

RESEARCH ON STRATEGIC DECISIONS: WHERE DO WE GO FROM HERE?

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ABSTRACT

This chapter gives our personal view of the way forward for research on SDs. To do so it draws on a number of recent reviews of the field and synthesizes the views of many other researchers. We conclude that future research should focus on *outcomes* (both performance and other outcomes such as learning, innovation and commitment), be *integrative* (taking into account the context and content of SDM as well as the process and outcomes), and use more *rigorous and consistent methods* (type of research method, terminology, and measurement).

The main aim of this book has been to describe the current state of the art of strategic decisions (SD) research by asking leading researchers to summarize their own areas of interest within this field. Several of the authors have also made suggestions as to possible future directions for SD research, which we draw together in this chapter. We also draw on three excellent recent reviews (Eisenhardt and Zbaracki 1992, Rajagopalan et al 1993, Schwenk 1995) as well as on many other, mostly earlier, publications. The chapter gives our personal view of the way forward for academic research on SDs, based on a synthesis of these others' suggestions, plus our own emphasis on the need to make such research more relevant to managers, as discussed in Chapter 17. Our substantive proposals are grouped under the following five headings:

- Increasing focus on outcomes to increase relevance
- Exploring the influence of context on SDM processes and outcomes

- More generally, the need for more integrative research
- Bringing the CEO and top management team into SD research
- Three emergent themes: learning, implementation, and information systems

Our overall impression from the literature is that, while strategy content researchers have developed a common vocabulary and a cumulative body of knowledge, strategy process researchers, including researchers on strategic decision-making (SDM), cannot make the same claim.

One conclusion from our discussion in Chapter 17 is that the great diversity in the methods and measures of past studies has made it impossible to arrive at a clear set of undisputed empirical generalizations (Barwise 1995). Several of the chapters in this volume have referred to topics (eg the process-performance relationship, the environment-process relationship, the consensus performance-relationship, and the role of conflict) where different studies have produced contradictory results. In the sixth section of this chapter we therefore give a number of methodological suggestions to address these problems: large-sample field research, longitudinal studies, some laboratory studies, common terminology, better operationalization and measurement, and more validation of retrospective data.

The chapter concludes with a brief list of substantive and methodological suggestions for future research on SDM.

INCREASING THE FOCUS ON OUTCOMES

While strategy content researchers usually link competitive and/or resource positioning to performance, process researchers and especially SDM researchers have been less preoccupied with the impact on performance. Even when performance has been measured, there is often little convergence even among studies focused on the same constructs (eg Fredrickson and Mitchell 1984, Miller and Friesen 1983). Bower (1997) stresses that there is little work that tries to explain the relationship between such process characteristics as comprehensiveness or decentralization and performance. Rajagopalan et al (1991) reported that, although 19 out of the 31 SD research studies they reviewed included economic and/or process outcomes, the results provided little useful advice for practicing managers. For example, with some exceptions (eg Bryson and Bromiley 1993, Bourgeois 1980, Bourgeois and Eisenhardt 1988, Dean and Sharfman 1996, Dess 1987, Eisenhardt 1989, Fredrickson and Mitchell 1984) empirical research has not yielded managerially relevant results on the relationship between organizational performance and dimensions of SDM processes (Rajagopalan et al 1993).

As mentioned in Chapter 17, there is some evidence that the SDM process may hold the potential for building competitive advantage (Hart and Banbury 1994). This reinforces the argument for research linking SDM to corporate performance (eg what are the trade-offs between decision-making rationality, formalisation, participation, politics, timing, etc. in the pursuit of superior performance?) (Schendel 1992).

Future research on SDs should aim to provide both descriptive validity and prescriptive relevance. According to Bateman and Zeithaml (1989), research needs to identify and explicate *relationships which are not obvious to managers*. Studies examining these links are likely to have considerable practical impact.

This leads to another question. What sorts of output variables should be measured? Most of the existing research employs measures of economic performance (eg Rajagopalan et al 1993). But is economic performance the only significant outcome variable? Should we not also focus on other outcome variables (eg learning, innovation, satisfaction, commitment)? We believe so.

CONSIDER THE BROADER CONTEXT OF SDM

Pettigrew (1985) considers contextualist research as the natural way to bridge SDM theory and practice and stresses the need for theoretically sound and practically useful research examining the context and the process of change. More specifically, he raises two major research questions: (1) what is the role and significance of context in shaping decision processes? and (2) does the nature of the decision problem and interests shape the process more than the organizational context within which the process proceeds?

Schneider and DeMeyer (1991) call for further research into the factors which are expected to contribute to the interpretation of a strategic issue (threat, opportunity, crisis etc) as well as the subsequent responses (eg scale of response, proactiveness-reactiveness). According to these authors, these factors can be located in the manager's individual characteristics (eg locus of control, experience, introversion/extroversion), the group dynamics, the organizational context (eg centralization, formalisation, politics, ideology), or the environmental context (eg economic, market, cultural, political). All these contextual domains could be the focus of future research. Hart (1992: 346) stresses the need for "*multi-industry survey studies [which] would help to establish the general relationships among strategy-making processes and firm performance*". Wally and Baum (1994) ask for further research on the impact of the broader context on the pace of SDs. The same has been argued by Rajagopalan et al (1993), Bryson and Bromiley (1993), Papadakis et al (forthcoming) and others.

Several of these studies point to the limitations of existing research on the context and the process of SDs: (i) there has been little research on the influence of broader context on SDM, (ii) most of the studies focus on a limited number of antecedents while ignoring other important sources of influence on SDM (model underspecification), (iii) most of the studies focus on just one characteristic of the process (eg comprehensiveness, politics, decentralization), despite the fact that SDM is multidimensional, and (iv) much of the evidence is contradictory and far from establishing a coherent theory.

Therefore, we are not yet able to answer the question "what are the key influences on the process of making SDs?". Is it the external environment as the population ecologists would argue, or the top management (CEO and TMT) as the proponents of management choice theories suggest? Do internal enterprise

characteristics significantly affect the process? How much do different decision-specific characteristics, as perceived by management, lead to different treatment of the decision? How does past performance influence SDM? What are the role and impact of formal planning systems?

These questions remain largely unanswered (Bateman and Zeithaml 1989, Bryson and Bromiley 1993, Pettigrew 1990, Rajagopalan et al 1993, 1997, Schneider and DeMeyer 1991). It seems that what is needed is a more holistic approach which views the SDM process as subject to multiple influences, and examines the effects of these various factors.

National Culture

Most research on SDs (as on any other topics in management and strategy) comes from the USA. In an era of increasing globalization, it is important managerially as well as scientifically to investigate how closely these results apply to SDs in other country settings. As Bower (1997) and Schwenk (1995) argue, we know little about the influence of national culture on SDM. Two chapters in this volume directly address this issue. Carr (1997) argues that the British culture is conducive to a financial orientation, as opposed to the German culture where companies are more strategically focused, more proactive and more thorough in their strategic debates. This means that the country context has a direct influence on SD processes.

Lu (1997) contrasts Chinese and British cultures in the making of strategic investment decisions (SIDs). He finds both similarities and differences. Similarities are identified in the overall pattern of SID-making, while differences are found in the influence of the wider context on SDs (eg government interventions). Lu concludes that SIDs have their own intrinsic nature which largely determines the pattern of SID processes; the broader context only causes variations in these processes. Shrivastava and Grant (1985) reached similar conclusions based on their studies of SDM in Indian firms.

We need far more research on SDM in different national contexts. We may in due course find that "*many of the conclusions about strategic decision making developed in the US context will have to be modified in order to be applicable across cultures*" (Schwenk 1995: 484).

Corporate Governance Structure

Another largely neglected influence on SDM is the corporate governance structure, especially the type of ownership and control. This has attracted much recent attention in the literature on the markets for corporate control and privatization. Several studies provide evidence on the implications of corporate governance/control (eg Lioukas et al 1993, Mintzberg 1973). The Bradford group found different decision-making patterns in British versus foreign-owned companies operating in Britain (Mallory et al 1983). Moreover, as suggested by several authors, public vs private ownership may decisively affect decision-making processes (eg Lioukas et al 1993). Yamamoto (1997) reports that corporate

governance has a significant influence on SDM by causing variations in planning, the generation of strategic projects, outside intervention, senior management's direct role in the process, and the emergence of the SD (deliberate vs emergent). These results suggest that the differential impact of governance structure on SDs may be a fruitful avenue for future research.

NEED FOR INTEGRATIVE RESEARCH

There is not only a scarcity of research focusing on outcomes and contexts, but also a paucity of integrative research more generally. Several authors have pointed out that the study of SDM has remained highly fragmented (Marsh et al 1988, Rajagopalan et al 1997, Rowe 1989). Bower (1997) argues that most articles in the area at best try to add 'another brick in the wall'. That is, researchers adopt the implicit assumption that any incremental effort adds to cumulative knowledge.

Most of the large-sample articles incorporate a small number of variables while ignoring the potential impact of a host of other (perhaps more important) variables. Past reviews of the literature (eg Rajagopalan et al 1993) have highlighted this problem. They have shown that most studies of SDM have a limited scope, focusing on only one main set of explanatory factors. For example, when studying the impact of context on process characteristics, most of the research efforts focus on one set of antecedent variables (eg corporate environment or internal structure/systems). This results in model underspecification and a confusion as to the relative importance of the omitted antecedent variables. It is quite likely that many of the relationships reported are confounded by a large number of factors extraneous to the research question under investigation.

According to Bower (1997), research of this type is nothing but 'partial derivatives' with limited research value. Future research should try to overcome the problem of model underspecification and the mindset of adding another brick in the wall. One possible solution is large-sample field research which takes into account as many important variables as possible and is based on rigorous analysis (Nutt 1984, Hickson et al 1986, Cray et al 1988, Sharfman and Dean 1997).

Ideally, we would welcome research which integrates process, context, and outcomes. With some notable exceptions (eg Bryson and Bromiley 1993), large-sample research on SDs has failed to address this gap. Others also share this view (eg Butler et al 1993, Hart and Banbury 1993, Priem et al 1995). Undoubtedly this is very demanding in terms of both time and financial resources, but it is needed in order to move the field forward. An additional benefit of these studies is that there would be a greater cumulation of findings and it would be easier to replicate the research design.

SDM and the Content of Strategy

There is a growing concern that the two main streams of strategy research, namely strategy process and content, have evolved in isolation (eg Schwenk 1995, Zajac and

Bazerman 1991). There is also a question as to whether this dichotomy between process and content is meaningful. According to Schendel (1992: 3), “*good strategy must be created, and it must be executed, and it is unlikely to be good strategy, absent luck, without understanding ex ante both of ‘what’ constitutes a winning position and ‘how’ such positions can be gained and sustained. The organization should..... understand both content and process, including their intimate relationship*”. The SDM field offers an opportunity to close that gap. According to Zajac and Bazerman (1991) and Maritan and Schendel (1997), we need more work which deals with the point of contact between SDM and competitive strategy.

BRING THE CEO AND THE TMT INTO THE SD RESEARCH

Two important themes of strategy research over the last ten years have been (a) the role of *top management* (Bantel 1993, Hambrick and Mason 1984, Lewin and Stephens 1994) and (b) the *process* of making SDs (Hart and Banbury 1994). Since Hambrick and Mason’s (1984) influential paper on “upper echelons”, much emphasis has been placed on the role and significance of top management (ie the CEO and/or top management team). This research stream has mainly explored the top management’s influence on: (i) corporate strategies (Finkelstein and Hambrick 1990, Miller and Toulouse 1986), (ii) innovation (Bantel and Jackson 1989), (iii) performance (Eisenhardt and Schoonhoven 1990, Haleblan and Finkelstein 1993, Norburn and Birley 1988, Smith et al 1994), (iv) organizational structure (Miller and Droge 1986), and (v) planning formality (Bantel 1993).

There has been little empirical work on the link between these two themes, ie top management (TM) and the process of making SDs. As Rajagopalan et al (1993: 364) stress: “*research relating organizational factors such as top management team (TMT) characteristics to strategic decision processes is limited*”. Others have argued along similar lines (Bantel 1993, Huff and Reger 1987, Lewin and Stephens 1994, Smith et al 1994). Moreover, the few studies which have been done on the links between TM characteristics and SDM processes have produced mixed results. The influence of TM on SDs therefore remains unclear. If upper echelons theory is to advance our knowledge of the role of the CEO and the TMT we need a better understanding of the impact (if any) of top managers on SDM processes and the underlying characteristics which are important (Smith et al 1994). Thus, we need to bring the CEO and the TMT back into the SD research.

THREE EMERGENT THEMES: LEARNING, IMPLEMENTATION, AND INFORMATION SYSTEMS

In addition to the above four issues for future research on SDM, we here note three other, emergent themes.

(a) Adaptiveness and Organizational Learning

Eisenhardt and Zbaracki (1992) discuss the three fundamental paradigms of SDM research (bounded rationality, power and politics, and the garbage can model of choice). They acknowledge that research within these paradigms has greatly increased our understanding of managers and decision-making, but argue that each of these perspectives has its own limitations.

Eisenhardt (1997) echoes these thoughts but goes one step further to propose a new paradigm to create a new broader agenda for SDM research. A prerequisite for this is to open up our conceptions of cognition and politics and to incorporate such other notions as *insight, intuition, emotion, and conflict resolution*. She then draws from the context of arts (eg jazz and rock). One common characteristic of these arts is *improvisation*. She argues that this improvisational model can be applied to SDM. Indeed, much of her latest research with Bourgeois and Kahwajy seems to support this novel view of decision-making. This paradigm looks at the top management team as a jazz band and the decision itself as music. It could be part of a broader improvisational metaphor that may push thinking about SDM beyond the traditional paradigms.

(b) Implementation

In a recent article, Dean and Sharfman (1996) argue that, in addition to the process and the context of SDs, one promising area for future research is the implementation of the decision. As the smooth implementation of SDs has a large, perhaps dominant, influence on the outcome of SDM (eg effectiveness, commitment, learning) this topic warrants further investigation. Unfortunately, the implementation side of SDs has been largely overlooked in SD process research (eg Bourgeois and Brodwin 1984, Nutt 1993, Skivington and Daft 1991). It deserves more attention in future research.

(c) Impact of Information Systems on SDM

Molloy and Schwenk (1995) were perhaps the first to provide empirical support that information technology can improve both the efficiency and the effectiveness of the SDM process. Given the ubiquity of corporate information systems, and their enormous capital and operating costs, we need much more work on their impact on the SDM process and its outcomes.

RESEARCH METHODS

There is little doubt as to the richness of SDM research over the last 35 years. Unfortunately, the cumulative value of the results has been reduced by a lack of consistent and rigorous research methods. Based on the literature, we here propose a number of guidelines.

Need for Large-Sample Field Research with Rigorous Testing

As argued in Chapter 17, much of the most managerially relevant SDM research is based on a fairly small number of cases studied in depth. Future research should not abandon this rich, insightful approach. On the contrary, it should try to build databases based on larger samples of SDs studied in-depth. This would give researchers the benefit of both rigor (through larger samples) and substance (through wider, multivariate descriptions, including outcomes).

This mainly suggests that a new wave of research is needed which has these qualities and is trying to test the existing theory. The field has developed some rich descriptive and theoretical results (although these sometimes contradict each other). What is missing is theory testing and theory integration (Rajagopalan et al 1993, Papadakis and Lioukas 1996). Moving towards this objective (ie testing the normative and predictive properties of existing research output) will enhance the value of academic SD research to both academics and managers.

This type of research is extremely demanding, but it is probably the best way forward. Large detailed databases need to be compiled similar to those collected by Nutt and by Sharfman and Dean. These databases take a lot of time to build, but once in place their value is irreplaceable. Doctoral students and other researchers can exploit these databases and extend them. For example, a doctoral student could use an existing database relating SD context and process, by conducting further fieldwork on some of the same companies but focusing on outcomes. This type of exploitation of existing databases offers many opportunities.

Longitudinal Research

Most of the research in the area has been cross-sectional. But in order to understand the causal linkages between the antecedents and outcomes of SDM processes, we need longitudinal research designs (Pettigrew 1990, Rajagopalan et al 1993, Van de Ven 1992). Longitudinal designs allow researchers to examine the multiple and intertwined links between context, process, and outcomes, as well as to achieve a better understanding of the degree and direction of causality. In addition they can offer researchers a much better understanding of how the SD evolves over time and the factors influencing the process of making the decision as well as its implementation. The limitations of longitudinal research are its cost in time and resources (both people and money) and therefore the difficulty of collecting enough observations to test hypotheses or conduct multivariate analyses.

Laboratory Research

One problem with SDM research is that it is rarely possible to observe the process and its characteristics during real time. Because of this, most researchers have resorted to either retrospective reporting or laboratory research. The latter avoids the problems of ex-post rationalization, memory failure etc. But laboratory research also has significant limitations which is why only about 3% of the published empirical results in strategic management use this methodology (Schwenk 1995).

Nevertheless, this type of research has produced some interesting results on SDM (eg Fredrickson 1985). Laboratory research has a role because it allows for the controlled manipulation of strategic variables. Yet people using this methodology should be cautious in their selection of subjects (eg college students may considerably differ from executives) and in interpreting their results, as the artificiality of the context may severely impair their validity. Critics argue that the artificial laboratory setting cannot remotely simulate the real-world SDM context (eg Locke 1986).

Adoption of a Common Terminology

It has often been observed that SDM research is replete with buzzwords and lacks a common terminology (eg Bower 1997). We agree. For example, within the conflict literature several of the most prominent researchers use quite different terminology to describe virtually the same constructs. Schwenk (1997) uses the terms of *positive vs negative conflict*, Eisenhardt et al (1997) *substantive vs interpersonal conflict* and Amason (1997) *cognitive vs affective conflict*. Others (eg Kirchmeyer and Cohen 1992) speak about *constructive conflict*. If we are to progress as a field, we need more common terminology.

Careful Operationalization and Measurement of Constructs

Empirical research on SDM suffers from a plethora of definitions and operationalizations. Take for example the comprehensiveness/rationality dimension. There are as many different operationalizations of this construct as researchers in the field. (eg Fredrickson, 1985, Papadakis and Lioukas 1996, Sharfman and Dean 1993).

A related methodological issue of great importance is construct validity and reliability. Here again, little attention has been paid. We need to use more multi-item measures and to provide the necessary reliability and validity tests.

Validation of Retrospective Data

The widespread use of invalidated retrospective data is a major methodological problem.

Any study based on participant recall, the dominant method of studying SDM, has inherent limitations (Bouchard 1976, Huber and Power 1985, Kumar et al 1993). A number of procedures have been suggested to reduce their impact, (Bourgeois and Eisenhardt 1988, Huber and Power 1985, Kumar et al 1993). First, archival records documenting the process of SDs and its characteristics can be collected. Second, discussions can be recorded so that the researcher has direct access to the original discussion, at later stages (Barwise et al 1986, Bourgeois and Eisenhardt 1988). Third, distortion and memory failure problems can be reduced by selecting recent decisions (Mintzberg et al 1976), by interviewing multiple informants, but only those with an intimate knowledge of the process (Kumar et al 1993), by adopting a “funnel sequence” in conducting interviews (Bouchard 1976),

and by cross-checking interview data with other data sources (eg documents, reports, minutes of meetings, Barwise et al 1986).

Another major consideration is the minimization of common method bias. To correct for such effects several precautionary measures can be taken: First, some of the main variables should be archival. Second, more than one questionnaire can be used and answered by different managers so that dependent and independent variables are collected from different respondents. Third, the items used in the analysis should be distributed throughout a lengthy interview. Fourth, scale anchors should be reversed in several places to reduce and compensate for the development of response patterns.

CONCLUSION: PRIORITIES FOR FUTURE RESEARCH

We share the view of Hart (1992) and Rajagopalan et al (1993) that there is a lack of an integrated conceptualization to provide an umbrella framework for ongoing research. We can safely argue that no accepted integrated theory of decision-making exists, partly because some of the theoretical standpoints remain mutually exclusive. We believe that the field, would benefit greatly from adopting a more integrated and cumulative approach. We also believe that research which is both rigorous and relevant (ie Quadrant IV research, in terms of our framework in Chapter 17) represents the best way forward. Researchers seem to be moving in this direction, especially from Quadrant I (rigorous, descriptive research). Our own wish-list of substantive and methodological priorities for future research on SDM is as follows:

Substantive Priorities

1. Focus on research which is both rigorous and managerially relevant, mainly by adopting more consistent terminology and measurement and by including measures of economic and other (eg learning) outcomes.
2. Integration of the context, process, and outcomes of SDs, partly to clarify the contingencies.
3. Attempt to bridge the gap between strategy process and strategy content research, and
4. Exploring the impact on SDM of different national cultures and governance structures.

Methodological Priorities

1. Rigorous testing large-sample field research designs based on SDs studied in-depth
2. Increased use of longitudinal research designs and programmatic research (eg using doctoral students)
3. Validation of retrospective data (eg using multiple organizational sources)

4. Adoption of common terminology and measurement.

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