

# STRATEGY PROCESS: INTRODUCTION TO THE VOLUME

## THE CHALLENGE OF STRATEGY PROCESS RESEARCH

Enduring scholarly interest in the process of strategy-making stems from an abiding assumption that some ways of strategizing are more efficacious than others, and thus lead to higher firm performance in the long run; higher than luck alone would bring. Expressions of interest in and endorsements of the strategy process are abundant in the academic literature. As Pettigrew (1992) points out, Hofer and Schendel's pioneering definition of strategic management is processual in character emphasizing the development and utilization of strategy. Rumelt, Schendel, and Teece (1994) list the policy process question – how does policy process matter? – as a fundamental question of the strategic management field. Porter (1996) expresses preoccupation with the leadership and organizational challenges of managing the process. And, Hamel (1988) exhorts the field to devote as much attention to the conduct of strategy, i.e., the task of strategy making, as they have to its content. For senior managers and leaders, the question of how to make effective strategies stands usually at the top of their agenda. Not surprisingly then, the quest to uncover stable principles of good strategy making has attracted much support and interest over the years.

Researchers who responded to the strategy process challenge had known many moments of exhilaration and disillusion, the exhilaration of discovery, and the disillusion from the often grotesque ratio of effort to outcome and the indifference of a scholarly community heavily reliant on a large sample variance approach. Indeed, empirical research in strategy process has often required large, often heroic and invariably idiosyncratic, longitudinal data collection efforts aimed at exploring the link between the strategy process and the quality of decisions, and ultimately the performance of firms. Such quests, potentially revealing and insightful but often risky from the researcher's career perspective, presented formidable, methodological, and practical hurdles. Moreover, the theory yielded by such ambitious quests,

while comprehensive, was often complex, messy, and notoriously fragile. The accumulation of scientific evidence in strategy process thus progressed slowly because of the relative paucity of studies and also because of their idiosyncratic nature. Scholarly insights took longer to accumulate, perhaps too long to serve as the sole basis for helping the eager practitioner in search of simpler but applicable advice.

## BENEATH THE SURFACE

As a result, a significant and often highly visible part of the field is characterized by a controversial normative orientation. Bold claims of two dominant flavors – strategic change or protecting and extending existing strategies (Chakravarthy, 2003) – are typically made with modest and often selective empirical evidence. Such prescriptions tend to be highly influential until they are outdated, out fashioned, or shown to be hazardous when poorly understood boundary conditions governing their use are exceeded.<sup>1</sup> Barabba, Pourdehnad, and Ackoff (2002, p. 5) put it eloquently: “a significant proportion of the advice produced by such management gurus is either incorrectly inferred from data (but nevertheless, may be true) or is unsubstantiated by genuine evidence.” Enduring effective strategy-making prescriptions are rare, or as some would claim impossible. Indeed, a prescription that would guarantee supra-normal profits consistently would de-facto become the strategic management field’s own version of the proverbial money machine.

As a result, the field as a whole seems to have ebbed in and out of favor with practitioners. This instability has stimulated poignant condemnations from several noted contributors to the field who sound their own versions of the naked emperor complaint. Mintzberg’s (1994) dissection of strategic planning denounces the “missing detail,” neglect of synthesis, which he deems crucial to understanding strategy making. Hamel publicly exposes the “dirty little secret” of the strategy field, the dearth of insight about where strategies come from (Jackson, 1997). Chakravarthy (2003, p. xii) points out that: “the process through which strategies are shaped, implemented, and changed remains ill understood.” This reality led some to deride the prevailing approaches to theorizing in business policy and strategy research as a confused and infantile art that was unlikely, by itself, to cumulate or pass the test of time (Camerer, 1985). Mere access to senior management seemed insufficient grounds for academics to muse about decision-making at the top. Once the excitement and energy that comes from observing strategy making in situ dissipates not much else was expected to remain.

But beneath this dramatic and unstable facade lies a gradual, patient, and seemingly more stable, hard-at-work, academic enterprise. Scholarly strategy process research apparently goes on, perhaps more than ever, suggesting that there is something fundamental and deeply interesting and profound about how strategies are made, where they originate in organizations, and how the process of strategy-making impacts the performance of organizations. Casual counts of journal articles and books focused on strategy making suggest that this is a rich and increasingly active area of investigation. Cognition, emotion, strategizing, strategy practices, strategy as organizing, organizing as strategy, complexity, architectures, patching, innovation, and the like are terms that are commonly associated with it in the contemporary literature. Indeed, the strategy process area seems to be a fertile meeting ground for many types of scholars interested in problems of strategy and organization.

Yet, the academic visibility of this part of the strategy process community has been partly hampered by its internal fragmentation. So the field is characterized by relatively homogenous, stable, and sometimes exclusive subgroups that protect zealously a set of fundamental beliefs about rationality, reductionism, agency, and other aspects of the process; a set of beliefs that are seemingly treated as non-discussable. These sometimes rigid divisions can cost visibility. Indeed the question of process was excluded from a landmark book discussing the fundamental questions of the strategic management field because of irreconcilable differences between subgroups of scholars (Rumelt et al., 1994, pp. 527–530). Academic visibility, so the belief goes, has suffered also because of the relative paucity of studies, and because scholarly journals are averse to this “kind” of research.

## PEARLS OF WISDOM

Yet, fragmentation notwithstanding, the contours of an enthusiastic strategy process community are increasingly apparent; a community that is less anxious to prescribe and more inclined to collect patiently the fruits of academic inquiry, more eager to subject itself to the test of time. There, in that community, wisdom and insights about the nature of strategy making have cumulated from patient observation of the strategy process and some of those insights are indeed beginning to meet the test of time, as a new generation of researchers is building upon them to design their own research.

A case in point is Bower’s (1970) resource allocation process model. Bower’s model remains in use after 35 years. The basic idea is that the

resource allocation process is a complex, multilevel phenomenon that fundamentally shapes a firm's strategy. Burgelman (1983) extended the model to distinguish between strategic activity that falls within the bounds of institutionalized corporate strategy and that which emerges from subunit activity in a way that makes the model congruent with an evolutionary perspective. Subsequent scholars strengthened the model by examining and applying it to other critical aspects of the strategic process.<sup>2</sup>

As Chakravarthy (2003) points out, the Bower and Burgelman's model fleshes out the important connection between the corporate context and the premise of decisions. They also review a number of classification processes, which include taxonomies for studying the strategy process. Like the Bower's model, some of those other taxonomies have served as points of departure for subsequent empirical work in strategy process. Such modeling and taxonomical efforts serve as the starting point for ongoing research. We will return selectively to some of them later in the preamble to our review of the papers in this volume.

## **RE-DISCOVERING STRATEGY PROCESS RESEARCH**

The three of us, despite our ongoing interest and involvement in the strategy process field, were taken by surprise by the scale and energy of this somewhat discrete strategy process research world. We re-discovered its full extent by accident in a workshop that the three of us organized at the 2002 Academy of Management conference in Denver, Colorado. Our first surprise was the size of the audience and its composition. We expected a modest audience composed by the usual suspects of the strategy process field. Sure enough, Joe Bower, Robert Burgelman, Paul Nutt, Andrew Pettigrew, and V.K. Narayanan acting as the SMS liaison were all there. But also, there were a new generation of talented junior faculty and Ph.D. students from a broad range of schools who presented ongoing work. And it was one of the best attended workshops that year!

We opened the workshop by asking senior researchers in the field to reflect on their experience. In response, Robert Burgelman asserted that process manifests itself in concrete substantive areas of organized activity over time. Hence it is necessary, he said, to study a real phenomenon. The study of process is the study of simultaneously occurring activities and of their linkages. On the link between process and content, he commented that process generates content and that content disciplines process. Yves Doz added that strategy process is particularly interesting in large firms where

multiple managers are interacting in complex decisions in real time. Those decisions are conflictual and cannot be routinized. He recommended that conceptual work should be done at the middle range. There is a need for relatively low level conceptual constructs that help make the data relevant. Effective researchers should put sufficient emphasis on descriptive insights and low to mid level conceptualization. Further he recommended focusing on surprises and anomalies. Paul Nutt reflected on some of the difficulties that he experienced when conducting strategy process research. In particular, he stressed the extraordinary efforts that were required to obtain the necessary detail, because it is important to collect the information in real time and when the process is actually happening. Study piety by studying a pious man in a religious moment, he said. He also commented on the challenge of staying above a sea of details and keeping it all in perspective. To this end he recommended juxtaposing process and content. Finally, we heard from Liisa Valikangas and Amy Muller, both consultants from Strategos, who pointed out that an important way in which companies come up with strategies is by buying them from consultants. So research should focus also on how consultants come up with strategies and how they sell them to companies. Andrew Pettigrew closed the workshop by saying that strategy process was much more accepted today than it was 10–15 years before. And that was the cause for celebration. He also called for critical reflection. He saw the need to ask whether strategy process research of any kind has actually made a difference. He saw the need to ask why one should bother to conduct strategy process research at all.

We became intrigued and wanted to dig deeper into what was really going on. What kinds of research and theoretical framings were people relying on? What were some of the new insights into strategy making that were emerging from the new generation of researchers in the field? Was the field becoming more “micro” by generating insights into the cognitive and emotional foundations of strategy making, or has interest shifted to broader organizational and institutional influences on strategy-making activities in and between organizations? And were there new methodologies that mitigated the difficulties of studying strategy making empirically?

## **THIS BOOK**

This volume is the culmination of our three-year effort to explore and uncover this relatively hidden or at least less visible side of the strategy process

field. The origins of this volume trace back to that 2002 workshop. That workshop attracted over 60 scholars from wide ranging theoretical and methodological perspectives who spent an entire afternoon discussing strategy process, presenting their work, discussing methodological and theoretical challenges, and assessing the prospects for the field. We were excited by the tremendous enthusiasm for process research among workshop participants and by the richness and diversity of the intellectual discussion, which drew on theoretical developments from fields as diverse as complexity theory, social movement research, and cognitive psychology.<sup>3</sup> Anchored in the gloom and doom of the public condemnations of the field, we emerged from that workshop convinced that we were fortunate to be witnessing the beginning of an energetic and widespread resurgence of interest in process-related topics, a resurgence which had transcended national boundaries and that was occurring all around the world.

The feedback that we received about the workshop reinforced that impression. Many of the participants expressed in their verbal and written comments to us a desire to keep the conversation going. So we decided to organize a follow-up conference the next summer at INSEAD in France. We chose a European venue to encourage scholars from around the world to send in submissions for consideration, and we were particularly keen to sample from the many very interesting research projects we knew were underway in Europe on strategy making in organizations. We invited participants based on refereed abstracts. Of the 71 abstracts submitted in response to an open call, we selected 35 and invited the authors to present their work in a two-day session in Fontainebleau in August 2003. Besides quality, our selection criteria also emphasized variety of theoretical and methodological perspectives at the conference. We clustered the papers in groups of 4 or 5 that shared the same level of analysis or that were otherwise similar. Each session had a discussant. Those specialized discussions were then followed by three lively plenary debates on strategy process.<sup>4</sup>

After the conference, we invited the authors of those 35 papers to revise them in light of the feedback that they had received at the conference and then re-submit them for consideration for this volume. To select the papers, we relied on a round robin half-blind review process where each paper was sent to two of the other conference participants. Reviewer comments were then used to winnow down the final set of papers to the 16 chapters that are included in this volume. We made final acceptance decisions after the authors had incorporated the feedback from the reviewers. We have asked the authors of some of these papers to revise them yet again for length, clarity, or both.

Taken together, the 16 chapters that we have selected for inclusion are, in our opinion, rather representative of the face of current scholarly strategy process research. We have emphasized to everyone involved that we would do our best to neutralize our own personal biases. We were keen on representing the field in the volume as close as possible to the way that “the community” actually saw it. As a consequence, we have tried many different classifications and ordering schemes for the chapters, once we knew what chapters were going to be included. We eventually settled on a relatively standard classification scheme that has proven most useful since the initial 2002 workshop, through the 2003 conference in France, and the round robin review process resulting in final chapter selections. We believe that it effectively conveys a sense of the similarities and differences between the chapters and the diversity of perspectives and research in this area.

Our classification scheme has four categories. The first has to do with the micro-foundations of the strategy-making process, the second with more macro-level processes associated with it, the third includes examples of empirical studies of strategy making, and the fourth captures commentary about appropriate techniques and methods for studying strategy making in action.

Accordingly, the chapters in the first section of this volume (Huy, Ocasio, and John, Haleblian and Rajagopalan, Canales and Vilà, and Fairbanks and Labianca) explore the cognitive and emotional micro-foundations of strategic thinking, and more broadly, the strategy-making process. Those in the second section focus on the larger context of organizations by theorizing about strategy making within more complex organizational resource accumulation and utilization processes (Dagnino and Mocciaro, Regnér, and Thomas, Kaminska-Labbé, and McKelvey). Those in the third section of the book provide detailed empirical studies of strategy-making processes using a variety of methodologies and analytic lens (Irmann, Salmador, and Bueno, Un and Cuervo-Cazurra, Coltman, Devinney, and Midgley, and Jett and George). Finally, those in fourth section are essentially meta-commentaries on research on strategy making, particularly the sorts of methodologies that are required to uncover meaningful data regarding how strategies unfold over time and in situ (Pajunen, Zan, Molloy, and Whittington).

The remainder of this introductory chapter provides a broader context for the present volume by reviewing the key historical issues in each of these four topical domains. We also highlight how the remaining 16 chapters are rooted in and extend longstanding conversations in the study of strategy making. Our goal is not so much to summarize the various contributions to the volume as it is to provide theoretical and historical perspective. To this

end, we begin each section describing each topical domain with some very brief thoughts about the history of research in that domain, emphasizing the one or two key issues that have motivated scholarly work in that area. We next discuss each of the chapters included in that domain, and draw out linkages between the chapters and prior research and theoretical efforts.

*The Cognitive and Emotional Foundations of Strategy Making*

Cognition is a fundamental facet of the strategy process. Porac et al. (1996) trace the cognitive turn in organization studies to two factors: (1) the work of Herbert Simon and its associates for showing how the quality of managerial decision-making could be contingent on the managers' psychology and (2) the relaxation of a strictly realist view of the world in which the environment imposes itself on passive perceivers in favor of a view where observers actively construct their environments thus making the environment at least partially contingent on the person or group who perceives it. As Kaplan, Murray, and Henderson (2003) explain there is a fundamental connection between strategy process and cognition because when characteristics of the situation are not sufficiently delineated to dictate action, it is the executive's mental model of the environment that must become a basis for strategic choice.

As per Huff's (2005) account, interest in cognition peaked in the 1980s then waned and it now seems to be undergoing a resurgence. Indeed, Eisenhardt and Zbaracki (1992) emerged from a comprehensive review of the literature of strategic decision-making with a proposed research agenda that emphasized cognition, first and foremost. Gavetti and Levinthal (2000) argued that when experience or information are insufficient, strategic decision-making must rely partially on an imagination-driven forward-looking logic of consequence, which they thought was neglected in the extant work. March and Simon (1958) establish an important link between decision-making and emotion through a rather general definition of conflict, which arises when standard mechanisms for decision-making experience a breakdown, when individuals or groups experience difficulty in selecting an action alternative, i.e., experience a decision problem.<sup>5</sup>

Despite the fact that strategy process research is rooted in cognitive approaches to decision-making and managerial thinking, one of the trends that came to our attention in developing this volume is the growing attention being given to "interest driven" and emotional influences in strategy making such as power and politics, status dynamics, and emotional contagion and

management. On the one hand, this trend follows from similar developments in psychology and sociology, and is thus not particularly surprising. On the other hand, given the complexity of strategy-making processes extended over space and time, one must wonder a bit about the feasibility of studying and theorizing the role of emotions in complex organizational decision-making. There appears to be considerable methodological and empirical hurdles to overcome in building emotion-based theories of the strategy process. Nevertheless, emotional influences on strategy making are intriguing given their implication that strategy processes might be directed, in part, by primitive psycho-social forces that are only partially under the control of the strategists themselves. These forces must be taken into account, managed to the extent possible, and certainly understood as vital elements in the process of creating strategic alternatives, committing resources, and implementing change. For this reason, we chose to begin the exploration of the micro-foundations of strategy making with Quy Huy's chapter, "An Emotion-Based View of Strategy Renewal."

Professor Huy, perhaps as much as anyone else these days, has been heavily involved in studying the role of emotions in strategy and organizations, particularly their role in strategic change. His chapter crystallizes insights gleaned from his research by suggesting that emotions can play both constructive and destructive roles when organizations are engaged in strategic transformation, and that the balance between these roles has much to do with how emotional processes are managed by the organization. According to Huy, successful strategic change is a function of collective mobilization, creativity, retaining key employees, and a receptivity on the part of the organization to embrace new ways of doing things. Each of these activities is facilitated by appropriate emotional substructures. Collective mobilization is encouraged by the elicitation of hope on the part of organizational members, creativity is stimulated by enjoyment and fun, openness to change is enhanced when management shows sympathy toward employees regarding the disruptions that take place during change, and employee retention is made stronger when organizational practices heighten attachment and identification with the goals of the organization. Huy's key insight is that the ability to create these facilitating emotional substructures is a particular kind of organizational "dynamic capability" that allows the emotionally skilled organization to manage change and renewal effectively.

Professor Huy's chapter is followed by Ocasio and John's piece entitled, "An Attention Based Theory of Strategy Formulation: Linking Micro and Macro Perspectives in Strategy Processes." This ordering nicely captures the theoretical transition between the emotional and cognitive foundations of

strategy in the sense that Ocasio and John's key argument is that strategy making is heavily influenced by formal and informal organizational "channels" that structure managerial attention, and thus the process of strategy formulation. Like emotions, attentional channels influence strategy making almost unconsciously, as ingrained routines and governance processes automatically sift through environmental and organizational cues, prioritize issues, and focus managerial decisions on those issues that have made it through this filtering process. Ocasio and John borrow from psychology the term "selective attention" to characterize strategy making, and it is indeed more than coincidental to note that selective attention has historically been considered one point of intersection between the emotional and intellectual processes of the brain. Ocasio and John take the notion of selective attention one step further, though, by suggesting that selective attention leads to selective retention of strategic initiatives and issues, and this retention forms one basis for a firm's competitive advantage. By linking selective attention with selective retention, the authors seem to traverse the boundary between strategy process and strategy content in the sense that the resources and capabilities that endow a firm with an advantageous competitive position are the result of a selective attention and retention cycle that has its roots in formal and informal organizational routines.

The next two chapters in this section of the volume are excellent follow-ups to Ocasio and John's arguments about managerial selective attention to strategic stimuli. The contributions by both Haleblian and Rajagopalan, and Labianca and Fairbank are focused on the particular stimuli to which strategists attend when shaping the direction of their organizations. In their chapter "Top Management Cognitions, Past Performance, and Strategic Change: A Theoretical Perspective," Haleblian and Rajagopalan highlight the important role of performance benchmarks in strategy making by suggesting that past performance creates double-sided forces on the strategy formulation process. On the one hand, persistently low performance creates pressures for changing strategic direction in order to improve the organization. On the other hand, persistently high performance creates pressures for strategic change by encouraging strategists to set stretch goals to enhance the organization's performance even more. However, while high or low performance cues thus have the potential to trigger strategic action, Haleblian and Rajagopalan suggest that the intervening strategy formulation processes involved are quite different in the two cases. Persistently poor performance triggers a reactive attributional process that guides the strategist's causal diagnosis of the situation. This diagnosis then determines whether the strategist will intervene to initiate a different course of action or

stay the course in cases where poor performance is attributed to events beyond the control of the organization. Persistently strong performance, on the other hand, heightens the strategist's internal feelings of accomplishment and efficacy, and thus creates the conditions for proactive strategic change by encouraging challenging performance goals and stretch objectives. In this way, the authors contend that performance stimuli alone are not sufficient to explain the link between past performance and future strategy. Rather, it is how the strategist interprets and explains past performance that is the immediate trigger for organizational change. Whether such interpretations are endogenous or exogenous to the attentional channels detailed by Ocasio and John is an interesting theoretical question to ponder.

Of course, the argument that past performance triggers strategic change begs the question of how organizations evaluate whether their performance is high or low. In their chapter, "Interorganizational Monitoring: Process, Choices, Outcomes" Labianca and Fairbank take on this general question by suggesting that organizations evaluate their performance in relation to other organizations in the environment. These authors argue that even internal aspirational targets are set within the context of interorganizational comparisons, and that such comparisons inform both internal and external stakeholders. According to Labianca and Fairbank, interorganizational monitoring is useful for categorizing an organization and defining its position in interorganizational space. On the basis of these categorizations, organizations then choose to compete with, emulate, and/or imitate other organizations that they deem relevant to their performance aspirations and objectives. Labianca and Fairbank suggest that organizations have a good deal of latitude in structuring their interorganizational comparisons, and that variability exists in both the depth and breadth of their monitoring proclivities. The authors use the example of a business school initiating a doctoral program to illustrate their point that organizations selectively monitor others in the environment according to the task at hand and the stakeholder audience to which the performance comparison is targeted.

We end this section of the volume appropriately with a contribution by J. Ignacio Canales and Joaquim Vilà entitled, "Sequence of Thinking and Acting in Strategy-Making." In one way or another, all of the preceding chapters in this section have argued that the emotional and/or cognitive processes involved in strategy making are important because they shape critical organizational outcomes such as the likelihood and/or efficacy of strategic change. And yet, the focus of these chapters has been primarily on detailing the emotional and cognitive inputs to strategy-making processes rather than the linkages between these processes and strategic actions and

outcomes. The linkages between strategy making, strategic action, and organizational outcomes are important topics in the next section of the volume. However, the chapter by Canales and Vilà represents a nice transitional set of arguments since their concern is with the micro-processes that connect thinking and acting together in the strategic management of organizations.

According to Canales and Vilà, strategy making is characterized by a dialectic between deliberate (primarily cognitive) and more emergent (primarily action oriented) processes that serve to bind strategy to action and outcomes over the course of time. Although thought and action are not necessarily coupled closely together in all cases, they can become so as thinking energizes action and action generates information that initiates reflective changes to earlier strategic frameworks. As action becomes more automatic and routinized, Canales and Vilà emphasize the importance of “triggering events” that interrupt strategic activity systems and induce strategists to move toward a more reflective mode of thinking that has the potential to initiate modifications to existing mental models. These events can be disconfirming and unexpected cues from the environment, or more deliberate requests to step back from and reconsider an organization’s strategies and priorities. Canales and Vilà, then, see strategy making as an ongoing activity system that is periodically interrupted by triggering events that induce changes in the content of strategies over time.

One of the implications of the Canales and Vilà paper is that organizations are characterized by a dialectic between periods of relative stability and periods of change and reorientation. This dialectic, in fact, has always been a central issue in theories of strategy and organization because of the tensions it creates when managing and organizing the oppositional forces involved. Properly designed strategic processes must be both dynamic and static. And, they must exploit existing and stable opportunities as well as explore, and even create, entirely new possibilities in the future. No doubt, this is easier said than done, and for this reason, a great deal of scholarly work has been done on the problem. The chapters contained in the next section of this volume represent some interesting recent efforts along these lines.

### *Institutional and Resource Foundations of Strategy Making*

The extensive selection – adaptation literature provides a useful backdrop to understand the institutional and resource foundations of strategy making.

An important theme in this literature is the extent to which intentionality matters (Lewin & Volberda, 1999). Such literature provides ways to conceptualize the extent and the limitations of managerial discretion. For example, in-depth accounts of the role of executive leadership in decision-making highlighted that an important role of the executive was to create, decision premises, i.e., a context where others would take decisions (Barnard, 1938; Simon, 1947). Such institutional features gained further texture with the work of historians such as Chandler (1962) and conceptual clarity with concepts such as corporate context (Bower, 1970) and strategic context (Burgelman, 1983). Penrose (1959) and the early thinkers in the field of international business such as Hymer (1960) developed what we would today call a resource – or competence – perspective to strategy making, i.e., how the resource endowment of the firm affects its decision-making. An interesting perspective on adaptation and on the limitations of managerial discretion was provided by March (1991) who highlighted the dilemma between the exploration of new possibilities and the exploitation of old certainties, and the complications of allocating resources between the two.

The exploration/exploitation framing struck a chord with strategy scholars as it suggests a sharp distinction between value creation and value appropriation. Value creation involves disruptive and new ideas that result in new markets and value chains being enacted, while value appropriation involves competitively capturing existing value in markets that already exist. In their chapter entitled, “The Development of the Resource Based Firm: Between Value Appropriation and Value Creation,” Destri and Dagnino compare and contrast the process issues involved in value appropriation and value creation, using recent work on the resource and knowledge-based views of the firm as their conceptual anchor. These authors point out that work on value appropriation is grounded in processes that are stable in nature and that are quite consistent with neoclassical conceptions of equilibrium competitive positions. Collections of firm specific resources and capabilities act as isolating mechanisms that build defendable competitive positions over time. Value creation, however, is dynamic and disruptive, and must be modeled as a more open-ended and subjective process of search and learning. In value creation, the key process problem is not so much how to deploy existing resources and capabilities as isolating mechanisms but how to recombine existing resources and capabilities in innovative ways to open up entirely new value chains. While different theoretical constructs have been used to understand value creation and appropriation, Destri and Dagnino argue strongly that Austrian economics provides a framework of constructs that can explain value creation and appropriation in an

integrated fashion. Along the same lines as Canales and Vilà, Destri and Dagnino suggest that the key here is to recognize that strategy processes are characterized by both periods of relative stability and periods of relative change. Strategy process, therefore, is cyclical in nature, with value creation and appropriation both reinforcing each other over long evolutionary periods in a firm's history. Destri and Dagnino go beyond Canales and Vilà, though, by making the leap from the micro-level of cognition and strategic thinking to the institutional level of resources, capabilities, and values.

In his chapter, "Adaptive and Creative Strategy Logics in Strategy Processes" Patrick Regnér carries on this line of argument by suggesting that the difference between creative recombination versus exploitation of existing resources is reflected in different strategic "logics" that become embedded in quite distinct organizational systems. What Regnér calls the "adaptive" logic emphasizes the exploitation of existing resources within existing market structures. This logic is embedded in an organizational design that tends toward more formal and centralized planning, more top-down decision-making, and an emphasis on allocative efficiencies. Regnér contrasts this with a "creative logic" that is oriented around the search for new resource combinations and new markets. According to Regnér, firms pursuing creative strategy making tend to have less formal and more horizontal organizational structures, decision-making that is dispersed throughout the firm, and an emphasis on flexibility and innovation rather than efficiency in organizational processes. In arguing this point, Regnér brings the discussion of value creation and appropriation, resource recombination and creation versus resource exploitation, down to the level of the organizational structures and processes that are more or less required to enact each strategic logic. Strategy making is thus an organizational and institutional process as much as it is an activity performed, or a mental model possessed, by individual strategists or top management teams.

In their chapter entitled, "Managing the MNC and Exploitation/Exploration Dilemma: From Static Balance to Dynamic Oscillation," Thomas, Kaminska-Labbé, and McKelvey complete the institutional section of the volume by extending this argument further. They consider creative versus adaptive logics within the context of the multinational corporate (MNC) form. These authors point out that a central problem in the design of multinational corporations is the balance between the exploitation of global efficiencies and the exploration of local market opportunities. Thomas et al. suggest that both logics must be present in order for the MNC to function effectively over time. The key question is the balance and emphasis placed on each of the logics at various points in an MNC's evolution. Thomas et al.

suggest that an organizational design that may be particularly effective in managing the simultaneous demands of exploration and exploitation is what they term a “tangled hierarchy.” Tangled hierarchies interweave top-down and bottom-up influence processes by creating layers of hierarchies with different properties within each other. For example, a centralized and vertically controlling hierarchy might exist along with a more horizontal set of relationships that are just as well understood and codified. At a given point in time, and depending on situational demands, one of these hierarchies will dominate decision-making and action within the organization. Tangled hierarchical processes are thus characterized by an oscillation between different influence regimes. Thomas et al. suggest that the problem of balancing global exploitation and local exploration in the MNC can thus be recast as a problem of oscillation between top-down and bottom-up hierarchical dominance. Rather than attempting to design an optimal “balance” between creative and adaptive logics, then, Thomas et al. suggest that the design problem is really a question of deciding on an optimal oscillation rate between the hierarchies associated with each logic. The authors go on to concretize their argument with a case study of oscillation in one particular MNC over time.

### *Contemporary Empirical Studies of Strategy Making*

Several methodologies are prevalent in empirical studies of strategy making. By far the most popular method is the single or multiple case study approach, followed by in-depth field studies, computer simulations, and experimental studies aimed at policy capturing; cf. Eisenhardt and Zbaracki (1992) for a comprehensive review of methods used in strategic decision-making studies. Important insights have been derived through all of these methods. While studies that embark in theory building or grounded theorizing in strategy process are increasingly accepted as a legitimate scholarly undertaking, the testing of theories of strategy process has proven particularly challenging because of the inherent difficulty of measuring process and performance over time.

Thomas et al.’s illustrative case study of hierarchical oscillation in an MNC, which closes the previous section makes for a nice transition to this section of the volume consisting of five empirical studies of strategy making processes in action. We have chosen these five papers because they represent a fairly broad swath of the topics that are currently being investigated empirically by strategy process researchers around the world. The topics are

thus necessarily varied, with no particular ordering. Together these chapters are representative of methods and topics in contemporary empirical strategy process research.

In his paper entitled, “Communication Dissonance and Pragmatic Failures in Strategic Processes: The Case of Cross-Border Acquisitions” Olivier Irrmann tackles the thorny problem of integrating international acquisitions into productive organizational subunits of the acquiring firm. It is well known by now that most mergers and acquisitions are only partially successful at best, and one key factor that has been mentioned to account for this underperformance is the difficulty of integrating two separate companies with distinct cultures, organizational infrastructures, and histories. Irrmann rightly argues that the integration problems entailed in cross-border mergers and acquisitions are particularly acute, and further suggests that these problems are rooted in cultural differences in communication patterns and styles. In a study of French and Finnish firms, Irrmann concludes that three sources of communicative “dissonance” make cross-border integrations difficult: differences in communication style (or how to communicate particular issues), communication content (or what to say in a given context), and communication media (what communication channels to use and when). Irrmann’s insights are important here because, as he points out, the prior literature on integration problems has tended to focus on general company differences in “culture,” “structure,” and “reward systems.” While Irrmann does not discount the importance of these variables in the integration process, his more “micro” approach to studying communication differences reveals that daily interaction patterns and preferences might be just as influential. As such, Irrmann’s paper adds even more grist to the argument that the best laid merger and acquisition plans must eventually pass the test of integration implementation for any benefits to be realized.

María Salmador and Eduardo Bueno consider the problem of strategy making in complex and fast-moving environments in their paper, “Strategy-Making as a Complex Double Loop Process of Knowledge Creation: Four Cases of Established Banks Reinventing the Industry by Means of the Internet.” In many ways, their research is quite consistent with the arguments by Canales and Vilà, Destri and Dagnino, and Regnér that strategy making is an ongoing cycle of action and reflection, exploitation, and exploration. Salmador and Bueno study the implementation of internet banking by different financial services groups in Spain. This venue is particularly revealing of strategy-making processes given the many uncertainties and ambiguities of internet commerce in general. The authors track the micro processes of strategy making in this technologically complex and volatile environment

and show how banks enacted their emerging strategic space via embedded cycles of action and experimentation, reflection, and codification of the lessons learned by such experimentation, the encouragement of creativity and new ideas, and the implementation of simple rules and routines that helped to cut through the complexities of the new market. In this way, Salmador and Bueno's study provides empirical detail for the arguments in earlier chapters on strategy making in this volume.

The focus of Un and Cuervo-Cazzura's chapter on "Top Managers and the Product Improvement Process" is the role of senior managers in product evolution rather than new-product development. Noting that the strategy and organizations literature has tended to emphasize the role of middle managers and bottom-up creative processes in organizational innovation and new product development, Un and Cuervo-Cazurra argue that product improvement differs from new-product development in that existing products are already contributing to a firm's profit position. Thus, top managers have an incentive to get involved. Moreover, the authors suggest that product improvement often requires changes in existing organizational routines, and the involvement of top managers is sometimes required to break down the political barriers preventing such changes from occurring. For these reasons, Un and Cuervo-Cazurra suggest that top managers might play a crucial role in guiding product evolution, and they test this argument in a sample of 30-product improvement projects within six different firms. The authors conclude that top managers have a number of key functions in product evolution such as the evaluation of product performance, the initiation of product improvement processes, and the monitoring of resulting improvement initiatives. Un and Cuervo-Cazurra's paper reminds us that top managers cannot be ignored as important sources of innovation in organizations.

Quintus Jett and Jennifer George explore the relationship between strategy and environmental dynamism in their chapter entitled, "Emergent strategies and their Consequences: A Process Study of Competition and Complex Decision Making." It has often been suggested in the strategy literature that dynamic environments constrain deliberate and planned strategy-making processes because such environments change too quickly for long-range plans to be enacted. This has led to the argument that dynamic environments call for much more open-ended and emergent strategies that evolve over time. Jett and George use a business simulation game to track the nature of strategic thinking and decision-making over time in a competitive environment. Addressing the difference between emergent and deliberate strategy-making processes in organizations, the authors show

how the best performing top management teams in their study used a combination of emergent experimentation and deliberate forward thinking to shape the direction of their company. According to Jett and George, deliberate and emergent strategies are not mutually exclusive, and in fact might even be complementary in the sense that forward looking anticipations often act to structure and direct more open-ended exploratory thinking.

The last chapter in this section, “Strategy Content and Process in the Context of E-Business Performance” by Coltman, Devinney, and Midgley, examines the relationship between certain organizational and environmental attributes and organizational performance. These authors focus upon the classic distinction between strategy content (e.g., environmental pressures, resources, and capabilities) and strategy process (e.g., managerial beliefs) and suggest that both process and content are important determinants of a firm’s performance. They test this argument within the context of e-business implementation. They find that both industry pressures to implement e-business processes in an organization as well as managerial commitment to the benefits of doing so are both important determinants to the success of the implementation. Coltman et al. conclude that the content vs. process distinction unnecessarily tends to orient strategy scholars to one or the other set of constructs in their research. Coltman et al. see great value to remembering that both sets of variables are important in shaping a firm’s future performance trajectory. That is, the resource and environmental constraints that impose themselves on a firm are important variables that influence the firm’s fate. However, how strategists within the firm interpret and understand those constraints and their commitment to any resulting course of action are critical as well.

#### *Meta-Commentaries on Methodologies for Strategy Process Research*

The three previous sections of the volume contain chapters that are largely arguments pertaining to particular aspects of strategy making, or, as in the immediately preceding section, are actual research studies that subject such arguments to empirical testing. Lurking in the background of these arguments, however, are a number of issues regarding the appropriate techniques and methods for studying strategy making in action. The prior sections reveal a number of methods and analytical lenses, from case studies to experimental simulations to questionnaire surveys. Aside from the typical questions that such methods usually evoke (e.g., questions of reliability and

validity), are there broader issues at stake that are important to keep in mind as one embarks on strategy process scholarship?

In this regard, an obvious and fundamental question that Van de Ven (1992) asks is what is the meaning of “process” that guides the research design. Is process a variance theory, a category of concepts that refer to actions, or a sequence of events that describes how things change over time? A more pragmatic but equally vehement suggestion comes from Pettigrew (1992), an advocate of a longitudinal comparative case-study approach, who emphasizes the importance of reducing complexity through research design when exploring the link between process and outcome by sticking with the careful comparison of a small number of (6–10) cases and choosing clear outcome measures. Research in strategy process, he says, is the pursuit of the proximate, not final causes, and of multiple intersecting conditions, an argument that resonates with Robert Burgelman and Yves Doz’s comments during our original workshop in Denver on the importance of theorizing at the middle range. Reflecting on his own extensive experience researching strategy process, Mintzberg (1979) recognizes an emerging strategy of “direct” research, a purely descriptive approach based on small but in-depth samples, which is purely inductive, and insists that systematic data be supported by anecdotal data.

A final “meta” issue to keep in mind for strategy process research is the level of analysis. In a sense, the main concern with large sample data collection has to do with how detailed each data point really is. Research based on careful, detailed, in-depth comparisons amounts to opening up the black box of the firm and becoming more “micro.” Herbert Simon once urged economists to reduce the reliance on sophisticated statistical methodologies to extract information from noisy data and instead collect data at a lower level of analysis, i.e., to become more micro. We echo Williamson’s (1985) response to Herbert Simon’s challenge: how micro is micro?

The authors of the three chapters in this last section of the volume raise such broader issues about methodology in their papers. All three chapters are essentially manifestos arguing for a particular meta-theoretical stance in the study of strategy making. For this reason, we have labeled the chapters “meta-commentaries” and have combined them into this concluding section of the volume.

In the first chapter, Pajunen champions the idea of explanation with causal mechanisms in processual strategy research. He first describes the data analytic techniques of event-structure analysis (ESA) and Boolean comparison/qualitative comparative analysis (QCA) as well as the idea of two-level theories. He then suggests how these approaches can be linked and

applied in the analysis of organizational processes and causal mechanisms. He demonstrates the application of the methodology by analyzing two organizational decline and turnaround processes.

Luca Zan then takes a strong historical orientation in his chapter, “Future Directions from the Past: Management and Accounting Discourse in Historical Perspective.” Zan advocates the value of a historical perspective on the strategy process. By examining shipbuilding in Venice he shows that complex organizational settings and strategies existed even then. He challenges the notion that complexity as a cornerstone of strategic management is a recent invention. He challenges the foundations and hence the novelty of strategy research. Most of it he says is wrapping up with new labels and rediscovering what has already been said. Whatever is left is true innovation. He asks, what can history learn from strategic management? He then asks another important question: where can one find the strategy process in an organization? He claims that strategy process is not just a kind of cognitive social thing but that it has a strong material element that it is embedded in the routines of the organization. Strategy process he claims is to be found in the detailed embedded practices, the calculative routines, and the productive routines, which can be found in the accounting practices of the shipbuilders in Venice.

In the third and last chapter of this section, Molloy and Whittington start from a simple but very powerful premise. Reorganizing happens more often, so some of it becomes routinized and is manifested in standardized practices. These practices however are adapted to the realities of the organization. Apparently bridging routines to reality is where a lot of inspiration and imagination has to be devoted. What looks like a small part of the reorganization is actually a big soak of imagination. And that is where the process resides. Of course, this paper evokes the specter of overwhelming level of detail in data collection.

## CONCLUDING THOUGHTS

This volume is the fruit of our journey through contemporary strategy process research and theory. In a sense, this chapter reflects a hard-won consensus among the three of us. We came from different vantage points into strategy process. We know that the literature is voluminous and extensive and we do apologize to the many contributors whom we have cited only indirectly (through the review of others) or to whom we failed to reflect

properly their hard-won and passionate contributions to the field of strategy process.

We emerge from this journey convinced that the field is rich and vital. We had lively discussions among ourselves and with others, discussions that touched many themes. Two of those themes however seemed to recur and we want to mention them here. The first was the enthusiasm for the field and whether it is likely to persist. We believe that the answer is a resounding yes! In a sense, the prospects of the field reflect the prospects of conducting credible empirical research, which in turn reflect the prospects for data availability. Our prognosis is based on an assumption of increasing availability of information about strategy process. That is because strategy process happens more often than it used to and it is becoming increasingly participative. Hence process will be more observable.

The second theme was the necessity of deriving normative prescriptions even when supporting evidence is precarious. Clearly, demanding full scientific proof, i.e., demonstrating correlation and causality, is a tall, possibly unrealistic, requirement in the near term for the field of strategy process. So, it is likely that a normative orientation will continue despite the fragile underlying empirical backing. We believe that for the field to thrive researchers need eventually to give back to the phenomena they study and normative advice is an important way to do so.

The question that should be asked however is: who should be entitled to provide such normative advice? Are there minimum “descriptive” qualifications that a researcher should aspire to before providing normative guidance? Is a modicum of familiarity with the phenomena at all necessary to do so? Should on the other hand total disregard for systematic data collection and analysis nullify such efforts? Should those who prescribe based solely on intuition or anecdote be ignored? Which minimum pre-requisites should those who aim to prescribe be asked to meet?

We do have our thoughts about these and many other issues in strategy process research but we will keep them to ourselves. We tried to reflect the community and what the community thinks. And it is in this spirit that we part company and leave you now to enjoy the chapters and find your own answers to such questions. We hope you will enjoy the scholarly community of strategy process research as we have discovered it.

Gabriel Szulanski  
Joseph Porac  
Yves Doz  
*Editors*

## NOTES

1. See, for example, James Brian Quinn's (1989) commentary on the application of Logical Incrementalism
2. See Bower et al. (2005) for a summary of the RAP model, its extensions, and its applications.
3. For more details on the PDW please visit <http://sw-mos.insead.edu.sg/workshop3/PDW.htm>.
4. For more details on the INSEAD conference visit <http://sw-mos.insead.edu.sg/workshop3/aism.htm>.
5. See Eisenhardt and Zbaracki (1992) for a thorough review of the power and politics perspective of strategic decision-making. As they point out, "The heart of the political perspective is the process by which conflict is resolved among individuals with conflicting preferences." Thus conflict provides a useful link between strategy process and emotion.

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