explained as a rational choice.

Although the central quest of economics, the explication of phenomena as the products of rational action, is unchanged, the context within which action is envisioned has undergone a dramatic evolution. During the first half of this century the great task before economics was the mathematization of Marshall's theories to produce the "neoclassical" theory of the firm. The economist's neoclassical model of the firm, still enshrined in textbooks, is a smoothly running machine in a world without secrets, without frictions or uncertainty, and without a temporal dimension. That such a theory, so obviously divorced from the most elementary conditions of real firms, should continue to be taught in most business schools as the "theory of the firm" is a truly amazing victory of doctrine over reality. This era may, however, finally be coming to an end as the cumulative impact of new insights takes its toll.

During the past 30 years, and especially during the last 20, at least five conceptual monkey wrenches have been thrown into what was once a smoothly running machine. These five are: *uncertainty, information asymmetry, bounded rationality, opportunism,* and *asset specificity.* Each of these phenomena, taken alone, violates crucial axioms in the neoclassical model. In various combinations they are the essential ingredients of new subfields within economics. For example, 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. Here each is briefly treated in turn.

Transaction cost economics. Of all the new subfields of economics, the transaction 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 that 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. For an example of such detail, see Joskow's (1988) study of price-adjustment clauses in long-term coal contracts.

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 an 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 arrangements for conducting economic activity. His basic point was that transactions should take place in the 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 (Armour and Teece, 1978; Monteverde and Teece, 1982).

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,[2] the structure of multinational firms (Buckley and Casson, 1976; Teece, 1985; Kogut, 1988), sales force organization (Anderson and Schmittlein, 1984), joint ventures (Hennart, 1988; Pisano, 1990), and franchising. Williamson (Chapter 13, this volume) provides a useful review of additional applications of interest to strategic management.

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 is 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

[2] Early contributions were Monteverde and Teece's (1982) study of auto components and Masten's (1988) study of aerospace.

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 toward wasteful investments or uses. Two cures for 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 of agency theory also have the tendency to see all managerial problems as due to incorrect incentives—a tautology for a perspective that assumes away any other sources of dysfunction (e.g., capital markets problems like those discussed by Shleifer and Vishny later in this book, 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 book 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 signaling 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.[3]

[3] Much of the technical foundation they used had been laid by Selten (1965) and Harsanyi (1967).

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 particular interest to strategic management: commitment strategies 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 mergers, and financial structure.

Reputations arise in games where firms or actors may belong to 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 in a way that changes 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, and students of both phenomena often ground ideas by pointing to the parallel. 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 toward 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, and implies that it is impossible to simply copy best practice even when it is observed.

Because evolutionary economics posits a firm that 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.

Organizational Sociology

The fundamental issue addressed by sociology is the structure and subjective meaning of social interaction. The center of the puzzle was and continues to be the stability of social structures and the amazingly strong controlling forces they exert on their members' actions. Although economists are also interested in patterns of exchange, two concerns distinguish the sociologists' approach: an interest in authority and a real concern with the subjective experience of social interaction. Whereas economists almost always study voluntary exchange, sociologists normally begin with the presumption that authority is a key source of social order. Equally distinctive and crucial is the sociologist's concern with the subjective. To an economist, exchange is a means to an end. When an employee exchanges labor for pay, for example, the economist sees two gains: the employee values the pay more than the discomfort of work, and the employer values the labor received at least as greatly as the cost of employment. To a sociologist, the exchange itself, and the system of exchanges within which it is embedded, generate value and meaning apart from their instrumental worth.

Organizational sociology grew from two important traditions. The "main line" flows from the work of Durkheim ([1893] 1984) and Weber (1947) through Parsons (1937), Merton (1940), and Homans (1950), to Selznick (1949, 1957) and Blau and Scott (1962). In general, this tradition has been concerned with the processes whereby authority is legitimized (accepted), with the general problem of social structure in society, and with the limits and dysfunctions of bureaucracy. The second tradition is more normative and practice-oriented, and springs from the early management theorists and from the "human relations" movement inaugurated by industrial psychologists.

From the mid-1960s through the 1970s, the contingency theory synthesis emerged and was widely disseminated. Contingency theory built on a variety of earlier insights: Woodward (1965) and Burns and Stalker (1961) showed that high-performing organizations did not have the same structure, but matched structure to the technological demands of production; the Aston studies established that there is no single factor with which organizational characteristics covary; Emery and Trist (1965) stressed the importance of the environment in determining structure; and Lawrence and Lorsch (1967) drew from their empirical work the picture of an organization with subunits adapted to differing local environments and with integrative mechanisms that assert the interests of the whole. Contingency theory hypothesized that organizations which contain subsystems "matched" to their environments perform better than those with a less perfect fit. Under competition, this implies that structure follows environment and must be able to cope with uncertainty, the most important variable in the environment. If strategy is taken to include the choice of environment, this hypothesis is consistent with Chandler's (and now strategic management's) dictum that structure follows strategy.

But contingency theory's apparent success at solving the puzzle of formal structure did not ensure its longevity. Carroll (1988: 1) gives a vivid explanation of what happened in the mid-1970s:

> Although its adherents continue working at a feverish pace, the once hegemonic contingency theory of organization has been deposed by a paradigmatic revolution. The beginnings of the revolution can be dated sometime around 1975, a period marked by the appearance of four new seminal theoretical statements about organizations: (1) the book on transaction

cost economics by Oliver Williamson (1975), *Markets and Hierarchies;* (2) the article on the population ecology of organizations by Michael T. Hannan and John Freeman (1977); (3) the article on institutionalized organizations by John Meyer and Brian Rowen (1977); and (4) the book on resource dependence theory by Jeffrey Pfeffer and Gerald Salancik (1978), *The External Control of Organizations.*

Each of these subfields of organizational sociology is relevant to strategic management (we have already viewed transaction cost economics in the previous section).

Resource dependence. Who or what determines what organizations do? The resource dependence model argues that much of what organizations do is determined by outsiders—by those parties who control the flow of critical resources upon which the organization depends. The strategic activities of management, according to this perspective, are those of accommodating or finding ways to insulate the organization from the demands of those who control critical resources. Resource dependence explains mergers, joint ventures, diversification, and board memberships in this way, and scholars working in this tradition have provided empirical support for these claims. Note that there is an affinity between resource dependence theory and transaction cost theory. Both are concerned with the governance of critical transactions, and both are concerned with the power of one party to damage the other.

Resource dependence theory also speaks to the distribution of power within organizations. Power, it is argued, is possessed by those who can influence the flow of critical resources from external sources and by those who have influence over the flow of discretionary resources. Thus, power in a consulting firm resides in those who can generate new business or influence clients, and great power in universities can be wielded by those who control relatively small discretionary funds.

Organization ecology. Economics and, to a large extent, strategic management view the firm as actively adapting to changed conditions. Organization ecology makes the opposite presumption—that firms do not adapt. Instead of the adaptive firm, organization ecology sees a population of firms that changes in composition over time as some flourish, others perish, and new organizations are born. The metaphor of biology has been frequently used by economists and strategy researchers, but Hannan and Freeman (1977) were the first to complete the metaphor, placing firms in the position of individuals with fixed genetic endowments and advocating the study of a population of firms (i.e., a species) over time, rather than the idiosyncratic features of individuals.

Although it assumes that firms do not adapt, organization ecology is much more receptive to the concept that firms have strategies than traditional organization theory. The critical difference is that organization ecology sees the strategy of a firm as fixed at its inception and as unchanging over time.[4] Once it is fixed, of course no further room is left for the strategic manager. This view is obviously at odds with much of the literature in strategic management, especially that which emphasizes strategic change, organization renewal and transformation, and flexibility. Nevertheless, it may be that strategic management scholars need to reexamine their assumptions—strategic change may well be the exception rather than the rule. Given the large number of case studies that feature companies unable to perceive or cope with a changing environment, it may be that the ecologist's assumption of strategic inertia is more realistic than the economist's assumption of rapid, rational response to change.

Much of the research in organization ecology has been directed at measuring the birth and death rates of organizations and, therefore, the rate of expansion or decline of the population under study. More recently, interest has focused on *density dependence:* the degree to which birth and/or death rates vary with the size of the population. In addition, interest attaches to niching, niche-overlap, and measures of competition across subpopulations. By keeping the economist's notion that the environment changes and selects the most efficient organizations, but abandoning rational adaptation, organization ecologists are able to measure "niche-width," competition, and similar concepts using straightforward data. This is in marked contrast to the difficulty in developing proper empirical measures of economic concepts, such as cross-elasticity of demand, or strategic management concepts such as "mobility barriers."

Of course, the fact that some organizations do adapt creates a natural tension within organization ecology. A natural response is to shift the locus of selection from the firm as a whole

[4] More precisely, organization ecologists assume strategic change is infrequent and independent of immediate environmental demands.

to some part of the firm, say its policies or its organizational subunits, and to see these subunits as unchanging but also subject to birth, proliferation, and extinction (Burgelman, 1990). It remains to be seen whether an empirical demography of policies or subunit forms can be created.

New institutionalism. The basic tenet of economics, much of strategic management, and a great deal of sociology and organization theory is rationality or functionalism—that the structures, concepts, and social arrangements which evolve are the "rational" or "efficient" solutions to the problems of production, coordination, and change. These functional structures are either designed, selected, or otherwise evolve. It is this view that motivates case studies of successful firms and that lies behind the economic analysis of institutions.

The new institutionalism[5] provides a contrary view. It claims that whereas some organizations survive through technical efficiency, there are others that survive through legitimacy—by acting in socially expected ways. Put differently, whereas an economist might see a joint venture as an arrangement for efficiently dealing with certain forms of co-specialized assets in a context of opportunism, the (new) institutionalist would see it as a currently accepted (or rationalized) activity. Joint ventures are undertaken, it might be argued, because other firms have done them and because academics have rationalized them. Hence, more firms undertake them. From this point of view, joint ventures may be something like a virus, multiplying in the social, cognitive, and economic context of modern corporate life. They proliferate not because they are efficient, but because they have become *institutionalized* (and are not obviously dysfunctional). As DiMaggio and Powell (1988: 3) put it, "The distinguishing contribution of institutional theory rests in the identification of causal mechanisms leading to organizational change and stability on the basis of preconscious understandings that organizational actors share, independent of their interests."

The new institutionalism is an intellectual descendant of the old, which was best represented by Selznick (1957). But Selznick used the word to denote the way value and meaning attached to a specific organization and its mission. To the new writers,

[5] The best survey is Powell and DiMaggio (1991), especially their introductory essay.

society at large is the source of concepts, professional roles, rules, standards, expectations, policies, strategies, and standard organizational arrangements. Organizations institutionalize (adopt) these things and thereby gain legitimacy. Thus, business schools teach business policy because it is "the thing to do" rather than because it is technically necessary. This form of nonrational behavior draws on new work in cognitive psychology that identifies behavior derived from unconscious scripts, rules, and routines. It is also akin to that studied by Elster (1988), who distinguishes between consequentialist and nonconsequentialist behavior: the first is action impelled by a consideration of consequences or payoffs; the second is action chosen according to a rule or according to its "appropriateness" in the context. Finally, and perhaps most simply, it should be recognizable as the logic of the legal system—procedural rationality.

Institutional theory is at its strongest in explaining those aspects of organizational life that are taken for granted. For example, the fact that superiors judge the performance of subordinates but not vice versa, the annual planning cycle, and the general use of financial measures of subunit performance are "institutions" which are accepted virtually without question. This viewpoint has obvious bite with regard to the diffusion of many new management concepts and fads (e.g., quality circles, value-based strategy, and TQM).

Another developing stream of thought that intersects organizational sociology in many areas is concerned with organization culture.[6] The study of organization culture derives from functional anthropology, semiotics, and phenomenology. It tends to reject reductionism, seeing culture as something that must be comprehended as a whole, and perhaps only by direct participation. It is a nonrational view to the extent that social behavior cannot be expressed as the outcome of individual rational optimizing behavior. Some scholars, however, do view the culture as a whole as the functional solution to problems of communication, cooperation, and intertemporal opportunism. Culture is sometimes seen as the impediment to change and at other times as the source of unusual excellence; in either case, the technology of changing, protecting, or creating culture is at a very primitive state of development.

[6] See Ouchi and Wilkins (1985) for an insightful review.

Political Science

The systematic investigation of political structures and processes has a tradition extending back to the Greek philosophers. And most political science has been within the classical form: the discussion of ideal states, the histories of particular political conflicts or events, descriptions of political structures and the rules governing their operation, and framers' expectations as to the value and functioning of various political structures. Like strategic management, political science lacks a central, generally accepted paradigm, and its many streams are not tied together in any coherent way.

However, two dramatic shifts in paradigm have occurred in American political science in the last 50 years. The first was the "behavioral revolution" that commenced in the 1950s. Just as the Carnegie School's views on behavioral, rather than "rational," models of human behavior influenced research and thinking in management schools, they also had an impact on political science. The new mode saw researchers looking at what political actors actually did rather than at descriptions of rules and structure or at a framer's expectations. For example, see Kaufman (1960) for a fascinating account of the Forest Service.

The second paradigm shift was political science's own "new institutionalism." Among its antecedents were the many empirical studies of voting that had been carried out over the years. These studies examined the effects of blocs, splinter groups, rules, and so forth, on voting behavior and outcomes. New institutionalism in political science also included abstract and rigorous analysis of how individual preferences combine through voting to produce political outcomes. This avenue of study had its origins in Arrow's (1951) Impossibility Theorem, which showed that literally centuries of talk about "the public interest" was vacuous—that one cannot aggregate preferences and treat a collection like an individual. The early attempts to model democratic processes took their structure from economics; voters were likened to consumers, policy makers to producers, and politics and voting to market competition. Of the many important contributions to this literature, two that stand out are Black's (1958) analysis of bloc voting and Buchanan and Tullock's (1962) analysis of when collective democratic action is individually preferred.

One might expect an economic metaphor applied to politics to produce the same conclusion—that competitive markets maximize welfare—but political scientists discovered substantial difficulties. When preferences were modeled as differing in only one dimension, everything worked well, with the policy outcome being the preferences of the median voter. But with two or more dimensions to preferences, outcomes were indeterminate. McKelvey (1976) is credited with the first "chaos theorem," proving that if there is no clearly dominating policy, any policy can be made the outcome through some adjustment of the agenda. That is, given majority-rule voting, a sufficiently clever chairperson can obtain any result he or she desires.

Although analysis showed chaos, real political institutions demonstrated substantial stability and predictability. Thus, the actual outcome of democratic processes, or at least their stability, it was argued, must be at least as greatly determined by the structure of institutions as by the preferences of voters. This insight generated a renewed interest in the structure of institutions, and a great deal of research has been done on the committee structures of the U.S. Congress and on its voting rules (Shepsle, 1979; Weingast, 1989).

A final stream of political science research of interest to strategic management is the study of bureaucratic biases or failures. The studies in this stream most familiar to strategic management are Selznick's (1949) study of the TVA and Allison's (1971) analysis of the Cuban missile crisis. Other important works in this genre are Downs's (1967) study of bureaucracy and Wilson's (1989) analysis of the properties and behavior of government agencies.

Summary

Each of the allied disciplines speaks to a unique metaphor. Economics is concerned with public welfare and wealth distribution in society. Sociology is concerned with groups of individuals and their activities as groups. Political science is concerned with choices made by groups where the objective function is diffuse and specified by the group itself. Psychology is concerned with individuals, the mind, and individual behavior. That all of these have something to do with individuals in combination with group choices and welfare is evident. But what of strategic management? What is its metaphor, and what is its domain? And how does it relate to these basic disciplines?

Strategic management has to do with groups, their birth and their continuing success. It does not assume that the group's purpose is beneficial, but simply that the group forms and tries to exist because it has purpose. Moreover, the group exists within a context, and the context governs conditions of success. It is management's responsibility to see that the group adapts to its context, and survival in the end is an objective definition of success. So the perspective is that of the management team assigned the responsibility of ensuring success, with success defined as either the entrepreneurial act of starting an organization, or those acts that condition survival.

The fundamental issues addressed by strategic management then are different from those addressed by the allied disciplines themselves. Related they are to be sure, but different perspectives separate their domains of inquiry, and one must expect different fundamental questions to be addressed by each discipline.

We turn now to an examination of what we see as fundamental questions of interest to strategic management. In so doing, we define the field as we see it today, and further, we separate the field from the allied disciplines with which it overlaps and with which it has common interests. Perhaps most important, by posing fundamental questions, we outline what we believe to be the boundaries of the field and its current and ongoing agenda for research.

The Fundamental Questions in Strategy

The Value of Fundamental Questions

A fundamental question acts to define a field of inquiry and to orient the efforts of researchers who work in that domain. Ronald Coase, for example, defined the field of institutional economics by asking, Why are there firms? Despite its apparent simplicity, this question has great power. When it was posed, the neoclassical theory of the firm was well advanced, but that theory sought to characterize the behavior of the firm, taking its existence as a given. Thus, Coase's question was a subtle critique of the state of microeconomics, and it proved to be an extremely fruitful impetus to new thinking. The value of the question is undiminished by the fact that it has not yet been answered entirely satisfactorily.

Fundamental questions are not necessarily the most often stated or the most fashionable; nevertheless, they serve to highlight the issues and presumptions that differentiate a field of inquiry, making its axioms, its methods, and the phenomena it studies different from those of other related fields. Thus, one of the fundamental questions for the strategy field is, Why are firms different?—a question that echoes Coase's but that directs attention away from common properties of all firms and focuses instead on the phenomena which produce and sustain continuing heterogeneity among firms.

For a fundamental question to energize research it must not only address a critical issue, it must also offer at least one clear path to follow in seeking answers. Thus, the power of Coase's question derives not only from its focus on the basics, but also from its association with a method for seeking its answer—comparative institutional analysis. Adam Smith invented economics by coupling the question, What determines the wealth of nations? with a new method of analyzing the collective results of individual self-seeking behavior. Durkheim virtually invented sociology by asking, What binds individuals into societies? and advocating the statistical comparison of behavior and social structure in various settings.

There are many questions addressing important issues that cannot motivate useful research. Some are too general; others lack connection to any usable theoretical structure or research methodology. For example, the question, What is good management? is too general, given our current knowledge, to be fundamental. We know that managerial work depends sharply on the task, the role, and the organization. In business research there is also the problem of asking questions that are too close to the entrepreneurial heart of the matter. In a competitive world, there can be no general answer to the query, How can a firm increase its market share? because if it were really general, it would also apply to the firm's competitors.

What questions energize research around a critical issue in the strategy field? And which of these provide a clear path to follow in seeking answers? Clearly, we need to understand how firms and organizations in general make assumptions and decisions about context. In other words, we need to learn more about just how organizations reach conclusions about action, and whether they can in fact be more rational than the individuals that comprise them. Certainly none of the basic and allied disciplines we have examined tells us how firms behave. All seem either to be passively descriptive or to postulate behavior from the outset.

We can also wonder how competition among organizations influences their nature. Among competitive business firms, we see a variety of successful firms with very different natures. Yet theory would suggest this should not be true. No good explanations exist for the difference.

There are concerns about the role of senior management that span several strategically distinctive business firms. Why are they needed and what do they do? All theory available suggests they add costs without corresponding value. Yet they persist, and though we know much about their activities, which are typically strategic in character, we don't know enough about the value they create.

As the world shrinks, competition intensifies, environments grow more complex, and it becomes increasingly difficult to survive. We know too little about these complex processes on an international scale, and we know too little about competitiveness. Nothing in theory much helps to explain this dynamic process of birth, survival, and death.

These kinds of concerns lead us to four fundamental questions that we believe characterize major concerns of the strategic management field. We now turn to their specific development.

The four questions addressed in this volume represent some of the most crucial puzzles in the strategy field, and emphasize the links between strategy and its allied disciplines. While other questions can be posed, almost all relate in one way or another to the four developed here. To satisfy the curious, we include a brief summary of some of the "also-ran" questions in the Afterword.

How Do Firms Behave?

Or, do firms really behave like rational actors, and, if not, what models of their behavior should be used by researchers and policy makers?

Strategy is about the choice of direction for the firm.[7] But what assumptions should the strategist entertain about the choices made by competitive firms, choices that inevitably are interdependent? Is it even reasonable to think of the behavior of a firm as reflecting "choices," or should a much less rational model be used? Thus, the question of how firms behave has two components: (1) the empirical issue of the actual patterns of behavior observed among firms, and (2) the more abstract question of what modeling assumptions are most fruitful in explaining observed patterns or guiding competitive strategy.

The dominant assumption used by economists is that the firm behaves like a rational individual. Therefore, the question, How do firms behave? directs attention toward situations in which the dominant assumption is unwise. Since there is good empirical evidence that individual behavior does not meet strict norms of rationality, even when it is intendedly rational, and since most firm behavior reflects organizational outcomes rather than individual action, it is a reasonable conjecture that the standard rational model of firm behavior is rarely accurate.

The subquestions that appear appropriate to this fundamental question are these:

- What are the foundation assumptions that differentiate among various models of firm behavior (e.g., resource dependence, "garbage can" models, Nelson and Winter's routines, and population ecology models)?
- According to behavioral decision scientists, there are predictable biases in human decision making. Are there predictable biases in firm or organizational behavior? What do we know about the relationships between organizational size (or other stable characteristics) and behavior?
- "Rational" models of competitive interaction posit players who engage in very subtle and complex reasoning. Yet our common experience is that decision makers are far less analytic and perform far less comprehensive analyses than these models posit. If one is a player, is it really "rational" to posit such complex behavior in others?
- Can game theorists deal with biased behavior or with the

[7] Clearly, the organization does not have to be a business firm. Any type of organization could be substituted in the remarks made here and those that follow.

nonrational aspects of firm behavior? Can analytic models of nonrational or extrarational behavior move beyond their present ad hoc status?

Why Are Firms Different?

Or, what sustains the heterogeneity in resources and performance among close competitors despite competition and imitative attempts?

One of the key empirical observations made by strategy researchers, an observation as well as a perspective that sets the strategy field apart from industrial organization economics, is that firms within the same industry differ from one another, often dramatically. In a recent study, Rumelt (1991:179) found that among businesses in the FTC Line of Business sample, the variance in return on capital could be apportioned as follows: 0.8% due to corporate effects, 8.3% due to stable industry effects, and 46.4% due to stable business-unit effects. Thus, the differences among business units within the same industry were eight times as great as the differences among industries. The source of this heterogeneity lies at the root of competitive advantage, and understanding *why* it arises and translating that into *how* it can be achieved is of central concern to the field.

For those who do not accept the idea of equilibrium, there is, of course, no puzzle in heterogeneity—people differ and so must firms. But competition, it is normally thought, should eliminate differences among competitors; good practices and successful techniques will be imitated, and firms that cannot or will not adopt good practices will be driven from the field. Therefore, the challenge is to retain the power of equilibrium thinking and still correctly explain the observed differences among competitors.

Differences among firms may arise from intention, or stochastically, and they may be created and sustained through property rights, active prevention of imitation, or through natural impediments of limitation and resource flows. In addition, these differences may also arise and be sustained through differing conceptual views, theories, or causal maps, differing organizational processes within firms, different levels of organizational learning and team skills, and/or through the action of ambiguity.

There are many different theories that can be used to deal with this question. The following subsidiary questions suggest the range of these theories and the underlying disciplines that may have something to offer:

- To what extent are the differences among firms the results of purposeful differentiation rather than unavoidable heterogeneity in resources and their combinations? That is, should strategy be thought of as the exploitation of existing asymmetry, or the search for and creation of unique resources or market positions?
- Are the most important impediments to equilibration rooted in market phenomena (e.g., first-mover advantages), or are they chiefly rooted in internal organizational phenomena (e.g., cultural differences or learning)?
- Is the search for rents based on resource heterogeneity contrary to public welfare, or does it act in the public's welfare?

What Is the Function of or Value Added by the Headquarters Unit in a Diversified Firm?

Or, what limits the scope of the firm?

The diversified corporation is the dominant form of business firm in the industrialized world. The creation and management of these enterprises has been heavily researched by strategy scholars. Nevertheless, the relative strengths and weaknesses of this organizational form remain poorly understood. In particular, the question of what is, or should be, the value added by the headquarters unit of such firms is of central concern to the strategy field.

There appear to be two general points of view with regard to the role of the headquarters unit in multibusiness firms: the first emphasizes value creation, and the second emphasizes loss prevention. According to the first viewpoint, the headquarters unit formulates the overall strategy for the corporation, including its degree of diversification and organizational form. Also, it manages the process of resource allocation among constituent businesses, apparently better than would the unaided capital markets. Finally, the headquarters unit maintains the existence of key shared resources and manages the processes by which business units share these resources.

By contrast, the loss prevention school of thought sees management as reviewing the strategies of the business units (strategic management), apparently to make sure that egregious logical errors are not made. Second, the headquarters unit monitors the operations of the subunits, providing surer supervision of the agents operating the businesses than would independent boards of directors or the competitive marketplace. Finally, the headquarters unit can extract free cash flow from a mature business unit at much lower cost than can the unaided capital markets or the market for corporate control.

There are, of course, perspectives beyond these two. Financial economics suggests gains from corporate diversification if bankruptcy is costly, and transaction cost economics suggests gains from internalizing businesses sharing co-specialized assets. Finally, there is a skeptical perspective that sees these complex firms as the result of agency problems—as long as managers prefer to invest excess cash rather than pay it back to stockholders, and as long as they can do so, maturing profitable businesses will spawn diversified firms.

The persistence of multibusiness firms cannot be ignored. However, it is no trivial task to isolate the forces that generate and sustain these firms. The subsidiary questions that may aid inquiry into these considerations include:

- Which is primary, strategy or structure? That is, is the multidivisional form (M-form) the administrative solution to the problems created by product-market diversification and/or the need to internalize transactions, or is it itself the innovation that permits efficiencies from the assembly of various business units in a common hierarchy?
- Which is primary, the entrepreneurial (value-creating) role of the headquarters unit, or the administrative (loss-preventing) role? Can a headquarters unit simultaneously perform both roles?
- What, if any, are the limits to the amalgamation of business units in multibusiness firms? Relatedly, how can the value now being created by the breakup of diversified firms be reconciled with the value created by their formation in the past?
- Strategic management is normally taken to mean the explicit oversight and review of the strategy formulation pro-

cess together with systems for allocating resources among businesses. Do firms that impose "strategic management" on portfolios of businesses add value, and if so, what is the mechanism?
- Are there corollaries to headquarters units in nonbusiness organizations and, if so, what are the comparative lessons to be learned?

What Determines Success or Failure in International Competition?

Or, what are the origins of success and what are their particular manifestations in international settings or global competition?

This question has two parts of interest. One part is the more fundamental issue of why some firms enjoy more success than others. What is the dynamic competitive process that leads to the relative success of some firms, and what causes some to decline and some to fail (or, more commonly, to be sold to other firms that more efficiently employ their assets)? This issue is at the heart of competitive dynamics and the workings of capitalism and needs to be understood better in its own right.

Another part of the question deals with international competition and the competitiveness of firms, and indeed, of nations and cultures. At stake is not just firm survival or success, but the quality of life in economies and their respective cultures.

There are a number of disciplines that can shed light on these fundamental issues, including international trade theory, political science, and organizational theory. Subsidiary questions that need to be addressed include these:

- To what extent do firms from different countries (cultures) possess inherent competitive advantages in certain arenas? The issue at stake is not simply the economists' comparative advantage, which would not operate when a domestic firm invests abroad, but a subtler set of management skills, technologies, and norms of work.
- Are there "strategic" industries and, if so, what makes them strategic? That is, are there significant positive externalities associated with the presence of a particular industry within a nation? Note that the often postulated efficacy of

Japan's MITI or an "industrial policy" in the United States rests on the presumption that there are strategic industries.
- Are there rules for global competition that are not simply the extension of rules for competition within a large nation-state or continent?

Summary

These four questions help define the field of strategic management. In our view they are fundamental to understanding the matter of managing groups, their formation or birth, their relative success, and ultimately their adaptation and survival. These questions relate to allied disciplines, but they are not central to them, and their perspectives differ.

The next four sections of the book present the papers that deal with each of the four questions, and the papers in turn present the perspectives of authors whose disciplines are not necessarily those of strategic management. Collectively, the perspectives offered raise research questions and practice issues we believe can help set important research and practice agendas within the strategic management field.

Part I

How Do Firms Behave?

The only well-worked-out, crisply predictive, and internally consistent theory of firm behavior is that of rational maximizing behavior—the economist's model. Unfortunately, most students of strategy and organization believe it is wrong. The dominant view outside economics is that although organizational actions can usually be individually rationalized by various interested parties, the actions are not consistent, nor can they be expressed, taken as a whole, as the consequences of maximizing choices. It is this split that has generated most of the heat and friction between the perspectives of economics and organizational studies.

This split in perspective has taken on a new shape in the last six or seven years because of two phenomena: the takeover boom of the mid-1980s, and the rise of game theory. To rationalize the takeover boom with theories of market efficiency, financial economists have been forced to argue that many managements may not be acting to maximize the value of the firms they control. As a consequence, the market for corporate control, working via takeovers, acts to replace such inefficient managements (see Shleifer and Vishny, Chapter 14, this volume). The intriguing result of this line of argument is that large numbers of firms may not be acting to maximize wealth and, therefore, do not act like rational "individuals." At the same time, another branch of economics has made great strides in applying game theory to competitive situations. As a consequence, there is a rapidly expanding literature on strategy in which firms are presumed to act with great rationality in complex competitive situations. Thus, economics no longer speaks with one voice on the question of how to describe the behavior of firms.

In this section, a group of researchers tackles the problem of describing firm behavior. Jay Barney ("Beyond Individual Metaphors in Understanding How Firms Behave: A Comment on Game Theory and Prospect Theory Models of Firm Behavior") leads off by criticizing the general tendency to use "individual metaphors" in modeling or simply in thinking about firm behavior. In particular, Barney challenges the practice of modeling firm behavior "as if" the firm were a rational individual, or "as if" it were an individual described by prospect theory. Barney argues that such models miss the essential point that firms are collections of individuals. As Freud recognized, the simplest model of irrational and inconsistent behavior is a "committee" of three rational individuals: superego, ego, and id. Larger "committees" are no more rational, and Barney argues that a realistic theory of firm behavior must confront the inconsistencies inherent in the behavior of collectives. In addition, Barney offers the argument that the individual metaphor may incorrectly focus attention on the "big" or strategic decisions. Real competitive success, he notes, may derive from a myriad of "small" decisions which would more appropriately be described collectively as culture or process.

In the next paper, "Timid Choices and Bold Forecasts: A Cognitive Perspective on Risk Taking," Daniel Kahneman and Dan Lovallo provide a model that, at first glance, appears to be exactly the type Barney attacks: an extension of individual cognitive models to the firm as a whole. Kahneman and Lavallo offer the fascinating proposition that individual and social psychological phenomena lead firms to produce optimistic forecasts of the future and to simultaneously avoid making choices and commitments. The cleverness in this argument, one that defuses most of the Barney critique, is that it joins a theory of expectations with a theory of action, and the combination predicts inconsistent behavior.

In the third paper, "Structure, Strategy, and the Agenda of the Firm," Thomas Hammond offers a theory of firm behavior drawn from political science. In particular, Hammond argues that organizations act to shape the information and choices that managers see; thus, structure has a strong influence on the types of decisions that are made. Hammond's model is clear and offers testable propositions about firm behavior, propositions that differ substantially from those produced by strictly rational models.

The next three papers address the connections between modern game theory and strategic management. Garth Saloner, in his paper, "Game Theory and Strategic Management: Contributions, Applications, and Limitations," 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. 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 gaining 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 strategic 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. There is no escape, he suggests, from using judgment on this matter, and he 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 ("Does Strategy Research Need Game Theory?") 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, "Burning Your Britches Behind You: Can Policy Scholars Bank on Game Theory?" 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 for generating explanations of behavior. Postrel uses humor to show 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 structures. Other than rationality, the substantive theory present in a model is in the assumptions, not in the mechanics of the solution.

A significant goal of this section is to question what assumptions managers are willing to make about behavior—not merely individual, but also group behavior—and the influence these assumptions have on resulting choices. Clearly, there is much yet to be understood about group behavior and how we can think about the basis for the choices that organizations make. If there is any message at all in the recent interest in worldwide competitive behavior, and particularly the recent success of Japanese firms, it is that (often implicit) assumptions made (or not made) about organization behavior have a powerful impact on ultimate success. This section attacks this matter at a fundamental level and should lead all of us to rethink how we approach management of organizations.

2
Beyond Individual Metaphors in Understanding How Firms Behave: A Comment on Game Theory and Prospect Theory Models of Firm Behavior

Jay B. Barney

Metaphors have long been an important analytical tool for understanding how firms behave. Metaphors help organize and communicate complex descriptions of organizations and organizational phenomena, and can suggest important insights about those phenomena that may not be accessible through more traditional analytical means (Pepper, 1942; Kuhn, 1962). In organization theory, for example, the "garbage can" metaphor has had an important influence on the study of decision making in firms (March and Olsen, 1976). In finance, the "nexus of contracts" metaphor enabled theorists to begin to examine intraorganizational phenomena using agency theory (Jensen and Meckling, 1976). Recently, Morgan (1986a) has suggested a wide range of metaphors for organizations, and has traced many of the interesting implications that these metaphors have for our understanding of organizations and organizational phenomena.

Though not strict examples of metaphorical reasoning (Morgan, 1986a), the papers in this section are nevertheless grounded in two different metaphors of organizations. The game theory papers suggest that organizations can be analyzed as if they were rational (perhaps even hyperrational) utility-maximizing individuals. For game theorists, the question of how firms behave in competitive situations becomes, How would hyperrational individuals behave in these situations? The prospect theory paper adopts a different metaphor when it suggests that organizations can be analyzed as if they were systematically nonrational individuals. For prospect theorists, the question of how firms behave in competitive situations becomes, How would biased decision makers behave in these situations?

Each of these metaphors for strategic analysis has a long tradition, and each has been shown to be fruitful in certain theoretical and empirical contexts. The rational individual metaphor has been at the core of most (but not all) economic analysis of organizations (see Simon, 1947, for a discussion of alternative rationality assumptions). It helps focus theoretical attention on the relationship between a firm (as a utility-maximizing entity) and its competitive environment, and thus enables the application of the powerful analytical tools of game theory and price theory in strategic analysis.

While a somewhat more recent development, the biased individual metaphor of prospect theory has also been shown to have important implications for strategic analysis. This metaphor goes beyond suggesting that perfect rationality can be costly and that individual decision makers have limited information processing capabilities (Simon, 1947) to suggest that there are some very strong psychological biases built into the decision-making process that make purely rational decision making virtually impossible (Kahneman and Tversky, 1979a). The growing body of experimental evidence in this area is impressive (Kahneman, Slovic, and Tversky, 1987).

Though separate groups of scholars have continued to research the implications of these two metaphors for strategic management for at least several decades, it would be incorrect to suggest that these views have not come into conflict. Indeed, the entire development of the biased individual metaphor in prospect theory can be thought of as an elaborate criticism of the hyperrationality metaphor (Kahneman and Tversky, 1979a). Adherents to these different metaphors continue to argue for the importance of simplicity and parsimony (for the rational individual metaphor) versus the importance of empirical validity and richness (for the biased individual metaphor).

While there is little doubt that at one level of analysis these two metaphors for how firms behave come into direct conflict, at another level they are in fundamental agreement. The agreement concerns the appropriateness of studying the strategic behavior of organizations as if organizations were individual decision makers. Each of these metaphors discounts differences in the decision-making calculus (rational or biased) used by different individuals within an organization. Since all individuals within an organization are assumed to be homogeneous in their decision making (homogeneously rational or homogeneously biased), it becomes a reasonable simplification to analyze organizations as if they were individuals, and to use logics originally designed to study individual decision making to study strategic actions of firms, e.g., game theory and prospect theory.

There is, beyond convenience, some justification for adopting the "as if" assumption in these two metaphors. In the case of the rational decision-making metaphor, there was until recently no theoretical or empirical alternative to the rational decision-making model that was sufficiently specific and rigorous to allow the kind of analytical work that was the objective of game theorists and other economists (Hirshliefer, 1988). While theories of organizational behavior provided alternative views on motivation (e.g., equity theory [Adams, 1963]; expectancy theory [Vroom, 1964], most economists would argue that these theories were either not sufficiently specific to allow rigorous theoretical work, or were special cases of utility-maximizing approaches. In the case of the biased individual decision-making metaphor, empirical work suggests that the biases that have been discovered are so pervasive, that the assumption that all decision makers are systematically biased in the same ways, and to the same extent, seems a justifiable simplification (Kahneman, Slovic, and Tversky, 1987). In both cases, concluding that individual differences in decision making within a firm are either uninteresting or unimportant has made it possible to use the metaphors to analyze strategic actions taken by firms as if they were individual phenomena.

Although the as if approach to organizational analysis has numerous benefits, it has liabilities as well. Metaphors enlighten certain aspects of organizations by darkening others. While the as if assumption enables game theorists and prospect theorists to make strong statements about the strategies firms will pursue in different situations, it also eliminates several phenomena that may be important for understanding the competitive actions and reactions of "real" organizations. This paper examines three important limitations of analyzing the strategic actions of firms as if firms were individuals. First, I discuss how the as if metaphor focuses attention on "big decisions" in organizations as sources of competitive advantage. Next, I examine how this individualistic metaphor assumes away intraorganizational contradictions that can have an important impact on strategic behavior. Finally, I discuss how the use of this metaphor eliminates broad classes of intraorganizational phenomena as possible sources of competitive advantage.

Big Decisions and Competitive Advantage

Analyzing firm strategic actions as if firms were individuals tends to focus on big decisions made by organizations as sources of competitive advantage. Big decisions in organizations are decisions that define an entire organization's relationship with its competitive environment. Game theorists focus on these big decisions when they discuss how one organization (taken as a whole) responds to the competitive actions of another organization (taken as a whole). Prospect theorists focus on big decisions when they suggest that the strategic decisions made by firms (taken as a whole) will be systematically biased.

In focusing on big decisions as strategy, both game theory and prospect theory reinforce a long tradition of research in strategic management. The tradition began at the Harvard Business School, with research on the role of general managers in large organizations (Learned, Christensen, Andrews, and Guth, 1965). This research adopted the same kind of as if assumption used in game theory and prospect theory. However, instead of firms being analyzed as if they were hyperrational individuals or biased individuals, firms were analyzed as if they were general managers, and the decisions made by general managers were assumed to determine the long-term success of a firm. While general managers might rely on other line and staff managers, ultimately general managers themselves held the future of their corporation in their hands. In this framework, understanding how firms behave becomes understanding how general managers behave.

This emphasis on general managers has had important pedagogical implications. In many of the cases used in capstone business policy courses, the task is to analyze how a particular general manager came to make the big decisions he (they usually are men) did, and what implications these big decisions had for a firm's performance. The case is an attempt to force students to ask the question, If I had been in this person's place, what decisions would I have made, and what impact would they have had on this firm's competitive position? Whether it is Howard Head at Head Ski (Christensen, Andrews, Bower, Hamermesh, and Porter, 1987), John Connelly at Crown, Cork, & Seal (Christensen et al., 1987), or Marcel Bich at BIC Pen Company (Christensen et al., 1987), the emphasis in much of traditional strategic management research has been on the big decisions made by "important general managers." The message of these cases has been clear: if you want to positively affect the competitive position of the organization you work for, you too must become a general manager.[1]

One reaction to this almost exclusive emphasis on the importance of the general manager for understanding how firms behave is Porter's (1980, 1985) work on the impact of a firm's environment on its competitive position. Porter suggests that the strategic options facing an organization are significantly constrained by the structure of the industry within which the firm conducts business. The competitive environment places constraints on the impact of general managers in a firm, and thus on how firms behave.

However, even incorporating the Porter framework into strategic analysis has not led to the complete abandonment of studying organizations as if they were general managers. General managers, in Porter's framework, are still assigned the responsibility of analyzing their firm's competitive situation and choosing a strategy to steer a course through that competitive environment. The strategic decisions these managers make, whether they concern product differentiation, cost leadership, or focus, define an entire organization's relationship to its competitive environment, and are still the big decisions of classic strategic

[1] This emphasis on general managers in case analysis is perhaps most completely revealed in the series of cases on International Harvester (Christensen et al., 1987). The first case describes problems requiring the attention of a "strong" general manager. The second case identifies this general manager, Archie McCardell. Accompanying this case is a video recording of McCardell giving a speech at the Harvard Business School, where he receives a 15-minute standing ovation from the students. The third case chronicles the steady deterioration of International Harvester and McCardell's role in that deterioration. In all the cases on International Harvester, the general manager, whether hero or villain, is represented as the person whose decisions ultimately determine the fate of the firm.

management research. If a general manager chooses wisely, the firm can gain competitive advantage. If a general manager chooses less wisely, no competitive advantage is forthcoming.

There is little doubt that big decisions made by general managers can sometimes have an enormous impact on the performance of a firm. Bich's decision to invest in low-cost pens, despite five years of losses, had a huge impact on BIC. Connelly's emphasis on customer service had a similar impact on Crown, Cork, & Seal. Head Ski's inability to exploit Howard Head's inventive genius significantly hurt that organization's performance.

However, big decisions made by general managers may not be the only source of competitive advantage for a firm. Indeed, such big decisions may not even be the most important sources of sustained competitive advantage. Consider, for example, a firm like Wal-Mart. It is obvious to everyone that Wal-Mart follows a low-cost leadership strategy. Thus, the big decision made by Sam Walton to be a low-cost leader is not, by itself, proprietary, nor is it a source of competitive advantage. To the extent that Wal-Mart enjoys a competitive advantage, that advantage does not reflect just the big decision to be a low-cost firm, but rather the hundreds of thousands of "small decisions" that operationalize a big decision on a daily basis. Other discount retailers can easily duplicate Wal-Mart's big decision to be a low-cost firm, but still fail to duplicate the hundreds of thousands of smaller decisions that make the big decision real.

From a competitive point of view, small decisions have at least two advantages as potential sources of competitive advantage. First, these kinds of decisions, because they are so "small" and so numerous, are virtually invisible to those outside a firm (Itami and Roehl, 1987; Barney, 1992). Even managers inside a firm may not be fully aware of the cumulative impact of these kinds of decisions (Polanyi, 1962). When competing firms are unable to describe the basis of a particular firm's competitive advantage, because that advantage is based on thousands of small decisions, that competitive advantage may be immune to competitive duplication and imitation (Reed and DeFillippi, 1990) and thus may be sustained over time (Lippman and Rumelt, 1982).

Second, it may be the case that competitive advantage depends not on any one, or any subset, of these small decisions, but rather on the interrelated set of these decisions. Other firms may be able to imitate one or two, or even a majority of these small decisions, but may find it more difficult to imitate the entire set of interrelated decisions. Because this system of small decisions works as a whole, it may be less subject to imitation and duplication, and thus a source of sustained competitive advantage (Barney, 1991; Dierickx and Cool, 1989).

Consider, for example, attempts by U.S. auto manufacturers to duplicate the quality and cost positions of Japanese auto manufacturers. Firms like GM have invested billions of dollars based on a series of hypotheses about what makes the Japanese plants so effective. One hypothesis was that Japanese robots were the source of the advantage. After several billions of dollars of investment there was some improvement, but GM found itself using world-class robot technology to manufacture mediocre cars. Another hypothesis was that it was the Japanese just-in-time inventory system. GM rearranged its supplier relationships and inventory management systems. Again, there was some improvement, but the Japanese were still ahead. Several other hypotheses were used as a basis for additional investment (e.g., quality circles, labor-management relations, and the location of plants in rural areas). While each one of these changes in manufacturing improved the situation, what became clear was that the source of advantage was not in any one of these attributes of Japanese automobile plants, but in the simultaneous implementation of all of them.

Recent work on lean manufacturing suggests that it is the simultaneous combination of several factors that enables a manufacturing facility to be both very high quality and very low cost (MacDuffie, 1989; MacDuffie and Krafcik, 1990). This complicated system of numerous interrelated, mutually supporting small decisions is difficult to describe, and even more difficult to imitate, and thus a source of sustained competitive advantage (Barney, 1992).

Another way to think about the emphasis on big decisions in strategic management research is to observe that this kind of research tends to focus on strategy formulation and ignore strategy implementation. The emphasis on strategy formulation implies a belief that the decisions made about which strategies a firm should pursue will have a definitive impact on a firm's competitive advantage. Recognizing that small decisions may be more important for understanding competitive advantages than big decisions suggests that the study of strategy implementation—the process by which big decisions are translated into operational reality—may be more important for understanding competitive advantage than the study of strategy formulation.

Harold Demsetz (1973) once observed that sometimes it is very difficult to know why some firms do better than others. Competitive advantage can often be found in this causal ambiguity (Reed and DeFillippi, 1990). To the extent that the as if metaphors of game theory and prospect theory simply perpetuate the field's historical emphasis on big decisions made by general managers, the impact that numerous small decisions can have on competitive advantage will be lost.

Intraorganizational Contradictions and Firm Behavior

Another limitation of as if assumptions in game theory and prospect theory models of how firms behave is the simple model of individuals used in these metaphors. In game theory, individuals (and thus organizations) are assumed to rigorously and unerringly apply precisely the same decision calculus (utility maximization) in making decisions. There is no doubt, no hesitancy, no internal conflict and disagreement, only unalterable application of an established decision calculus. The decision calculus in prospect theory (biased rationality) is perhaps more complicated than the rationality assumption of game theory. However, the application of that biased rationality decision-making process is assumed to be just as unerring, just as internally consistent, just as predictable. In its application to organizations, internal contradictions (e.g., those that occur when different parts of the same organization are biased in different ways) are not emphasized. The individuals described in game theory and prospect theory are less like the individuals described by Freud (1953) and more like those described by Skinner (1953). These simple models of individual decision making are translated, by the as if metaphor, into rather simple models of how organizations behave.

If one desires to maintain an individualistic metaphor in analyzing firm behavior and decisions, perhaps an additional metaphor (beyond "firms as rational decision makers" or "firms as biased decision makers") could be "firms as individuals with multiple personality disorder." This metaphor admits the possibility that, within a single organization (individual), several distinct, often conflicting "personalities" might exist, that the content of decisions and actions taken by firms depends upon which personality happens to dominate at a particular time, that who will be dominant in decision making is essentially unpredictable, and that different personalities within an organization may be more or less skilled and able to make quality decisions (Kluft, 1987). Like individuals afflicted with multiple personalities, organizations with multiple personalities may sometimes appear to be perfectly rational, at other times to be biased rational decision makers, and at other times to be non-rational decision makers, and they may be continuously contradicting themselves over time (Ross and Gahan, 1988; Spanos, Weekes, and Bertrand, 1985).

The organization imagined by such a metaphor is not nearly as neat and easy to analyze as the organizations imagined by game theory and prospect theory. However, the multiple personality metaphor may be more appropriate in some situations, and it certainly raises a broad set of issues that are relevant in the study of how firms behave and that are not easily accessible to analysis using more traditional metaphors.

Of course, just as there are some individuals who are internally consistent and single-minded, there may be some organizations that behave as if they had no internal conflicts or contradictions. However, research in organization theory suggests that these organizations are the exception rather than the rule (Morgan, 1986b; Simon, 1947). More typically, different individuals and groups within an organization vary in their commitment to courses of action (Staw and Ross, 1988; Bowen, 1987). Some of this variance in commitment reflects the allocation of resources and rewards implied by a particular decision. Marketing and sales managers, for example, will often be very supportive of implementing a product differentiation strategy because such a strategy puts a premium on the skills they possess. Manufacturing managers may be less enthusiastic about product differentiation strategies, but strong supporters of a cost leader strategy. Other conflicts within an organization may reflect individual differences in experience, intelligence, and taste, and be independent of traditional organizational categories. Certain decisions may be preferred by some managers while contradictory decisions may be preferred by other managers.

Analyzing firms that contain numerous subgroups pursuing a variety of potentially contradictory agendas certainly complicates the study of how firms behave. Instead of firms behaving as if they were rational, or biased, or irrational, and so forth, researchers face the difficult problem of studying firm behavior as if it were simultaneously rational and biased and irrational.

One solution to this problem has been proposed by several organizational theorists (Pfeffer and Salancik, 1978). These theorists recognize the multiple, conflicting groups within an organization, and then focus on the political processes that unfold within firms and that enable firms to make decisions. And, once again, these researchers apply the as if assumption. In this case, however, firm decisions are analyzed as if they were the decisions of the "dominant coalition." Studying how firms behave thus involves (1) understanding how a particular group becomes part of the dominant coalition, and (2) examining how that dominant coalition makes decisions.

This solution to the multiple personality metaphor of organizations has many attractive properties. For example, it recognizes intraorganizational processes without losing the parsimony associated with as if kinds of analyses. However, this approach also makes some strong assumptions about the impact on firm behavior of groups that are not part of a firm's dominant coalition. Much of this work assumes that if a group is not part of the dominant coalition, it no longer has a significant impact on firm behavior. When researchers study firms as if they were a dominant coalition, they are implicitly assuming that those groups and individuals outside the coalition are not important for understanding how firms behave.

This simplification is inconsistent with the multiple personality disorder metaphor of organizations. The metaphor suggests that "losing" groups or individuals (i.e., those not in a firm's dominant coalition) continue to have an impact on firm behavior. These "other personalities" resist change and continue to pursue their own agendas, quite independent of the wishes of the dominant coalition (Staw and Ross, 1988). In other words, dominant coalitions are rarely as dominant as our simplified models would suggest. From the outside, firms do not appear to be implementing a single consistent strategy over time; rather they appear to be implementing several strategies, each of which may be internally consistent but which contradict one another. Sales managers may be trying to pursue a low-price, high-volume strategy, while marketing managers may be trying to pursue a low-volume product differentiation strategy, while accounting managers are focusing their efforts on reducing a firm's tax liability, while human resource managers are most concerned with meeting EEO requirements. How does this firm behave? Like a person with multiple personality disorder.

At first, it might appear that this kind of schizophrenic organization will be at best a temporary phenomenon, since more focused firms—even if they are biased in their rational decision making—will generally have a better chance of surviving over the long run (Friedman, 1953). And this may be true in industries characterized by little uncertainty, where optimal strategic paths can be chosen and implemented (Alchian, 1950). However, in a highly uncertain context, a multiple personality disorder may be a source of creativity, innovation, and random variation (Woodman and Schoenfeldt, 1990), which may enable this type of firm to survive where a more focused, less creative organization may be selected against by the environment (Hannan and Freeman, 1977). When a firm cannot know for sure what the "best" thing to do is, doing lots of different things at the same time can increase the chance of stumbling onto a good strategy (Kogut, 1991). Peters's (1987) description of a chaotic, playful organization includes many of the characteristics of multiple personality disorder described here—and he suggests that this structure is essential for success in highly turbulent environments.

Moreover, even in industries characterized by low levels of uncertainty, the time period needed to generate an equilibrium of all focused, single-personality firms can be substantial (Nelson and Winter, 1982). If we are to understand how firms behave, we must at least admit the possibility that some firms simultaneously pursue multiple contradictory strategies. To the extent that game theory and prospect theory models of strategy adopt relatively simple models of individuals as metaphors for the firm, this kind of behavior will remain inaccessible to systematic analysis.

As If Assumptions and Firm Heterogeneity

A final limitation of as if assumptions in game theory and prospect theory models of strategy focuses on the assumption of firm homogeneity implicit in these models. In game theory models, firms (as individuals) are assumed to accurately perceive the actions of competing firms, and then to calculate a response based on some well-defined objective function. In prospect theory models, firms (as individuals) are assumed to perceive the actions of competing firms in a biased manner, and then to calculate a response based on some well-defined, but not fully rational, objective function. Differences across firms in these models are limited to differences in their position in the competitive environment and differences in objective functions.

Models of firm behavior that assume high levels of interfirm homogeneity have recently become popular in strategy research. Most of this work is derived from industrial organization economics (Conner, 1991). The original objective of industrial organization (IO) economics was the identification of the structural attributes of industries that either facilitated or prevented the development of perfect competition (Porter, 1981). Thus, the level of analysis in IO economics was the industry, and firm differences within an industry were not a primary concern (Rumelt, 1984; Barney, 1991). As applied in strategy research, this emphasis on the impact of industry structure on average firm performance has continued (Porter, 1985). Models of the impact of industry structure on firm performance either assume that firms within an industry are homogeneous, or that firms within the same "strategic group" within an industry are homogeneous (Barney, 1991). When these models attempt to make statements about firm heterogeneity within an industry, they largely abandon their IO economic theoretical roots (Porter, 1985).

While models that assume firm homogeneity within an industry (or group) have grown in popularity, they are not fully consistent with the strategy formulation problem as it has been traditionally posed in the field (Christensen et al., 1987). Historically, strategy researchers have looked both to a firm's competitive environment and to its internal strengths and weaknesses in order to understand strategic actions. In the traditional framework, how firms behave was seen as depending both on the firm's competitive environment and on the unique resources and capabilities it possessed. By adopting the assumption that competing firms are relatively homogeneous, game theory and prospect theory models fail to acknowledge the importance of firm heterogeneity in determining behavior.

Recently, there has been a resurgence of interest in understanding the sources and competitive implications of firm heterogeneity. The models developed to analyze firm heterogeneity are collectively referred to as the resource-based view of the firm (Barney, 1991). This view generally adopts two assumptions about firms: first, that firms within an industry may possess different strategically relevant skills and capabilities and, second, that these differences can last over time. Skills and capabilities that enable an organization to conceive of, choose, and implement strategies that exploit environmental opportunities or neutralize environmental threats are strategically valuable. If few competing firms possess these resources, exploiting them can give a firm a competitive advantage. If firms without these resources face a cost disadvantage in obtaining them, and if there are no substitutes for these resources, they can be sources of sustained competitive advantage.

Applications of this resource-based logic are growing. It is even possible to apply this logic to game theory and prospect theory models of strategic behavior. For example, a resource-based game theory model would begin with the assumption that firms are fundamentally heterogeneous, in terms of the objective functions they pursue, the skills and abilities they bring to bear in maximizing their objective functions, and the strategies they can conceive of and implement in response to their competition. Indeed, several game theory models have been developed where firms are presumed to be heterogeneous in terms of some underlying key attribute (e.g., some firms are more "honest" than other firms [Axelrod, 1984], some firms are able to act more quickly than other firms [Leiberman and Montgomery, 1988], and so forth).

Prospect theory models that adopt a more resource-based view would also reflect firm heterogeneity. Thus, some firms might be dominated by one kind of cognitive bias, while other competing firms might be dominated by another type of cognitive bias. There is, for example, some recent research that suggests that entrepreneurs are systematically subject to some kinds of biases to a greater extent than managers in large organizations

(Busentiz, 1992). Thus, the behavior of firms dominated by entrepreneurs might be systematically different from the behavior of firms dominated by managers in large organizations. How these different kinds of firms would interact in a competitive setting is an interesting question.

It is not true, then, that game theory and prospect theory approaches to understanding how firms behave are incapable of being generalized to include firm heterogeneity. The models are probably general enough to include such work. However, to capture firm heterogeneity, these models will have to be extended in directions where they have traditionally not been extended.

To accomplish this generalization, both game theory and prospect theory will have to adopt an even more microanalytic perspective. This call for more microanalytic research may seem ironic, since game theory is a microeconomic technique, and prospect theory is grounded in cognitive psychology. However, the micro kind of analysis that is required would recognize that individuals (and by analogy, firms) can be fundamentally different, and that theories of how firms behave must include this heterogeneity in the analysis. Thus, future research will not only have to examine the implications of different decision-making calculi on individual and firm behavior, but also how different individuals and firms have different decision-making calculi.

Conclusion

There is little doubt that both game theory and prospect theory have a great deal to add to our understanding of how firms behave. The metaphors that firms behave as if they were rational individuals and as if they were biased individuals have obviously generated numerous important insights in strategy and related fields. However, additional progress in understanding how firms behave may depend at least in part on abandoning the comfort and convenience of these as if assumptions. Once abandoned, they are replaced by different kinds of game theory and prospect theory models of firm behavior. Some possible models have been examined here, including models of firms that focus on the cumulative impact of thousands of small decisions, models of firms that adopt a multiple personality metaphor, and models of firms that recognize firm heterogeneity and its impact on competitive advantage.

There are obviously many other ways that the as if assumptions of game theory and prospect theory can be modified. There is also a great deal of work to be done in tracing the implications of retaining these two metaphors. The next decade of research on how firms behave will depend, in large part, on how these metaphors are both extended into new areas and abandoned in favor of alternative metaphors.