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Author(s): Christopher A. Bartlett and Sumantra Ghoshal

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GLOBAL STRATEGIC MANAGEMENT: IMPACT ON THE NEW FRONTIERS OF STRATEGY RESEARCH

CHRISTOPHER A. BARTLETT

Harvard University, Graduate School of Business Administration, Boston, Massachusetts, U.S.A.

SUMANTRA GHOSHAL

INSEAD, Fontainebleau, France

Historically, North American companies have suffered a major handicap as they expanded internationally—they were located in the world's largest, richest and most sophisticated market. This benign curse led managers in a large number of U.S.-based multinational corporations (MNCs) to regard their international operations somewhere along a spectrum between attractive sidebet and distractive nuisance. At best they were thought of as organizational appendages that generated incremental revenue, but whose role was tangential to the mainstream of corporate strategy.

In some ways this attitude was reflected in the academic community. In the management field, international research was often treated as a specialized and rather esoteric field. It had its uses: just incorporating the new dimensions of the global environment forced some researchers to rethink their comfortable models; for others, studying the practices of non-U.S.-based companies illuminated often subtle culturally biased assumptions about strategies or organizations; and for many the added complexity of the international environment provided a 'stress laboratory' for testing certain assumptions developed in the process of analyzing domestic markets. Overall, however, while some of these efforts were considered interesting, most were

regarded as having marginal relevance to the mainstream of the field.

It was the powerful and dramatic impact of foreign competition, particularly from Japan, that jolted awake most American managers, and with them, students and analysts of management and firm behavior. What followed was a decade when managers, consultants and academics alike made 'global' one of the most overused adjectives in the business lexicon. As we were flooded with definitions and analyses of global competition, global strategies, and global organizations, it became increasingly clear that rather than representing a special case, the study of industries, strategies and organizations in their global context needed to be regarded as the norm.

With this change, the fields of international business and strategic management have begun to find increasingly common ground, particularly as researchers in both fields began to focus on the development and management of global strategy. Indeed, as we read through the papers submitted for this Special Issue, we became aware that the most interesting and provocative had gone beyond trying to define the field of global strategy, identifying the ways in which it differed from domestic strategy, or testing traditional models in a global environment. Instead, they used their own findings as well

as the accumulating body of concepts and frameworks that have emerged from recent research in the field of international management to challenge some of the established assumptions and theories that dominate the strategy field. As a result, one of the criteria we used in selecting the final group of papers from those that filtered through the review process was that the authors not only had an important contribution to make in the area of global strategy, but also that they helped define and inform the broader debate on strategy research.

As those two fields converge, it is important to understand how research findings in each informs and stimulates the other. A brief review of the evolution of research in these two areas of inquiry will highlight the differences in the origins and traditions of the two fields. In doing so, we will emphasize the contributions and lessons international business brings to strategic management (just as we would focus on the opposite if we were writing for the *Journal of International Business Studies*).

FROM INTERNATIONAL BUSINESS TO INTERNATIONAL MANAGEMENT

The international business (IB) area has become increasingly interesting to scholars of strategic management primarily for the body of knowledge that has been accumulated in its extensive and wide-ranging literature. But some have also begun to see important differences in the way the two fields have developed. Probably because it has evolved from the work of scholars from a variety of functional areas, IB seems to have developed a broader and more eclectic theoretical base and has established stronger traditions of cross-disciplinary integration. Such lessons may well be the most important ones that researchers in strategic management can learn from their new international colleagues.

Like strategic management, IB is a relatively young academic field. Until the 1960s, most IB research was focused on trade flows between nation states, reflecting the field's roots in classic macroeconomic theories and, particularly, in the theory of comparative advantage. But, beginning with Stephen Hymer's seminal thesis (1960), the field began to pay increasing attention to patterns of foreign direct investment triggered by the rapid post-war expansion of MNCs. On this

foundation the field began to develop in many different, complementary streams—sometimes flowing along in parallel, sometimes merging, and other times branching off from each other. Building directly on Hymer's contribution, a wave of research in the 1970s led to increasingly sophisticated explanations of foreign direct investment in terms of oligopolistic competition among firms (e.g. Kindleberger, 1969; Caves, 1971; Knickerbocker, 1973; Graham, 1974). Meanwhile, Raymond Vernon (1966) and his students were moving the focus down from the industry level to the level of the firm. Vernon's product cycle theory linked the flows in international trade and international investment by focusing on the observed behaviors of MNCs. By the late 1970s, influenced by the work of Oliver Williamson (1975), an important new stream of research emerged that moved the focus further within the firm—into its internal processes of information transfer—to explain the existence and behavior of MNCs (e.g. Buckley and Casson, 1976; Rugman, 1981; Hennart, 1982).

Over a period of a decade and a half the field had shifted its focus from the international economy to the firm and, even within the firm, to its internal organizational processes. Rather than compartmentalizing the diverse approaches, this shifting focus led to a broader and more eclectic approach to the phenomena. The progression into lower and lower levels of analysis only emphasized the need for a multi-level theory to provide a more complete explanation of the increasingly complex patterns of foreign direct investment by multinational companies (Dunning, 1977, 1988).

Simultaneously, a parallel stream of international management (IM) research was developing with a stronger administrative focus. Aharoni (1966) examined the process of foreign direct investment from a more managerial perspective, while Stopford and Wells (1972), Franko (1976), Dyas and Thanheiser (1976) and others extended Chandler's work (1962) to examine the strategy and structure of MNCs. Taking the perspective to an even more managerial level, a group of students of Joseph Bower built on the research traditions of the so-called 'process school' to examine management actions and strategic processes in MNCs (e.g. Prahalad, 1975; Doz, 1976; Bartlett, 1979). Another group was pursuing similar work under Lars Otterbeck's

supervision at the Stockholm School of Economics (Hedlund, 1978; Leksell, 1981).

Developing in parallel, these diverse strands of research in the areas of IB and IM were forced to draw on each other to explain the complex multi-level phenomena that were shaping MNC actions in a rapidly changing global economy. In the course of this merger, it became manifest that foreign trade could not be analytically separated from foreign investment which, in turn, had to be understood in the context of environmental forces (at both industry and societal levels) as well as strategic, organizational and managerial factors (at both the firm and intra-firm levels).

The result of this sometimes confusing intersection, overlap and complementarity of work, is a field of great diversity and dynamism. Grounded in a wide range of disciplines and functional applications, yet willing to draw freely from each other's findings, researchers are creating eclectic and multi-disciplinary frameworks that are reflected in a number of articles in this volume. In both the rich multi-faceted concepts as well as in the collaborative model of building theory, the IB/IM field has some important contributions to make to those in the adjacent strategic management area.

FROM BUSINESS POLICY TO STRATEGIC MANAGEMENT

In contrast to the IB/IM area, the roots of strategic management (SM) are most clearly traced back to the strong administrative traditions of the business policy (BP) literature, and, in sharp contrast to developments in the IB/IM area, research in this field has increasingly migrated from a managerial focus to higher levels of analysis. In this process we believe that the SM field has become narrower and more compartmentalized and, in its search for stronger and more rigorous theory, risks losing some of the richness that once was its hallmark.

The BP/SM field was built on intellectual foundations laid by Barnard (1938), Selznick (1959) and Chandler (1962), reinforced by the concept of strategy, explicitly borrowed from the military. Authors such as Ansoff (1965) and Andrews (in his text for the widely used casebook, Learned *et al.*, 1965) defined many of the concepts that would later be developed and

refined in more formal research.

Of this early work, the Andrews (1971) model probably became the most influential, shaping the views of a full generation of students. Perhaps its greatest contribution was to articulate a concept of corporate strategy that was unusually broad and inclusive. The relevant environment was defined in terms of the economy, technology, ecology, industry, society and politics; strategy encompassed both the broad patterns of purposes and policies defining the company and its business as well as the more specific choice of product markets; and organization was conceptualized not only in terms of structure, processes and leadership, but also as the unique portfolio of corporate resources and distinctive competence they embodied.

Over the years this concept of business policy was subjected to increasing challenge and criticism. On one side, practitioners wanted to reduce the complexity and generality to more applied and quantifiable tools. On the other, academics wanted to move the broad normative models toward a more explicit discipline-based theory of strategy. Both gave birth to new initiatives in the development of the field.

In response to the former demands, the decade of the 1970s saw the development of increasingly sophisticated strategic planning processes. Representative of the proliferation of highly quantified and prescriptive portfolio models and factor-analytic tools were the BCG growth-share matrix (Henderson, 1973) and the PIMS model (Buzzell *et al.*, 1975). By the close of the decade, however, there was growing disillusionment that something as complex as corporate strategy could be reduced to boxes and bubbles or regression coefficients. The frustration was evident at a 1977 conference organized by Dan Schendel and Charles Hofer, which was held at the University of Pittsburgh under the auspices of the Business Policy and Planning Division of the Academy of Management. This conference marked an important renewal and revitalization of strategic management (including formalizing this new name for the field) and triggered a round of empirical research that brought it academic respectability and recognition.

By the 1980s the initiative had passed fully from the consultants to various academics who were determined to provide the field with new concepts that provided more robust taxonomies

and testable relationships. Perhaps the most influential was Michael Porter (1980), who made important contributions by linking the theory of industrial organization (IO) to the business policy traditions represented by Andrews and others.

This industrial organization-based view of strategy significantly enriched the environmental analysis dimensions of the Andrews model, particularly in analyzing a firm's industry structure and competitive position. In doing so, however, most IO economists' focus led them to view industry structure as the primary determinant of the competitive rules of the game, and thus of firm strategy. The internal organization, competence and resource variables that were the other half of the Andrews framework became peripheral to their analysis. Unfortunately, the economists' prodigious efforts were not matched by more administratively oriented strategy researchers, and the field developed a strong IO orientation.

Nonetheless, the tools these researchers brought to bear on this promising area of study were powerful. As a result the decade of the 1980s saw the development of new concepts of strategic management that were considerably more refined and rigorous than the underdeveloped and often flimsy concepts of the early writers. The field finally began to develop a stronger theoretical base, clearer concepts, and testable hypotheses.

But the progress came at a price. In contrast to the broad and encompassing view of strategy that involved effective matching of external environmental analysis with internal organizational capabilities, the successful interlinking of formulation and implementation, and the creative development of an interdependent strategy and structure, the new streams of research tended to have a narrower and more focused perspective. Thus, just as the environment came to be conceived primarily in terms of a detailed industry analysis (Porter, 1980), the previous rich view of strategy was also simplified to become something considerably less, like interactive competitive games (e.g. Karnani, 1984) or product-market positioning (Day, 1981). And the broad perspective of organization as a portfolio of resources assembled to build distinctive competence was often ignored or perhaps reduced to the classic yet mechanistic M-Form hierarchy model (Williamson, 1975). As a result a field once

distinguished by its breadth, scope and managerial relevance became academically elegant, but increasingly fragmented and compartmentalized.

This was not the only cost of progress. The ascendancy and eventual dominance of economists in the field seemed to totally eclipse those doing administratively oriented research. In some circles there was great skepticism of findings not derived from large-sample statistical research, and any field-based clinical research was particularly suspect. In this environment the role of management was either ignored or accorded only passing reference, as research on strategic groups, risk-return relationship, and diversification focused on the firm as an abstract economic entity (and often as a black box) rather than as a social institution with an economic purpose.

Even within organizational theory the contingency models that had long focused attention on the strategy-shaping organizational and management processes were being challenged by population ecologists and institutionalization theorists who joined the industrial economists in focusing on the environment to explain performance differences. By implication, the role of management in making strategic choices and in building organizational capability became far less relevant in such models of environmental determinism.

Unlike the trend in IB/IM, the research focus, level of analysis, and range of research methodologies in the SM field increasingly became quite narrow, and multiple-level, cross-disciplinary, or other integrative research approaches were limited. Not surprisingly, as research has moved further along the spectrum towards mathematical game theory (Shapiro, 1989) and population ecology (Lambkin, 1988), those who are concerned that we may be moving too far from our roots as students of management are calling for a re-evaluation of the field. The most powerful voices for a redirection have come from two sources, and in both, scholars working at the intersection of international management and strategic management are making important contributions.

The first of these is a group whose work has come to be referred to as the resource-based view of strategy (see Collis' paper in this volume for a brief review of this literature). These researchers are refocusing attention on the forgotten half of the Andrews model—the mar-

shalling of internal resources to develop distinctive competence. They point to evidence that firm-specific resources and capabilities provide much stronger predictors of performance than industry characteristics (Cool and Schendel, 1988; Rumelt, 1991), and argue that we need to focus our attention on understanding how managers assemble unique portfolios of resources and develop distinctive competencies and capabilities that provide a source of sustainable competitive advantage.

The second group calling for a new focus in the field consists of researchers who are loosely referred to as belonging to the process school (see Bower and Doz, 1979 for a review of the roots of this literature, and Doz and Prahalad's article in this volume for references to more recent contributions, particularly in the context of multinational management). This group is contesting the externally focused view of strategy that reduces firms and management to black boxes and argues instead for an internal view to describe and analyze the strategy-making process. Strategy, they claim, cannot be separated from its context, which includes not only external environmental demands but also internal organizational processes and the factors, like quality of management, culture, and history that shape those processes.

CHALLENGES FOR THE MERGING FIELDS

These concerns, we believe, represent the triggers for the next round of evolution of the strategy field. The challenge is not one of reverting to the frameworks of the 1960s. Instead, the key task now is to retain and build on the firmer theory-grounding that has been achieved while broadening our scope and perspective to create an even more integrative and managerially relevant field. Such a change would require the strategy field to expand its discipline base and its research focus, to complicate its models, and to legitimize a wider range of research approaches than are currently used. It is here that intersection with the IB/IM field can open up a somewhat broader and more eclectic theory base and expose researchers to a tradition of cross-discipline integration and tolerance for methodological

variety. Such stimulus, we believe, can help trigger and facilitate this next round of evolution of the field of strategic management.

For example, the notion of competitive advantage being based in differentiated resource portfolios is a very familiar one to a field that counts Ricardo as one of its earliest contributors. As the macroeconomists who dominated the early development of the IB field were subsequently joined by microeconomists and eventually by managerial researchers, this foundation in the theory of comparative advantage has remained a distinct feature of the field. Through such collaboration this underlying resource-based view of competitive advantage has been translated across units of analysis from nation states to industry to firms. Not surprisingly, this emerging view of strategy has found one of its richest areas of application in understanding the strategic behaviors of multinational companies (Prahalad and Hamel, 1990). Because of the strikingly different portfolios of resources and capabilities that are developed by companies nurtured in different economic, political, and social environments, the international context clearly offers a rich opportunity to develop this important new research stream.

Similarly, the multinational corporation provides a particularly rich context for developing the more administratively focused view of strategy as suggested by the process scholars. When organizational units are separated by large barriers of distance and time, and managers are isolated by differences in language and culture, the administrative challenges become both complex and highly observable. It is not surprising, therefore, that much of the recent work on organizational and administrative processes in large companies has been carried out within the field of international management (e.g. Prahalad and Doz, 1987; Hedlund, 1986; Bartlett and Ghoshal, 1989), and this body of work provides a basis for further research on the strategy process.

The papers in this volume provide ample evidence of these contributions that international management research can make to the development of the strategy field. These papers deal with broad, overlapping issues and they often span multiple levels of analysis. Any effort to categorize such a diverse and rich set of articles inevitably runs the risk of doing injustice to the authors and their contributions. Yet, to organize the

volume and to relate the papers to the broad argument we have presented, we will describe them in three categories corresponding to three important research domains in the field of strategic management: the environment and environment–strategy interactions, strategy and competitive advantage, and organization.

ENVIRONMENT AND ENVIRONMENT–STRATEGY INTERACTIONS

In characterizing and operationalizing the environment, strategy researchers over the past decade have been deeply influenced by Michael Porter's assertion that 'although the relevant environment is very broad, encompassing social as well as economic forces, the key aspect of the firm's environment is the industry or industries in which it competes' (1980: 3). This primacy accorded to industry structure and the attending de-emphasis on the broader economic technological, social, and political aspects of the environment was premised on the assumption that these broader environmental forces affected all firms more or less equally and were, therefore, 'significant primarily in a relative sense'. In extending his analysis to the domain of global competition, Porter (1986) provided a conceptual framework that retained microeconomics as its main focus and the industry as the primary level of analysis. This perspective and the frameworks used to conceptualize it triggered a tradition of IO-based analysis of the causes and consequences of globalization of business and became the basis for much of the empirical work on this topic.

The rich body of findings that resulted from this research stream is strong evidence that the IO lens has been an extraordinarily powerful one through which to examine the environmental forces shaping global strategy. But, like most powerful lenses, it has limitations: it focuses only on part of the phenomenon being observed, and it tends to blur or distort one's vision of the objects at the periphery of its focus. The papers in this Special Issue contribute to our understanding of the firm's relevant environment either by refocusing our view through traditional optics, or by letting us observe familiar phenomena through new conceptual lenses.

The paper by Stephen Kobrin makes a particularly important and provocative contribution to the IO-based literature by challenging some of its basic assumptions. Based on his analysis of fifty-six manufacturing industries, Kobrin argues that it is technology intensity and not manufacturing scale that is the primary determinant of cross-border integration. While this finding is of particular significance to future research in the area of global competition, it should also catch the attention of strategy scholars in general, who have paid too little attention to the impact of technological change on industry structure or firm strategy.

Researchers in the field of international management have also been more willing to view the world from perspectives other than the IO-based view that has dominated domestic strategy research. Focused as they are on competition among firms of different national origins, whose developments have been shaped by very different economic, social, political, and cultural milieus, they have been relatively more sensitive to the influence of environmental factors that lie beyond the boundaries of specific industries. Therefore, while acknowledging the importance of industry structure as a driver of firm strategy, they have made an increasingly compelling case for returning to the broader and more encompassing definition of the environment to include not just the economic and competitive attributes of the industry but also the social, political, and cultural characteristics that so clearly influence company strategy, organization, and management in the global arena.

The research note by Franke, Hofstede, and Bond builds on this tradition by arguing that economic performance may result, at least partly, from differences in national culture. The support of this argument is based on an empirical analysis of economic growth during the periods 1965–80 and 1980–87 for two samples of eighteen and twenty countries. In another article, Kogut explores a related phenomenon—the persistent competitive differences among countries over long periods of time—and highlights the role of institutional structures in influencing economic performance of societies. By highlighting the ways in which non-economic factors, beyond the boundary of specific industries, influence economic and competitive outcomes for both

nations and firms, both papers challenge us to broaden our perception of the relevant strategic forces in a company's operating environments.

Some of the papers in this volume also challenge the traditional view of how the environment influences firm strategy and action. Given their disciplinary roots, the IO-based strategy models tended to be relatively static and a-historical. Since the environment was viewed as an exogenous reality, strategy became an analytical exercise of adapting to changing environmental demands. Despite some notable efforts to model the role of history in industrial organization and competition (e.g. Kreps and Spence, 1983), an overwhelmingly large proportion of theoretical and empirical work continued to reflect a zero-based view of strategy in which little attention was paid to the effects of internal organization, the firm's culture, or history as moderators of the process of environmental adaptation.

Once again, by focusing on firms that are products of very different cultures and histories, research on global strategy has increasingly revealed the fallacy of such a zero-based and a-historical view of strategy. These studies have highlighted the continuous and on-going interactions between organizational and environmental forces that shape a firm's strategy and actions.

Both David Collis analysis of the global competition in the bearings industry and Bruce Kogut's explanation of persistent differences in country competitiveness make compelling cases for such a dynamic and historically based view of the process of environmental adaptation and change. Collis analysis shows how differences in their cultural heritages and physical infrastructures led Minebea, SKF and RHP—three competitors in the bearings industry—to respond very differently to the same set of environmental forces they confronted. Kogut's model of country competitiveness is similarly based on the notion of path dependency that influences the pattern of evolution of 'organizing principles' of nations which, in turn, serve as the bedrock of their international competitive advantage. In essence, both papers echo an argument made by Andrews over twenty-five years ago: that the 'creative art' of matching environmental opportunities with a company's distinctive competence and other internal resources establishes its economic mission.

STRATEGY AND COMPETITIVE ADVANTAGE

As influential as they were in the area of environmental analysis, the economists' most profound impact on the SM field was in the analysis and understanding of firm-level strategic behavior. By grounding their research in oligopoly theory supported by the attending discipline of econometric estimation, they brought to this topic a level of rigor and quantification that had previously been absent. Yet there was a cost. Rooted as they were in a theory of markets, industrial economics-based strategy models tended to view strategy almost exclusively in terms of a firm's actions in its output markets. A theory of excess profits was grounded in the theory of market imperfections. As a result, the mechanisms for building, protecting and exploiting such imperfections in favor of the firm became the conceptual anchors for research on strategy content.

Increasingly, this exclusive focus on output markets has been questioned by the emerging resource-based perspective to which we referred in the introductory section. While some authors have represented this view in competitive terms vis-à-vis the more mainstream economic paradigm, we share the view expressed by Collis in this volume that this new framework for strategic analysis supplements and enriches the traditional IO-based models and does not supplant them. We also believe, however, that building this internally focused part of the Andrews model to the same level of rigor and precision as the externally oriented part has been built represents one of the more exciting opportunities for strategy researchers in the 1990s.

Almost all the papers included in this volume contribute to this resource based view of strategy in one way or the other. In fact this may well represent the thread that holds these papers together. Some, however, ground their arguments in this emerging perspective more explicitly than the others.

David Collis analysis of the global bearings industry is explicitly grounded in three concepts—administrative heritage, core competency, and organizational capability—and he demonstrates how these concepts can be applied, in an internally consistent way, to understand and

explain the strategic behaviors of firms. His analysis demonstrates the compatibility of these arguments, as well as the benefits of combining such an internally focused analysis with the external orientation of industry and competitive analysis.

Stephen Tallman's paper, while very different in its objectives and methodological approach, shares a common point of view with that of Collis. Tallman compares the resource based perspective directly with both the industrial economics and transaction cost based theories of the firm to explore the strategy, structure and performance linkages for multinational corporations. His analysis of competition among firms of European and Japanese origin in the U.S. automobile market not only demonstrates the power of the resource-based perspective to explain differences in strategic approaches and market share performances of firms, but also suggests how application to the context of global competition can potentially enrich and enhance this new perspective.

The papers by Gary Hamel and by Nitin Nohria and Carlos Garcia-Pont apply the resource and competency perspective to the analysis of a relatively new phenomenon of considerable significance: that of large companies cooperating with one another through the formation of strategic alliances and networks. While Hamel's focus is on dyadic relationships within an alliance, Nohria and Garcia-Pont investigate the broader web of relationships that are emerging to link groups of competitors from diverse national origins.

While focusing on a common phenomenon—that of collaboration among competitors—the two papers adopt very different methodologies and levels of analysis to pursue very different objectives. Hamel's research is aimed at understanding the process of skill transfer within an alliance, and the factors that influence this process. Nohria and Garcia-Pont, in contrast, aim to explain the pattern of relationships that are formed by competitors. Despite such differences, their overall conclusions are very similar: alliances are formed to acquire new skills and competencies, and the networks of alliances that emerge as a consequence represent efforts by a group of firms to match the collective capabilities of other groups.

ORGANIZATION: STRUCTURE, PROCESS AND PEOPLE

If theories of industrial economics provided the anchors for the field's conceptualization of environment and strategy, structural contingency theory has played a similar role in our analysis of organizational attributes of companies. As a result, in much of the strategy literature, organization has come to be viewed in purely structural terms. Following Chandler (1962) and Williamson (1975), the concept of structure has been further simplified to a generalized configurational model, the M-form, which has come to dominate the strategy literature as the representation of the complex organization required to manage diversified strategies.

We described how the field's grounding in IO led us to focusing most of our efforts on understanding how the environment affected firm strategy (while always acknowledging, then disregarding, the converse influence). In the same way, our training in structural contingency theory led most strategic management research to be premised on the assumption that strategy should guide structure (while parenthetically recognizing, then ignoring, the possibility of structure influencing strategy). With this bias, structural change has been viewed primarily as a management tool for implementing strategic change.

Again, research in the field of global strategic management has exposed scholars to situations that caused them to question each of these premises. The complexity of organizations that require management to integrate operations and coordinate decisions across multiple national environments quickly highlight the inadequacies of analyzing organization in purely structural terms. It is no accident that the primary locus of the process school of general management research has been focused on the management of multinational enterprise, and this body of work, described in Doz and Prahalad's article in this volume, has strongly emphasized the need to view complex organizations as networks of relationships that are influenced by formal and informal administrative processes. Formal structures are neither complete nor particularly adequate representations of these relationships and processes.

This importance of management processes in the implementation of strategy is the topic of the paper by Chan Kim and Renée Mauborgne in this volume. Drawing on the tradition of justice-based research and the associated conceptual and empirical literature in the fields of social psychology and law, these authors show that the quality of the strategy generation process and, more specifically, the extent to which the process is perceived to be fair and just, affect the levels of commitment, trust and social harmony among subsidiary managers in multinational companies and, thereby, the effectiveness of strategy implementation in these companies. While Kim and Mauborgne's empirical work is rooted in the context of MNCs, their arguments apply equally to all large and dispersed firms and their conclusions are therefore likely to be of interest to the broader community of academics, managers and students of strategy.

The second limitation of the traditional strategy–structure approach which was revealed by recent research on multinational corporations relates to the applicability and usefulness of the M-form model as a representation of today's complex organizations. While this conceptualization was perhaps adequate for reflecting the organization of General Motors as it was reshaped by Alfred Sloan, large corporations in the 1990s have evolved to a level of complexity which the hierarchical M-form fails to capture. In particular, different geographic or product divisions within the same company increasingly tend to be structured and governed differently, in response to differences in their environmental context and strategic focus. Further, these divisions also tend to be increasingly linked to one another through both sequential and reciprocal interdependencies. Symmetrical hierarchies are giving way to organizations that could better be described as differentiated networks. The stylized representation of the M-form, as operationalized in the strategy literature, underemphasizes or even ignores the important new organizational issues of internal differentiation and interdependence.

The paper by Doz and Prahalad raises this and many other issues to challenge the relevance and applicability of present organizational paradigms for describing or explaining the behaviors of large and complex organizations, such as MNCs. Drawing on their extensive field research in a wide

variety of American, European and Japanese MNCs, these authors review seven major strands of organization theory to suggest that, while each or at least most of the theories can be useful for dealing with some specific and relatively narrow set of questions, none of them are robust enough to serve as a broad umbrella to guide MNC-related research. Finally, these authors describe a 'new paradigm' that is evolving from the work of a group of process scholars in the field of multinational management and suggest that this new paradigm is relatively more useful for research on such complex organizations.

THE NEXT FRONTIER

The nine articles in this Special Issue provide a fair representation of the variety and richness of current research on global strategic management. The papers draw on theories from a diverse range of disciplines—economics, sociology, psychology, social psychology and law, to name a few. They also apply a wide range of methodologies from clinical research in a few companies to statistical analysis of large data sets. Most significantly, they point toward a panoply of new research issues at the levels of environment, strategy, and organization that are likely to be of interest to a wide body of scholars in the strategy field.

While we have discussed some of these research implications in the earlier sections, one issue remains that we believe deserves special attention. In reading these nine papers, and many others that we received, we were repeatedly struck by the realization that the concepts, hypotheses and findings they described had some important implications for managers. Yet, while we received a relatively large number of papers that focused on the environment and on strategy and a somewhat smaller proportion that dealt with organizational issues, we received only one paper that explored the roles and tasks of managers in the new environmental, strategic, and organizational context. (This paper, unfortunately, could not be revised adequately in time for this volume.) If this paucity of submissions reflects the amount of research attention being given to this topic (and from all we know this appears to be the case), it is our firm conviction that herein lies one of the key challenges and most important

opportunities for strategy researchers in the 1990s.

Perhaps the most powerful impact of global strategy research will result from the magnitude and rapidity of change that it has helped us describe and understand. The dramatic changes that have occurred in environmental forces, industry structure and firm behavior are individually and collectively having a profound impact on managers' jobs at every level of the organization. In this context a persuasive case can be made that the job of the general manager in today's multinational corporation is fundamentally different from that of his or her counterpart in the same company in the 1960s.

Andrews described a general manager who was, in most respects, an heroic figure who determined the company's strategic direction, delegated responsibility for implementation, and monitored results and rewarded performance. Although there was some dispute about the degree to which that role was executed through deliberate strategic planning versus logical incremental actions (Quinn, 1980), the task was largely defined in terms of the comparatively constrained and stable corporate environment in which the general manager was assumed to operate in that era.

The management context described by authors in this Special Issue is vastly different. The globalization of markets, technology, and competition has increased both the scope and the dynamics of management's relevant operating environment (see the papers by Collis, Kogut, and Kobrin), straining top management's ability to perceive, let alone understand, all the information vital to the company's strategy. Firm boundaries, once assumed to be clearly defined, have become increasingly fuzzy and permeable, as companies create a complex network of relationships with suppliers, customers, governments, and competitors that greatly complicate ability to develop and deploy vital resources and capabilities (see Nohria and Garcia-Pont, Hamel). Even within the corporation, the clear command and control structure of the classic hierarchy has been eroded by de-layering and transformed by the power of new information technologies into a network form where the management of horizontal information flows is at least as important as control over the classic vertical processes (see Doz and Prahalad, Kim and Mauborgne).

Yet despite the major impact that these changes have had on management tasks, roles, and responsibilities, very little work in our field has focused on these issues or even on this level of analysis. Scholarly researchers may fleetingly acknowledge the importance of this change in the obligatory 'implications for managers' paragraph near the end of the paper. Sometimes, however, these briefly outlined proposals suggest changes that alter the fundamental nature of the manager's task. Those writing directly to practitioners are equally culpable. After suggesting that companies need to undertake massive strategic changes, organizational restructurings, or cultural transformations, they inevitably conclude that 'such changes must be initiated and managed from the top.' If top-level general managers were to follow the collective and cumulative advice of even a handful of these gurus, their working life would quickly grind to a halt in a gridlock of people, plans and paperwork.

As a result, while managers and students of management are developing an increasingly sophisticated view of the strategic imperatives of their environment, the alternatives for building sustainable competitive advantage, and the organizational requirements for creating such capabilities, they have very little guidance on the implications for managerial action. We now need to spend more time understanding the impact of our findings on the manager's job. The research that led to Mintzberg's (1973) landmark book on this subject occurred over two decades ago. In the light of the massive changes that have taken place in the corporation and the environment in which it operates, it is clearly time to pick up this neglected research agenda. It represents an enormous gap in our knowledge and understanding of strategic management—one that provides a vital link in transmitting our findings to managers and students of management who will need to develop the new skills and capabilities we will describe.

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When we accepted Dan Schendel's invitation to edit this Special Issue we had no idea of the amount of work that was involved. Yet, at the

end, the people we feel most thankful to are those who contributed most in increasing our work—the authors. For us, the most rewarding aspect of the assignment was the quantity and quality of papers we received: 103 papers covering a wide range of issues from authors around the world. This response, though somewhat overwhelming, was perhaps the best evidence that research on global strategy is reaching the state of maturity necessary to attract large numbers of established scholars.

If the number of papers at the starting point reflected a widespread interest in this area of research, the embarrassment of riches at the end of the refereeing process bore testimony to the quality of research on global strategy. We could include in this volume only a few of the articles that the referees found acceptable, and were forced to pass on others to Dan Schendel for consideration for future issues of *SMJ*.

This enthusiastic response to our call for papers quickly strained the refereeing process. While we had assembled a relatively large group of highly distinguished scholars to serve as our referees (see list at the end of the volume), we had to request a number of them to review two or even three papers. We deeply appreciate the amount of work the referees invested in this process, and the speed and grace with which they responded to our emergency requests.

We also acknowledge the support we received from both the Harvard Business School and INSEAD, not only in terms of administrative help but also in terms of the encouragement and help provided by our colleagues and the Deans. Julie Weigley and Gerda Rossell, our respective secretaries, bore the brunt of managing the logistics of the review process on top of their normally impossible jobs. Given the need for transatlantic coordination at each step, on-schedule publication of this volume would have been difficult without their firm hands at the helm.

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