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Source: *Strategic Management Journal*, Winter, 1992, Vol. 13, Special Issue: Fundamental Themes in Strategy Process Research (Winter, 1992), pp. 5-16

Published by: Wiley

Stable URL: <https://www.jstor.org/stable/2486363>

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THE CHARACTER AND SIGNIFICANCE OF STRATEGY PROCESS RESEARCH

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This essay discusses the character and significance of strategy process research. Process research in strategic management is paradigmatically diverse and empirically complex. Strategy process research has been narrow in its focus and its undoubted contribution has sometimes been obscured by the lack of explicit discourse about its analytical foundations. The essay draws on a wide range of social science ideas to lay out a set of internally consistent insights and assumptions to guide thinking and empirical inquiry about the analysis of process issues in strategic management. The essay also provides a guide to the eight papers contained in this special issue.

INTRODUCTION

In 1979 Schendel and Hofer provided a definition of strategic management which was processual in character. 'Strategic management is a process that deals with the entrepreneurial work of the organization, with organizational renewal and growth, and more particularly, with developing and utilizing the strategy which is to guide the organization's operations.' (1979: 11) Implicit within this definition lie some of the core precepts of a process approach to management. Thus the language of developing and using strategy assumes the treatment of strategy as a process not a state, and the emphasis on renewal and growth and of entrepreneurial work, all suggest a concern for action and movement in the analysis of the firm. Regrettably the promise in the Schendel and Hofer (1979) definition has not been realized. Much of strategic management writing, like a good deal of the social sciences, is an exercise in comparative statics. Cross sectional research

designs are combined with the static metaphors of contingency thinking to analyze the fit between the positioning and resource base of the firm and its performance in differing environments.

More recently a number of scholars from differing intellectual traditions in strategic management have made calls to bring time and dynamics more centrally into the language, thinking and methods of strategic analysis. Rumelt, Schendel, and Teece (1991: 26) ask for more dynamic explanation and commend recent interest in time-based analysis of competition and the administration processes that select and coordinate the firms attitudes.' Porter (1991: 115–116) meanwhile, applauds contemporary research exploring the chains of causality that constitute a more dynamic theory of strategy. What balance between environmental determination and company/leader choice shape the process and outcomes of competition? How are specialized skills created and used in the process of competition and what causes inertia in the over commitment of firms to certain competitive positions? Strategic management questions posed in the language of becoming rather than of being demand detailed, comparative and longitudinal

Key words: Strategy process research, analytical character

0143–2095/92/100005–12\$11.00

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data covering long periods of time. This may mean, as Pettigrew (1987, 1990) and Porter (1991) argue, that strategy research has to tilt towards the orientation, craft skills and methods of the historian.

But aside from a generalized concern with action, dynamism, time, development and outcomes, what are we to include within the intellectual domain of strategy process research? Chakravarthy and Doz (1992) commit themselves to a limited definition of strategy process, 'with how effective strategies are shaped within the firm and then validated and implemented efficiently.' The view that process research and strategy is essentially concerned with choice processes (strategic decision making) and implementation processes (strategic change) is, of course, understandable. The great bulk of the contributions of process scholars to strategic management have been in the study of choice and change processes. Thus the decision process scholar (March and Simon, 1958; Bower, 1970; Pettigrew, 1973; March and Olsen, 1976; Mintzberg, 1978; and Quinn, 1980, amongst others) used prevailing rational analytical schemes of intentional choice and outcome as a target in order to project more realistic views of choice behavior which recognized the importance of bounded rationality, organizational context, politics and power and chance. Meanwhile the change process scholars, (Mintzberg, 1978; Miller and Friesen, 1980; Pettigrew, 1985a; Johnson, 1987; Pettigrew and Whipp, 1991, amongst others) were puzzling over why there were often gaps between the formulation and implementation of strategies and why and how distinct periods of change and continuity could be discerned in the overall pattern of strategic development of the firm. Conceptual transfer began to occur between the choice and change theorists as first decision making processes, and then change processes, were interpreted in incremental, political and cultural terms.

Three principles guide the intellectual framing of strategy process research in this special issue. The first is the need to extend the themes pursued by process scholars beyond the still important but limiting areas of decision making and change. Secondly, to abandon the intellectual trap now clearly evident in classifying strategy research into content and process domains. And thirdly, to offer the reader a series of review

and research agenda papers which critically assess and synthesize existing research in eight themes demonstrably important to the development of the field of strategic management.

Strategy process research and the developing forms of processual and contextual analyses which underpin it will develop more widely and confidently if process questions and research designs can be extended beyond the exploration of choice and change processes. In this special issue, alongside the important review papers on strategic decision making by Kathy Eisenhardt and Mark Zbaracki, and cycles of organizational change by Henry Mintzberg and Frances Westley, we include a range of additional themes not always recognized, and certainly not always analyzed, in dynamic terms. Thus Arthur Francis reviews recent research on the Process of National Industrial Regeneration and Competitiveness; Ewan Ferlie introduces the quite new topic for strategic management scholars of The Creation and Evolution of Quasi Markets in the Public Sector; and Leif Melin examines the scattered and underdeveloped literature on Internationalization as a Strategy Process. Hiroyuki Itami and Tsuyoshi Numagami pursue the theme of the Dynamic Interaction between Strategy and Technology and in a related paper Rita McGrath, Ian MacMillan and Michael Tushman present new ideas on the role of executive teams in shaping the firm's technological trajectory. Finally, Andrew Pettigrew's paper synthesizes and critically reviews three intellectual traditions in the study of managerial elites and argues for the development of a complementary research tradition which combines a contextual and processual analysis of managerial elites.

It has been customary in the field of strategic management to distinguish strategy process research from the better established and more voluminous subfield of strategy content research. For a time this distinction between content and process has been useful. It has allowed a process tradition to develop in the field, with as Chakravarthy and Doz (1992) suggest, a different focus, disciplinary bases and methodologies. However, it is now clear that as elements of the content tradition are asking more dynamic questions (for example, Nelson 1991, Porter 1991) and parts of the process tradition are more explicitly linking process themes to the time series analysis of strategy content (for example,

Pettigrew and Whipp, 1991; Itami and Numagami, this issue), so the sharp distinction between process and content appears more of an analytical hindrance than a help. Certainly in the analysis of strategic change there seem to be strong advantages not only from linking process to content but also from exploring simultaneously the links between the contexts, content and process of change together with their interconnections through time. A further development of the context, content, and process analysis is to examine how variations in context and process explain different performance outcomes (Pettigrew and Whipp, 1991; Pettigrew, Ferlie, and McKee, 1992). These and other analytical refinements about the character of process analysis are discussed in the next section of this introduction. For the moment it should be clear that in the conduct of strategy process research, the what and the how, the content and the process, are best regarded as inseparable.

This paper has two further sections. The first section draws on theoretical and methodological contributions from a range of social science disciplines to discuss the ontological and analytical character of process analysis. The purpose of this section is to further elaborate the character and significance of strategy process research by laying out some of the assumptions which underpin it which are derivable from time series research underway in Sociology, History, Evolutionary Economics and Organization Theory.

The final section of the paper introduces the eight review and research agenda papers in this special issue.

THE CHARACTER AND SIGNIFICANCE OF STRATEGY PROCESS RESEARCH

In a recent paper, Van de Ven (1992) offers a helpful account of some of the unspoken assumptions and choices in studying strategy process. He is clear that strategy process research is diverse and cannot be contained within any single paradigm. He asks us to be clear about the meaning of process in our research, to be explicit about the theory of process we draw on, and to design process research in a way that is consistent with our definition and theory of process.

Van de Ven (1992: 169) argues that process is often used in three ways in the literature: (1) as a logic used to explain a causal relationship in a variance theory, (2) as a category of concepts that refer to actions of individuals or organizations, and (3) as a sequence of events that describes how things change over time. Of these three approaches, only the third explicitly and directly observes the process in action and thereby is able to describe and account for how some entity or issue develops and changes over time. It is this third view of process, which takes a historical developmental perspective and which thereby focuses on the sequences of incidents, activities, and actions unfolding over time, which is closest to the view of process explored in this essay.

In building on Van de Ven's (1992) third view of process, I add a number of crucial requirements for process analysis which are especially apposite for the complex subject matter under examination in the field of strategic management. Thus one needs to be much more aware than Van de Ven indicates of the enabling and constraining influences of various features of the inner and outer contexts of the firm upon the content and process of strategy development. Much clearer that there may be different processes underway at different levels of analysis (firm, sector and economy) and that a crucial source of change at any focal level of analysis may be the different rates and trajectories of development of the processes at these different levels. Clear also that context and action are always interwoven whenever human beings make their own history. Prepared also to construct research designs which can test how and why variations in context and process shape outcomes such as the pace of change and the performance of the firm.

As Bhaskar (1978) and Cohen (1987) argue, there is virtue in guiding any substantive field of empirical inquiry with a set of ontological assumptions or internally consistent insights which can inform the inquiry in a theoretically nonrestrictive manner. This is especially important in an area such as strategy process which is paradigmatically diverse and empirically complex. The adequacy of any set of guiding assumptions presumably will be dependent on their ability to support a wide range of theoretical and empirical investigations and at the same time maintain a sense of coherence in the overall

approach. In a range of publications (Pettigrew, 1985a,b, 1990), I have attempted to clarify a set of assumptions to inform and guide one of the themes important in the strategy process domain, the study of strategic change. The brief attempt here to generalize those assumptions across the strategy process domain has been greatly assisted by scholarly writing across a variety of disciplines in the social sciences. Discussions of process analysis in Sociology by Giddens (1979), Sewell (1992), and Sztompka (1991); in Sociology and History by Abbott (1992) and Tilly (1984); and in evolutionary theories of economic and technological change by Saviotti and Metcalfe (1991) and Hodgson (1991) have all been illuminating.

In the very title of his book, *Society in Action: The Theory of Social Becoming*, Piotr Sztompka captures two of the key words in process analysis. Exploring the how of social life begins with a theory of action or agency but with due recognition of the structural framework within which human conduct takes place. In delving further into processes of emergence, development and decay, into processes of social becoming, so time and history become crucial. Sztompka (1991) captures his view of the ontological assumptions of social reality by outlining a range of agential and historical postulates. The most important of these for scholars of strategy process are that:

1. Social reality is not a steady state but, rather, a dynamic process: it occurs rather than exists.
2. The social process is constructed, created by human agents—individual or collective—through their actions.
3. Social life is a process of structural emergence via actions, and the tension between actions and structures is the ultimate moving force of the process.
4. Action occurs in the context of encountered structures, which it shapes in turn, resulting in the dual quality of structures (as both shaping and shaped) and the dual quality of actors (as both producers and products).
5. The interchange of action and structure occurs in time and is cumulative, such that the legacy of the past is always shaping the emerging future. What happens, how it happens, why it happens, what results it brings about is dependent on when it happens, the location in the processual sequence, the place in the

rhythm of events characteristic for a given process.

Useful as these postulates are for defining some of the guiding assumptions of a process analysis, they require greater elaboration than there is space in this essay. Of course, the postulates themselves and the links between them contain many analytical riddles. Sztompka (1991: 42) mentions one; the riddle of history, how links in the cumulative sequence of the process engage causally with the different levels of analysis in the context. Event sequences are important to clarify in any process analysis but so are the underlying mechanisms which shape those events.

Equally well, there is no dispensing with assumptions of model of person in process research. March (1991) argues that human beings do not reliably follow procedures of rationality, but are invariably witnessed to be following logics of imitation, experience, appropriateness or faith. Giddens (1979), Cohen (1987) and Sewell (1992) all remind us that although there is capacity for agency in all human beings, differences in power, knowledge, and other resources provide different opportunities for the realization of influence in social processes.

The study of sequences of events are crucial in any process analysis. However, the purpose of the process analysis is not just to describe the sequence or tell the story, but to identify patterns in the process often across several carefully chosen cases. Any assumptions of path dependency, that what has happened at an earlier point in time will affect later sequences and outcomes, does not necessarily assume either fixed or predetermined phases or stages of inevitable outcomes. Shifting contexts themselves mean that social processes are inherently discontinuous, open ended and full of surprises. There is, therefore, the additional riddle of recognizing patterns amongst diversity and unpredictability.

Abbott (1992) brings in an added complexity. The analysis of any single process occurs not just in a nested context but also alongside other processes. There may be a requirement to understand a network of intertwined processes; what he calls the interactionist field as a whole.

Such an analysis moves the researcher away from theories of individual causes towards theorizing about a constellation of forces shaping the

character of the process and perhaps explaining differences in outcome. For most process scholars, that kind of holistic theorizing is barely an ambition and rarely an accomplishment.

In their recent review of developments and trends in evolutionary economics, Saviotti and Metcalf (1991) acknowledge the absence of a coherent analytical structure and well codified set of conceptual tools. Evolutionary economics is 'still far from being a complete theoretical system' (1991: 2). Strategy process research has a similar problem, indeed perhaps a deeper one. Empirical research of varying quality exists, but there is no tradition of ontological and theoretical discourse which seeks to unravel, debate and clarify the foundation assumptions of process in strategy process. If we are to make the study of processes one of the fundamental building blocks of strategic management, we need some clearer analytical guidance to steer empirical research. Abbott (1991) makes his appeal for a return to processual thinking and analysis in Sociology and History by his attack on the 'exhaustion of the variable paradigm.' He contends that the variables paradigm in Sociology has led to 'real actors being replaced by variables, narrative causality by the reified causality of variables, real time by the order of the variables' (Abbott, 1991: 203). Recently Daft and Buenger (1990) and Bettis (1991) have exercised similar strident criticism upon 'the normal science straight jacket' enveloping strategic management. They may or may not be correct, but criticism, however relevant, is rarely sufficient to encourage the emergence of newer traditions of thinking and research.

If strategy process research is to flourish, and there are many scholars now who wish that so, then we need to encourage more explicit thinking and writing about the analytical and conceptual assumptions which underpin processual research. This is not an appeal for doctrinal orthodoxy, but a request for a language to guide discussions about strategy process and indeed, at the most practical level, for more knowledge about the process of studying process.

One way forward is to present some of the core assumptions about process analysis derived from other social science disciplines. A second way to encourage progress is to reflect on and codify our own experiences. Van de Ven's (1992) recent suggestions for studying strategy process research are undoubtedly a product of his

experience on the Minnesota Innovation Research Program. In what follows, I offer a highly synoptic picture of learning points from our experience of strategy process research at the Centre for Corporate Strategy and Change, University of Warwick.

This discussion of the character of strategy process research is organized around five internally consistent guiding assumptions. We have found these assumptions are capable of supporting a wide range of the theoretical and empirical investigations and at the same time maintaining a sense of coherence in the overall approach. They are:

1. embeddedness, studying processes across a number of levels of analysis;
2. temporal interconnectedness, studying processes in past, present and future time;
3. a role in explanation for context and action;
4. a search for holistic rather than linear explanations of process; and,
5. a need to link process analysis to the location and explanation of outcomes.

What precepts about process and strategic management guide our work? The first is that strategic management is a multilevel series of activities and processes. Therefore the exploration of strategic choices and changes in the firm has to be embedded in an analysis of the inner and outer context of the organization. Outer context includes the economic, social, political, competitive and sectoral environments in which the firm is located. Inner context refers to the inner mosaic of the firm; the structural, cultural and political environments which, in consort with the outer context, shape features of the content and process of strategic development. Processes are embedded in contexts and can only be studied as such. Thus explanations of the changing relative performance of firms should be linked to higher levels of analysis (sector changes and alterations in national and international political and economic context), and lower levels of analysis (the drivers and inhibitors of change characteristic of different firms' culture, history, and political structures). There is also the recognition that there are processes at different levels of analysis, (firm level internationalization as well as sector level internationalization), and also multiple processes at the same level of

analysis (firm level strategy and technology development). A source of change is the asymmetries between levels of context, where these intertwined processes often have their own momentum, pace and trajectory. Thus the rate and trajectory of change in an industrial sector facing significant boundary changes may be much faster than the sensing and adjustment pathways of individual firms to the regrouping of the sector. The relative slowness of the sensing and adjustment process of firms, and their failure to recognize that the bases of competition may have changed in that sector, is a key factor explaining their loss of competitive performance (Pettigrew and Whipp, 1991).

The second guiding assumption of the Warwick process research is the need to reveal temporal interconnectedness. There is a search to catch reality in flight. Understanding the sequence and flow of events over time is a crucial requirement for the process scholar. There is the looming presence of the heavy hand of the past. History is crucial. Antecedent conditions shape the present and the emerging future. But history is not just events and chronology, it is carried forward in the human consciousness. The past is alive in the present and may shape the emerging future. Beneath the surface events and chronology, the process scholar searches for recurrent patterns in the process, for structures and underlying logics. But there is no assumption of predetermined timetables, of ordered and inevitable sequences or stages. Trajectories of strategy processes are probabilistic and uncertain because of changing contexts and human action. As Loasby (1976: 5) has so eloquently put it, 'if choice is real, the future cannot be certain; if the future is certain, there can be no choice.'

Loasby's telling phrase reminds us that context and action are also inseparably intertwined. We cannot talk about process without also discussing human agency in context. Here the key starting point is that it is not a question of nature or nurture, or context or action, but context *and* action. Context is not just a stimulus environment but a nested arrangement of structures and processes where the subjective interpretations of actors perceiving, learning, and remembering help shape process. Thus strategy processes are both constrained by features of context such as tradition and technological commitments and also shape contexts by, for example, preserving

or altering technological strategies or corporate cultures. We recognize Giddens' (1979) theoretical contribution of superseding the dualism of structure and agency by creating a duality between the two. This duality provides two crucial analytical building blocks for the process scholar. First of all, structure and context are conceptualized not as barriers to action but as essentially involved in its production. And second, there is scope to demonstrate empirically how aspects of context (such as a deteriorating industrial and economic scene) can be mobilized by key actors as they seek strategic outcomes important to them (Pettigrew, 1985a; Pettigrew *et al.*, 1992).

Our fourth guiding assumption in the conduct of strategy process research is a natural concomitant of the first three. Links between multiple levels of context can only be established by exposing actions and recurrent patterns in the processes under investigation over years and sometimes decades. Time is captured in our work through a combination of retrospective and real time analysis. The longitudinal comparative case study method is our primary approach. A natural advantage case studies have over other comparative methods is the opportunity to explore holistic explanations within and between cases. There is scope to examine causal processes directly, to look at them in context. The world is not lost in abstract, timeless scores on variables, but as Tilly (1984: 14) puts it, the focus is on 'real times, people, and places as referents.'

In holistic explanation causation is neither linear or singular. The search is not for single grand theories. As Tilly (1984) indicates, the pursuit is for proximate, not final causes. Or as Ragin (1987) contends, the search is for multiple intersecting conditions which link features of context and process to certain outcomes. Thus in our competitiveness research, five key features distinguished our high performing automobile, investment banking, insurance and book publishing firms from their lesser performing comparators. But the real issue explaining relative competitive performance was not just the isolation of these five features of environmental assessment, human resources as assets and liabilities, managing strategic and operational change, leading change and coherence; but by the convergent interaction and interconnected loops among the five features, in the firm over time.

Our fifth guiding assumption for conducting strategy process research is the ambition to link the analysis of process to outcomes. There are great complexities and many unresolved problems in attempting the form of highly contextual and longitudinal research outlined here. These complexities naturally encourage the wary practitioner to look for ways of reducing the complexity. I concur with Tilly (1984) and Ragin's (1987) suggestion to stick with the careful comparison of a small number of cases (often in our example, 6 to 10). Other routes to simplification are rehearsed in Pettigrew (1990). The fifth guiding assumption seeks complexity reduction through research design. There are great advantages in having a clear outcome to explain in strategy process research. Thus in our competitiveness research the outcome variable was the differential performance of the eight firms in the study (Pettigrew and Whipp, 1991). Whilst in our research on public sector strategic change processes, the outcome variable was the differential rate of change amongst sets of health authorities facing similar content areas of change (Pettigrew *et al.*, 1992).

Building an outcome into a process research design has a number of advantages. Firstly, the outcome both simplifies and complicates the study by providing a focal point, an anchor for the whole investigation. Secondly and crucially, there is the possibility to explore how and why variations in context and process shape variability in the observed outcomes across the comparative investigation. How policy outcomes are shaped by the policy process and the policy context is after all one of the central distinguishing questions that can be posed in strategy process research. In many social science fields, and certainly in equilibrium models in Economics, the interest is only in the result of the process, not the process itself. Strategy process research is capable of generating sound knowledge not only of processes and outcomes but also of why and how outcomes are differentially shaped by processes.

In summary, what can be said about the significance of strategy process research? Its distinctiveness and additiveness derive from the forms of process analysis outlined above and illustrated by the wide range of themes examined in this special issue. Its central questions are about the description, analysis and explanation of recurrent patterns in the process of strategy

management, together with the exploration of why, when and how policy outcomes are shaped by features of policy processes and contexts. The special focus is on action and context. The critical role of time and history in understanding the emergence, development, decay and regeneration of key people, teams, institutions, sectors and political and economic systems. The search is to catch reality in flight and a return to embeddedness as a principle of method. Exploring how and why links in the cumulative sequence of a process engage causally with different levels of analysis, to explain when and why certain outcomes are realized. These broad questions can be profitably linked to the dynamic analysis of processes of decision making, change, competitiveness, market creation, internationalization, business strategy and technology, and the role, conduct and performance of managerial elites in firms and societies.

A GUIDE TO THE PAPERS

The themes, papers and authors for this special issue have been invited by the guest editor. The authors bring wide experience and expertise to each theme. They also carry the perspectives of the differing disciplinary and national academic traditions from which they come. The authors originate from the academic fields of Sociology, Public and Social Policy, Organization Theory as well as from Business Strategy. In their papers they draw on an even wider academic canvas to review and critically synthesize a wide range of research and writing on strategy process. As a European guest editor, I am particularly mindful of the wide variety of national academic perspectives and traditions which exist outside the United States of America and Canada. I am delighted we have been able to include some of these national traditions in this special issue. Thus Arthur Francis draws on recently published work from a national program of research on the competitiveness of U.K. industry funded by the Economic and Social Research Council. Ewan Ferlie introduces to the strategic management field a topic now critical within many European public sector systems, the creation of quasi markets, and Leif Melin brings a Swedish perspective to the internationalization processes of the firm. We are also delighted that Hiroyuki

Itami and Tsuyoshi Numagami are able to contribute a paper on the important theme of the dynamic interaction between strategy and technology. This paper draws heavily and appropriately on North American published research, but also links that tradition of work to Japanese work not previously made available in English, and utilizes with effect, many Japanese cases and examples.

The invited papers satisfy the high standards of the authors. Many have benefited from the advice of colleagues. All have been open to feedback provided over a number of drafts by the guest editor. My own paper on the study of managerial elites was tested on academic audiences at the annual conferences of the British Academy of Management and Strategic Management Society in September and October, 1992. The paper has also been enriched by the thoughtful comments of Michael Useem, Richard Whittington and Edward Zajac. As ever, the versions published here remain the responsibilities of the authors.

In addition to disciplinary and national academic diversity, three other principles have guided the shaping of this special issue. I have sought papers which extend the themes of strategy process research beyond the customary areas of strategic choice and change. I have encouraged topics and authors who, to some degree, are interested in linking strategy process to content and context. And I have asked all the authors, not only to review and critically reflect on existing research and writing on their theme (the classic requirement for a review paper), but also to conclude their paper in a constructive manner by signaling elements of a future research agenda. I hope in this way all of the authors will stimulate a rapid take-up of their ideas as colleagues find some ready stepping stones to take strategy process research forward.

Many scholars are now sensing that the field of strategic management is at another transition point in its development. There is talk of more longitudinal research, organizational histories and a greater pluralism (Bowman, 1990); encouragement for methodological diversity, more problem driven as distinct from theory driven research (Bettis, 1991), and a need to break out of the 'normal science straight jacket' (Daft and Buenger, 1990). There is also real evidence of further disciplinary interest in strategic manage-

ment as a field of research, particularly from Economics but also from Sociology, Social Policy and Organization Theory in this special issue. If this disciplinary interest and involvement can both consolidate and create new themes and questions for strategic management and at the same time through conceptual transfer deepen the analytical and ontological base of the field that will be a positive step. Certainly, as I argue earlier in this introductory essay, such deepening is no longer a nice to have, but now an essential condition for the further development of strategy process research.

The papers

The focus of Kathy Eisenhardt and Mark Zbaracki's paper is a review of research on strategic decision making. This is one of the core areas of interest for strategy process scholars and center stage in the process of strategic management. It is in strategic decisions that the most fundamental choices are made which can shape the direction of the firm. Eisenhardt and Zbaracki characterize the field of strategic decision making as a set of multiple and mature paradigms with incomplete assumptions. They critically review research underpinned by rationality, politics and power and chance. They conclude that to varying degrees empirical evidence supports notions that people are rational, but only boundedly so, that power wins battles of choice, and that chance affects the course of strategic decision making. However, all these paradigms rest on unrealistic assumptions about how people think and act, and no longer controversial debates about goals and rationality. They note that the time frame of investigations testing each of the paradigms influences the appropriateness of the frameworks. Thus a longer time perspective improves the efficacy of the garbage can model and a short time perspective, the rational and political models. As time progresses, the scope of decisions may increase, the range of participants may become more varied, and the number of solutions larger.

Eisenhardt and Zbaracki conclude with a research agenda which is part incremental of existing work and part rather bolder. The incremental element suggests further research on rationality as a multidimensional rather than as a monolithic construct; more research addressing

whether politics is effective or dysfunctional for strategic decision making, and further tests on whether the garbage can paradigm is relevant at all. The bolder agenda is underpinned by a request for a more complex human being at the center of decision making; by more normative and less descriptive work which seeks to explore the relationship between decision processes and outcomes; and by more contextually-based research drawing on comparative sector and cross-cultural designs.

Henry Mintzberg and Frances Westley organize their review and thoughts on strategic change processes by pursuing the theme of cycles of organizational change. They articulate three approaches to organizational change, procedural planning, visionary leadership, and inductive learning. Each of these approaches implies alternative process characteristics. Thus the procedure mode is deliberate and deductive, the visionary suggests deliberate leader action, and the inductive learning is informal and emergent. They suggest there may be sequences of change approach over time, with one pattern involving a shift from learning, through visioning, to programing. Mintzberg and Westley's paper contains a rich proliferation of metaphors whose very imprecision catches the reader's attention and imagination. Organizations are seen as being always under change, with episodes and stages over time appearing as patterns of evolution containing bumps, oscillations, spirals and cycles. Their framework is elaborated in conclusion by developing three models of change experienced by major world religions, labeled enclaving, cloning and uprooting. Their conclusions for future research on strategic change processes are a plea for studies of change to be embedded in a rich contextual analysis, to be examined holistically, and for pattern recognition in change studies to be reinforced by longitudinal evidence of processes over long periods of time.

Arthur Francis deals with that most crucial area for strategic management, national and firm competitiveness. He notes that research in this area has largely developed in two independent streams, with economists pursuing issues of national competitiveness, and management scholars more interested in firm level competitive performance. The basic premise of the Francis paper is that a convergence between these two streams is now desirable and possible. His starting

point is that part of the explanation for a national economic performance results from the behavior of individual firms. As such, there are identifiable processes within firms that generate competitiveness which can be researched and which are capable of being linked back to the specific institutional arrangements within firms and the wider generic institutions characteristic of different nations. Francis argues that the problems of linkage between levels of analysis and explanation are at least partially resolvable by the recent theoretical and empirical work of the new institutionalists in Sociology and Organization Theory.

Drawing on recently published U.K. research on firm level competitiveness, Francis assesses evidence that regeneration is possible to reestablish competitive performance; that choices about the content of strategy are important but equally well so are the need to build competences and institutional arrangements in and outside the firm to implement strategies; and that social and political expertise to manage processes is critical to the fate of firms. Francis also points to the important role of learning processes in firms, especially about how to simultaneously cooperate and compete.

Francis contends that the bridge between the studies of national and firm level competitiveness will also span another divide, that between the study of processes and outcomes. His paper is also encouraging of strategy process research across several levels of analysis. He concludes by proposing a research agenda which focuses on processes within the firm itself, but which analyzes these at least partly in terms of the national context. Future research should identify with greater precision the institutional characteristics at firm, regional and national levels that are relevant to the firm level processes of generating competitiveness.

Ewan Ferlie's paper is at one level an appeal for strategic management scholars to begin to take public sector organizations and service provision as seriously as they have the competitive strategy of business firms. At a second level it is a call to bring a new process theme, the creation of quasi markets in the public services, to the attention of all social scientists.

Referring mainly but not exclusively to a U.K. context, Ferlie sees no dramatic withering away of the public sector in the 1980s and 1990s.

Health, education, social security, defense and criminal justice will remain as core goods and services distributed through public sector organizations. However, there are great pressures for change in the way those services will be distributed and accounted for. Delaying is a theme in the public sector. There are changed roles for producers (the professionals who traditionally have decided on the what, why, when and how of service care). There is also a new interest in the customer and the market and with that the emergence of hybrid forms of organization. Thus in a quasi market, the State becomes a fundor, purchasing services from a variety of private, public and voluntary providers, who by definition and practice are no longer monopoly suppliers of services, but compete for contracts and reputation in a quasi market.

Quasi markets have traditionally been approached analytically through the twin lenses of neoclassical and transaction cost economics. Ferlie critically assesses these twin lenses alongside published work on a new economic sociology. He concludes that institutional change and market creation are profoundly political and economic processes. He also sees an important role for sociological concepts of social relations, networks, power, culture, trust, reputation and obligation in the understanding of how markets really work.

In his research agenda, Ferlie argues for the development of theoretically informed but middle range propositions to test the rather abstract and separated theoretical perspectives currently used to discuss quasi markets. He concludes with five propositions which can be used in empirical inquiry to test the relative validity of those theoretical frameworks.

The processes through which firms internationalize their operations is a central research and policy issue in strategic management. Swedish scholars have made theoretical and empirical contributions to the study of international business and it is appropriate that Leif Melin, a scholar well familiar with European research, should lead us critically through elements of that literature. Melin assesses stage models of the internationalization process, research on the strategy structure link in multinationals, and some of the most recent writing on the structures, systems, cultures and competences deemed necessary to manage multinational firms. Melin criticizes the field of international business for

its largely cross sectional research and static models, and concludes the paper with a statement of new research themes to study internationalization more explicitly as a strategy process.

Melin argues we need more research on the role and significance of cross-border acquisition processes in the overall internationalization process. This research should examine the stated and unstated motives for such acquisitions, when, how and why they occur in the evolution of firm international strategies, and their consequences for issues of cross cultural integration. More research is also necessary to underpin the consultancy-based writing on the international firm. This is particularly necessary to identify patterns in the transformation of structures and other coordination mechanisms in multinationals, and to examine the role of intra and interfirm networks in facilitating the international strategic development of businesses.

The two papers by Hiroyuki Itami and Tsuyoshi Numagami and Rita McGrath, Ian MacMillan and Michael Tushman deal in a complementary fashion with the relationship between business strategy and the market and technological development of the firm.

Itami and Numagami's paper probes the interactive links between technology and strategy over time. They demonstrate how and why strategy capitalizes on current technology; how current strategy can cultivate certain technological developments; and how in turn technology accumulation can alter the cognitive and idea system of the organization in a way which has net effects on future business strategy. Itami and Numagami summarize these three links or effects as respectively: strategy capitalizes on technology; strategy cultivates technology and technology drives cognition of strategy. They note that past strategy research has been dominated by the exploration of the first of these links and effects. They see big intellectual pay-offs for strategy process research through the pursuit of the third link, how technology drives cognition of strategy. Here processes of technology accumulation and strategy development can be profitably explored as simultaneous, emergent, iterative and featured by strong elements of learning from doing. In such processes, organizational capability for learning becomes a critical intangible asset. As a counterpoint to the McGrath, MacMillan and Tushman paper, Itami and Numagami portray

strategy emerging from evolutionary processes involving many people in the organization and not a special province of the executive suite. However, in their research agenda, Itami and Numagami argue we need to know more about how cycles of development and evolution in top management teams affect the dynamic interaction between strategy and technology. The paper by McGrath, MacMillan and Tushman seeks to link strategy development processes in the firm to understanding and action around current strategy, technological development and the opportunity space created by the 'lumpiness' of limited population markets (defined as unevenly concentrated groups of customers). The paper is action oriented, arguing and demonstrating that there are mechanisms through which executive teams can shape technology strategy by means of the scope, direction and quality of technology strategy decisions. For the strategist a key question is, how do I create a dominant design in the industry for my firm? This question also needs to be answered in relation to the market space afforded certain technological developments and product classes at certain times. The paper assumes the top management team can calculate and target such market knolls. The team can also shape technology development by the way in which they intervene in three subprocesses:

1. the generation of variations of design;
2. shaping of the odds of selection of that design in the market; and,
3. delaying the diffusion of a selected design to competitors.

The research agenda in the McGrath, MacMillan and Tushman paper flows from their attempt to create linkages between design attributes, market demand characteristics and technological capabilities of competing firms. They ask can the concepts of market knoll be incorporated with benefit into empirical research on strategic management? How do idiosyncratic or intangible assets evolve, and how is the success of such evolution related to the structure of the market spaces occupied by competing firms? Which technology strategies yield dominant designs, and under what circumstances? Are technological regimes consciously managed, and how? And what role do executive teams play in processes which link strategy, technology, markets and organization?

The study of managerial elites is one of the most important, yet neglected areas of social science research. The final paper in this special issue by Andrew Pettigrew synthesizes and critically reviews three intellectual traditions in the study of managerial elites. These are: interlocking directorates and the study of institutional and social power, the study of boards and directors, and the composition and correlates of top management teams.

Pettigrew's critical review notes the strengths and weaknesses of all three traditions of work. The distinctive methodological approach in the interlocks and top management team research has given those traditions greater intellectual coherence and impact than work on boards and directors. The more scattered, limited and prescriptive character of the boards work, has produced a less easily identifiable set of theoretical and empirical achievements. By and large all three areas of research have developed in isolation from one another, yet share the common limitation of studying managerial elites several paces from the actors, processes, and issues facing elites. Rarely are interlocking networks studied in action. Little is known at first hand why directors form ties across the boundaries of their own organization, to what purposes such ties are put, and what issues are created or resolved through such behavior. Power relationships are attributed. Control relationships within and between social classes are inferred. The mobilization and use of power to achieve outcomes in line with perceived interests remain unobserved.

In the top management team research, easily measurable demographic characteristics are used as surrogates for unobserved intervening processes and inferential leaps are then made to a range of organizational outcomes. The existence of a top management team is assumed. No one sits close enough to the phenomenon to identify whether and to what extent the top team exists and through what processes the team fashions its impact.

Pettigrew concludes with a research agenda whose purpose is to tilt research on managerial elites towards processual studies of interlocking networks, boards and top management teams. The aim of creating a tradition of process research on managerial elites is to complement, not replace the three traditions reviewed in the

paper. The paper ends by specifying a range of research questions to inform process studies of managerial elites.

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