# Part I

## INTRODUCTION

# 1

# Crucial Trends and Issues in Strategic Decision Making

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## INTRODUCTION[1]

Studies of strategic decision making are central to organization theory. March and Simon (1958) suggested that managing organizations and decision making are virtually synonymous. The dynamics of organizing require a deep understanding of decision making. As organizations grow and become more complex, decision making becomes a central activity. Managers are expected to make choices among alternatives that are often uncertain and to choose wisely in order to benefit both the organization and its key stakeholders. This has prompted researchers to study decision processes to find ways in which decisions can be improved.

The study of decision making has spanned a number of levels of analysis, which range from individual cognition to the cultural characteristics of nation states. Many disciplines inform our knowledge from mathematics to behavioural theories of social science. The term *strategic* decision making is often used to indicate important or key decisions made in organizations of all types. The term *organization* includes any collective social, economic or political activity involving a plurality of human effort. Strategic decisions emphasize the social practice of decision making as it is carried out among and between individuals in the organization. When studying decision making, both the organizing of decision activity as a collective phenomena and the cognitive processes of individual decision makers take centre stage.

Strategic decision making is more than computation carried out to make judgements and choices. Various branches of mathematics can inform us about risk, options, game theory and choice. All have their utility in understanding choice processes, but are less useful when considering how people in organizations make decisions. As an example, consider the most well known variant of game theory (decisions between two players), the prisoners' dilemma. Two criminals in separate cells have to decide whether to betray each other, having agreed not to betray one

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another in advance of the game. The greatest pay-off results when both prisoners stick to their agreement, but most betray each other and experience a significantly reduced pay-off. Computational mathematics help the players maximize their returns, but this is just part of the strategic decision-making story.

Why consider *strategic* decisions? As we will discuss later in this chapter, the term strategic has become more confusing than enlightening. Popularized by Mintzberg *et al*. (1976), strategic decisions are seen as large, expensive, and precedent setting producing ambiguity about how to find a solution and uncertainty in the solution's outcomes. Once implemented, a strategic decision stipulates premises that guide operational decisions that follow. A strategic decision is often difficult to reverse once human and financial resources have been committed to their cause. Furthermore, strategic decisions have the following characteristics:

- They are elusive problems that are difficult to define precisely.
- They require an understanding of the problem to find a viable solution.
- They rarely have one best solution, but often a series of possible solutions.
- Questions about trade-offs and priorities appear in the solutions.
- Solution benefits are difficult to assess as to their effectiveness, in part because they lack a clear final end point against which effectiveness can be judged.
- Other problems in the organization are connected to solutions for a focal problem.
- High levels of ambiguity and uncertainty are associated with solutions.
- Realizing hoped for benefits has considerable risk.
- Strategic decisions have competing interests that prompt key players to use political pressure to ensure that a choice aligns with their preferences.



Strategic decision making is often treated as an instantaneous choice between two or more known alternatives. However, this 'point of decision' approach is unable to capture the richness and complexity of the processes that unfolded to the point of decision including how problems were uncovered, the way in which search was conducted, what was done to ensure decision adoption and the steps taken to assess benefits. Decision making from a point of decision perspective also assumes that managers have complete control over decisions. It is more likely that the decision maker has limited discretion in selecting among courses of action. This occurs, for example, when strategic decisions are constrained by interventionist government policies, such as privatization or deregulation, requiring all strategic actions to be framed and shaped by this wider context. Nevertheless, managers still have some degree of strategic choice even if the wider context (e.g. privatization) is firmly set in place. This includes strategic decisions involving topics such as organizational design, choice of suppliers, choice and sophistication of information systems and general product or service portfolios.

Theorists such as Drucker (1974) and Weick (1995) show how decision-making processes in organizations are as much about defining the question as they are about providing an answer. To understand a strategic decision one must decide whether there is a need for a decision and, if so, what that decision is about. Weick likens this process to those of boards of inquiry following a disastrous event. Such

a board has a number of roles. The board acts like a historian – reconstructing the past to make sense of what happened and to prevent future disasters happening again should similar events occur. The 'historian' takes an outcome and interprets it as the result of a series of decisions, which are seldom seen by those involved as discrete choices made to resolve a problematic situation. Much of strategic decision-making research requires this kind of social reconstruction.

There are many other views of strategic decision making. Mintzberg (1987) provided a useful way to categorize decisions with his five Ps classification. We summarize it here because it raises some key questions about the nature and definition of a strategic decision. Strategic decisions can be viewed as a *Plan*: the decision is an intended course of action carried out in advance with a clear purpose. Alternatively, strategic decisions can be seen as a *Ploy*. Here, decisions take shape as a set of actions designed to outwit the competition, which may not be the 'obvious' content of the decision. For example, a decision to build a new building in order to expand may not be the overt strategy, but is more concerned with increasing barriers to entry for potential competitors. There are connections here with the military roots of strategic decision making. The plans of campaigns may have similar characteristics to those of a ploy to outwit the 'enemy'. Thirdly, strategic decisions can be seen as a *Pattern*: decisions are not necessarily taken with a clear planned purpose and decision makers do not always have access to the range of knowledge required to create a plan of action. However, decisions taken over time form a pattern. It is this pattern of resulting (emergent) behaviour that we call the strategy of the firm. Strategy is therefore characterized as a pattern that emerges from a stream of decisions and may not be an attribute or descriptor of a single decision.

Strategic decision making can be seen as achieving a *Position*. A decision is less about the dynamics of planning or gamesmanship and more about trying to realize a match between the organization and its environment. This position can be one of alignment, so that the organization matches its environment, such as designing highly decentralized structures to cope with a turbulent and unpredictable environment, or one of trying to secure competitive advantage, where the organization solidifies a unique position in the market.

Finally, strategic decision making can be viewed as a *Perspective*. Here decisions are characterized as a reflection of how strategists in an organization perceive the world and their organization. To illustrate, the strategic perspective of Nokia is one of continuous and sometimes radical change (Nokia began as a paper and pulp company); IBM favours a dominant marketing perspective; and Hewlett-Packard favours an engineering excellence perspective. Such a perspective, if pervasive enough, can influence the kinds of decisions taken, in respect of their content and their processes. We can see the effects of this embedded view of decision making by observing that organizations in similar industries often choose similar strategic decisions and become second movers. From this perspective universities tend to follow broadly similar strategies, as do large retailers and service organizations.

These decision types divide into strategic and organizational. Organizational decisions tend to result in plans or ploys. However important or costly such individual decisions may be, the *strategic* element of them is apparent only when a number of decisions are examined together and the patterns and themes in them are

uncovered. Using the term strategic for individual (or single) decisions that are plans or ploys seems poor practice. Strategic decisions are more apt to be a pattern, a position, or a perspective. Interestingly, researchers often examine plans and ploys using a process perspective whereas patterns, positions, and perspective receive very little attention by researchers to uncover their generative nature. We return to this debate surrounding the application of the term strategic to decision making later in this chapter.

Over the last 50 years, there have been radical changes in how strategic decision making is researched. For example, in the 1950s and 1960s research emphasized a planning approach to decision making. Such tools included industry structure analyses and portfolio matrices, for example, the matrices offered by Ansoff and the Boston Consulting Group. In this era, strategic decision making was mostly about planning. The 1970s onwards saw a different emphasis. Decisions began to emphasize the pay-offs to organizations should different strategic directions (options) be pursued. Typical options were diversification decisions, but this was also the era of innovation (R&D), acquisition, joint venture, and internationalization decisions.

The 1980s saw a move away from examining the content of strategic decisions – what they were about – to examining them more as processes. The question became whether we could map the progress of a strategic decision and make inferences about why such processes might occur. David Hickson and his colleagues (1986) characterized such processes as sporadic (discontinuous), fluid (continuous and smooth), or constricted (restricted to a small group of stakeholders and highly political). This work also underscored the importance of such processes since they underpinned the recognition among managers of the need for strategic change. The 1990s onwards have seen a continuing interest in unfolding the characteristics of decision processes, but the emphasis has changed to focus on whether or not there are any links between decision making and results. For example, did the decision succeed or fail (e.g. Nutt, 1999, 2002; Hickson *et al*., 2003)? Do a number of failed strategic decisions lead to organizational failure as Landis Gabel and Sinclair-Desagne (2008), for example, suggest?



Finally, some recent approaches to strategic decision making have concentrated upon the more micro aspects of how managers think, act, and interpret strategic decisions. Such an approach has been termed the *strategy as practice* perspective (Whittington, 1996) or as activity-based (Heracleous, 2003; Jarzabkowski, 2005). Here, the thrust is to dig into what managers actually 'do' when they 'strategize', a term that seems to have emerged alongside the emerging popularity of this perspective. As Jarzabkowski and Wilson (2006) note, much of 'traditional' strategic decision-making theory has been criticized because it is not actionable in practice, so researchers should concentrate on what managers do when they engage in strategic activities. However, it is by no means easy to tell when strategic activity is taking place and when it is not; nor is it easy to identify an appropriate level (or levels) of analysis to examine such activities. For example, should we examine the cognitive and psychological aspects of individuals when they engage in strategic activities, or should we look at their physical activities and try to describe the processes in which they engage (such as decision making), or all

three? There are no concrete answers to the above questions. All would be legitimate ways of drilling deep into what managers do when they engage in decision making.

Jarzabkowski (2005) provides a useful perspective to the practice approach by concentrating on what she terms an 'activity-based' view. By this, she means that managers themselves define what is, and is not, strategic by their actions. These discussions and decisions constitute an important part of understanding decision making. One of the key contributions made by Jarzabkowski's (2005) study is that decision making is a 'situated' activity. Although there are many arguments about what is meant by the term 'situated', it identifies the relational nature of managers as actors with situations being the contexts in which they operate. Any particular action by managers must be seen and understood in the context of the situation in which that action occurs. Managers are both recipients and creators of the situational context in which they carry out the activities that go into decision making.

Why is such a micro focus useful? The main answer is that the strategy as practice perspective highlights differences in strategic decision making that might otherwise be missed. From a more macro perspective, organizations can look fundamentally similar. They face similar social, political, and economic contexts in which they are embedded. However, this similarity can be deceptive. Jarzabkowski (2005) shows how three Universities, all facing the same often mutually contradictory tensions of increasing revenue from research and from commercial activities, craft and implement very different decisions to try and resolve these tensions and increase revenue streams. Only a micro focus can reveal these key differences between organizations in the ways in which their managers handle decision making.

The practice perspective shows how face-to-face interactions between managers are imbued with the context of administrative and organizational procedures, all of which can influence decision making over time. There is no sharp distinction between decision formulation and implementation from this perspective. Decision making is a blend of individual interactions and the organizational context over time and is not necessarily a step by step or a logical sequence.

Heracleous (2003) also argues for a situated and micro perspective on decision making. He views decision making as a performative art, represented both by what managers do (practice) and what and how they communicate (discourse). Decision making can be best understood by looking at the language managers' use and the activities in which they engage.

As a field of study, strategic decision making has experienced many attacks on its theoretical and empirical foundations. It has not only survived these attacks, but has prospered in recent years. Many established authors are returning to some of the original key ideas in decision making and applying them to other areas of organization theory. Two recent examples provide illustrations. One recent theme can be seen with March's emphasis on the importance of knowledge and what he terms 'organizational intelligence' in decision making (March, 1999). Another can be found in Karl Weick and others who focus on specific decisions needed to prepare organizations for extreme or highly uncertain events, such as a disaster or a terrorist attack (Weick and Sutcliffe, 2007; Starbuck and Farjoun, 2005; Sullivan-Taylor and Wilson, 2009).

Following the organizational intelligence theme, March examines the characteristics of decisions that allow decision makers to follow courses of action to ensure that the organization continues to benefit in the face of scarce resources and heightened competition. March categorizes these as intelligent decisions that merge desires, actions, and outcomes in a positive way (i.e. outcomes fulfil desires as far as possible). March shows how intelligence (information, experience, and aspirations) can lead to poor decision making. Accelerating errors found in decisions initially thought to be clever can lead to poor performance. The trick according to March is to assemble intelligence in decision making in ways that facilitate successful performance.

Following an 'extreme event' theme, authors such as Weick and Sutcliffe focus on decision making in which levels of uncertainty and ambiguity are very high. They suggest that decision making needs to create and sustain what they term 'high reliability organizations' that are not only capable of withstanding extreme events better than other organizations, but are also highly resilient – they can recover quickly after disasters strike.

In the above two broad themes we can see how relatively modern concerns of organization, competing on knowledge and being prepared for extremes, are being addressed by reference to decision-making theories. Such extensions of decision-making research to other aspects of organization theory have a considerable history. For example, the notion of *incrementalism* or the piecemeal attention to small steps in any process came from Lindblom's (1959) research into how decisions are made. The notion of *problemistic search* (managers seek only information when they have to or when there is a pressing problem) came out of work by Cyert and March (1963). The concept of *enacted environments* (managers only see and interpret the bit of the operating environment that they focus upon) came out of research by Weick (1979). All of these concepts were developed within the field of strategic decision making and to become more generically applied to organizational processes. Strategic decision making has proved a rich ground for the emergence of such concepts.



The work of James G. March identifies many of the key features and debates in strategic decision making. His approach can be illustrated in Figure 1.1. The major contribution of this simple flow diagram is twofold. The processes it identifies underpin most key organizational processes, revealing the centrality of decision making in organization theory generally. Secondly, its very simplicity can be misleading. The cycle shown in the figure can be broken or can malfunction at each stage of the process and between stages. March taught us to beware assumptions of rationality both in individuals and in organizations. Actions can be taken for a variety of reasons that correspond to the ways in which organizations are structured (each specialized function developing its own view on what should happen). This entered the vocabulary of organizational decision making in the form of 'local rationality' (Cyert and March, 1963).

March (1994) was later to refine this concept by emphasizing local preferences, rather than rationality. He argued that the main thing in organizational decision making was forming *interpretations,* not making choices. Interpretations cover a wide

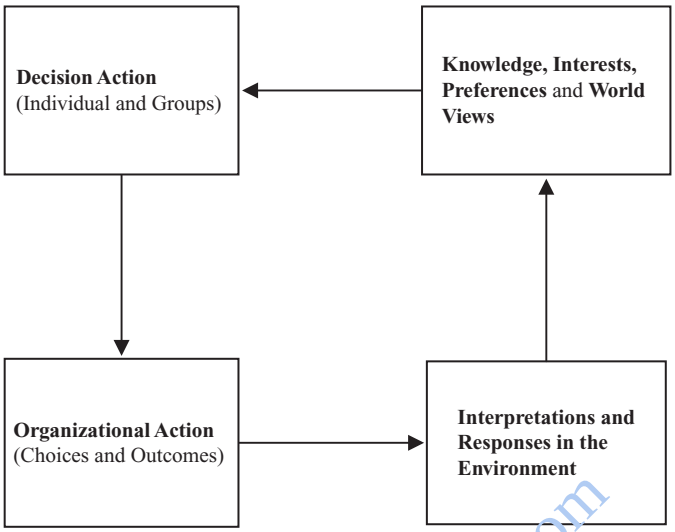


FIGURE 1.1 Strategic decision-making processes.
Adapted from March (1994)



arena when examining organizational decision making. In particular, March set out to show the differences between decisions that are choice-based and those that are rule-based. The main distinction was whether decision makers pursue logic when making choices among alternatives and evaluating their consequences in terms of prior preferences, or whether they pursue a logic of appropriateness, fulfilling identities or roles by recognizing situations and following rules, which match appropriate behaviours to the situations they encounter. In this respect, organizations provide the context in which such interpretations are formed and sustained, and sometimes changed.

March (1994) also pointed out that organizations could engender two very different types of decision behaviour. One can be characterized by clarity and consistency and the other by ambiguity, inconsistency, and chaos. In the former, organization is all about coherence and reducing uncertainty to avoid equivocality. In the latter, organization is anarchic and acts as a background for decisions that may not be linear in process and may not be logical in a consistent sense and where solutions may precede outcomes. Here organizations, by their very nature, are collections of solutions already made – waiting for new decision opportunities to apply each solution.

Finally, March (1994) argued that decision outcomes can be seen as primarily attributable to the actions of autonomous actors in organizations, or can be the result of the systemic properties of organizations as an interacting ecology. This makes the links between organization and decision explicit. Is it possible to describe decisions as emanating from the intentions, identities, and interests of independent actors? Alternatively, is it necessary to emphasize the ways in which individual actors, organizations, and societies fit together?

There is no doubt that decision making is both complex to study and replete with conceptual and empirical dilemmas. We explore these in the next sections of this chapter.

## Issues Confronting Decision-making Research

Decision-making research has offered many insightful studies that have illuminated an interesting and complex field. A vast number of empirical investigations, descriptions, prescriptions, structuring techniques, as well as mathematical models that offer analytical tools have been produced. Despite all these notable efforts, few have made headway in integrating this body of knowledge into coherent theory. We will offer our view of some of the reasons for this state of affairs, identifying key issues that must be addressed to construct such a theory. In brief, these issues are unit and level of analysis, purpose, dilemmas in conceptualizing decision making, and the influence of contingency, frameworks, and methodology.



### *Unit of analysis*

Decision-making researchers selected both decisions and choice opportunities (sorting alternatives) as their unit of analysis (Mintzberg *et al.,* 1990, Bell *et al.*, 1998). In our view, the decision is the preferred unit of analysis. Such a focus considers the full range of issues that can arise during decision making. Choice considers only the comparison of alternatives, an overly narrow interpretation of what is required to make a decision. The level of analysis is another crucial consideration, which poses considerations that differ from those posed by the unit of analysis. In past efforts, the unit of analysis has often been confounded with level of analysis. Confounding arises in several ways. Some studies have attempted to deal with decisions that span a number of managerial levels (Bell *et al.*, 1998) or consider the choices or the decisions made by CEOs, top management teams, middle managers, and department heads (Nutt, 2001c). Choices or decisions must be separated by level or type of decision maker in such studies. Confounding also results when there is a mixing of several related decisions or choices to capture an organizational project, such as in disaster management (Weick, 2001) or a large-scale initiative (Cameron and Lavine, 2006). Decisions (or choices) are confounded with organizations when multiple cases are drawn from several organizations, without accounting for the nesting of decisions within each organization. Although Hickson *et al.*, (1986) found these nesting effects to be minimal, they should not be overlooked in future research efforts.

To deal with confounding, factors that denote who is involved, the type of decision maker (e.g. CEOs), the link of decisions to major projects, and the organization in which each decision (or choice) takes place must be identified for analysis. We contend that researchers must carefully make these distinctions and include factors for each in their research. Action theory depends upon generalizing about decisions as well as comparing prescriptions across organizational levels, etc.

## *Purpose*

Purpose poses a formidable challenge to integration. Investigators have examined decision making with many purposes in mind ranging from developing decision-making techniques, to prescription, to descriptions of what decision makers do. This has led to a vast outpouring of projects that consider facets of decision making, with only a few addressing the entire decision episode. Attempts to integrate have been frustrated by framing dilemmas, the difficulties of amalgamating description with prescription, and arguably misguided attempts to deal with process.

*Framing dilemmas.* A variety of frames can be found in decision-making research. Eisenhardt and Zbaracki (1992) contend that bounded rationality, power and politics, and chance provide the more useful frames. Bounded rationality draws on Dewey's (1910) notions of logical inquiry in which he calls inquiry a process. Qualifications and elaborations have followed, exemplified by the work of March and Simon (1958). Still further extensions made decision rules explicit, modified process steps, and incorporated uncertainty (e.g. Thompson, 1967; Perrow, 1976; Allison, 1971). Research in this tradition has found process steps to unfold in a variety of ways that are subject to cycling and interrupts (Mintzberg *et al*., 1976). Others define what are believed to be essential steps, such as intelligence gathering, direction setting, option search, option selection, and implementation (e.g., Nutt, 1989; Daft, 1995; Hickson *et al*., 2003; Miller *et al*., 2004). Behavioural research finds that decision makers often ignore recommendations derived from these findings (Nutt, 1984; 2002) and simplify their decision-making processes when faced with conflict or novel situations (e.g. MacCrimmon and Taylor, 1976; Janis, 1989).

Power/politics and chance have been suggested to overcome the limitations found in framing with bounded rationality. Both chance and power/politics are thought to have face validity. The exercise of power and the emergence of happenstance fit one's everyday experiences with decision making. Those advocating a power or a politics frame contend that whereas individuals can be rational, a collective, made up of those same people, is not (Pfeffer, 1992), and that the collective must be managed should conflict arise (Langley, 1995). Emphasis is placed on resolving differences using tactics such as coalitions, cooptation, information control, and influence (Pettigrew, 1973). Studies find that managers also turn to politics when thwarted; and when there is a power vacuum. The value of turning to politics can be questioned. Empirical studies find that politics prompts animosity, which slows down decisions and leads to poor results (Eisenhardt and Bourgeois, 1989). Dean and Sharfman (1996) find the pervasiveness of politics to be exaggerated.

The chance frame treats decision making as the accidental connection of a choice opportunity (the call for a decision) with a fortuitous solution. In what has become know as the 'garbage can' (Cohen *et al*., 1972) choice situations, ideas that zealots and others believe to be useful, concerns, and people looking for action meet due to chance. To be adopted, a solution must be conspicuous and have the support of the right people (Cyert and March, 1963). The chance frame contends that decision makers, distracted by many concurrent demands, connect a solution with a problem to appease stakeholders (Carley, 1986; Masuch and LaPotin, 1989). Timing and luck

are the operant ingredients. However, a chance explanation lacks both prescriptive and explanatory power.

To complicate things further, the frames appear to be self-fulfilling. Look at a decision as a process with unfolding steps and one sees a process with steps. Look for politics or chance and they appear (Harrison and Phillips, 1991). This suggests that each frame offers a particular view and that no single view is best. One way to cope is to merge frames, as suggested by Eisenhardt and Zbaracki (1992). For example, a merger of the more powerful frames, politics and bounded rationality, in the study of decision making seems feasible. This calls for studies that account for both a rational perspective, which uncovers cognitions, and a political perspective, which reveals the social context.

*Prescription/description conflicts.* Some investigators stress description and offer a rich commentary on the events, motivations, and circumstances surrounding a decision. Others concentrate on prescription and offer guidelines for taking action. Both draw upon the other to justify many of the key positions and conclusions provided. Surprisingly, few advocates of either position attempt to measure success. Many of the descriptions and the prescriptions in the literature fail to include empirical investigations that demonstrate effectiveness or generalizability. Even prescriptions that embody mathematical tools (which are not considered in this volume) seldom go beyond a case that illustrates how the tool works to studies that offer empirical evidence, such as comparing one tool to another. As a result, description and prescription have become disconnected, which has kept the conflicting claims made in support of each from being reconciled.

As with many management topics, decision-making research *can be* focused either descriptively or prescriptively. Many contemporary researchers have become strident proponents of one and implicitly, sometimes explicitly, opponents of the other. Contemporary researchers prefer to deal with decision making from a single perspective. In addition, there has been a not so subtle shift in what journals prefer to publish to what is (and is not) acceptable. These trends have led to description dominating research efforts and creating a near mandate for this type of research, which has pushed out prescriptive work. As a result, much of the effort in the prescriptive arena has been shunted to consultants that rarely share their approaches and insights. The descriptive domination of research has also influenced methodology. An explanatory focus has led to a set of methodologies that are far less useful for prescriptive research, which we will discuss in more detail later.

This shift has become troublesome, because description and prescription represent two sides of the 'same coin' (Nutt, 2004a). On the one hand, there is action theory and normative science. On the other, there are behavioural/explanatory explanations of what the researcher observes. Prescription calls for the researcher to identify frameworks, tactics, and the like to test them to see if they produce something of value in real world applications. Description deals with use. How many people act in a certain way, how many subordinates get involved, what is the skill level of key players? One informs the other. Theory that denies or invalidates one or the other is incomplete. Linking the actions taken to success provides a key piece of the action-theory puzzle. Noting whether a prescription is followed, and how, also

informs practice. One of the intents of this volume is to call for a more balanced approach to decision-making research. To provide this balance, we will discuss some of the benefits and the pitfalls that follow the research focus found in both approaches.

*Misrepresenting process.* Management research was founded with case studies. Before empirical studies were initiated by Ford Foundation funding, the case study offered the primary insight from which people in the field wrote books and taught their classes. Cases, often with little or no generalizability, are still dominant in fields such as strategic management. Decision-making research also has its roots in the case study approach. Its origins can be traced to the Cyert and March (1963) seminal case study of decision making in organizations. Many such studies followed (e.g. Bower, 1970). The description offered by a well-constructed case provided powerful imagery that indicated what was done and sought to uncover why. There have been many useful findings from this kind of work. Perhaps the most important was the deep understanding that a rich case provides, with its focus on the specifics of what happened and its attempts to tease out why this happened. Cases have been particularly useful in reaching out to practitioners, where there is a decided preference for explicit discussion of application. Unfortunately, this has led to discounting action science (also called action theory), like that found in medicine and engineering, which has produced many breakthroughs in both arenas.

Action theory offers an if-then approach to taking action in which an approach is crafted to deal with issues of interest to managers, much like the book of signs and symptoms used by Internists that connect signs and symptoms with possible therapies. Action theory calls for a shift in emphasis from the *is* to the *ought,* which is context dependent. An 'is emphasis' captures what was done and ignores possibilities. An 'ought' approach identifies what can be used to improve the results of action taking and offers tests of what works and what benefits can be expected as a contingency. Prescription offers tools, techniques, and procedures as well as best practices by expert practitioners, fitting each to a process to deal with issues that arise during decision making.

Action theory could have been incorporated into past efforts, but case study researchers invariably ignore *how* the decision was crafted – the steps taken to produce it. Thus, the core for constructing an action theory is missing from the case. This skips over what organizational theorists call process, which has led to process being neglected in most of the research being reported in management journals. This is due in part to the descriptive tradition, noted above with its implicit focus on what is, and its ostensible preference to ignore how things got this way. We will discuss the needs to consider process and its role in formulating an action theory for decision making throughout the book.

This challenge is daunting. Such processes often take place over time, posing problems of observation and codification. The process itself has been seen by some to be somewhat structured and by others as chaotic. Many researchers have also changed their conceptual position over time. For instance, Mintzberg has shifted his view of process from structured (Mintzberg *et al.*, 1976) to messy (Mintzberg and Westley, 2001). Initially, he appeared to argue that decision making was more like planned activity with periods of formulation followed by periods of

implementation, with some internal cycling to deal with each. His more recent position seems to argue that decision-making processes are a messy mixture of formulation and implementation that do not necessarily precede one another in temporal sequence (as case studies portray more often than not). Instead, they should be conceptualized as inextricably interlinked. It makes no sense, under this view, to try to separate them out as distinct 'phases' of a process. Still another view is that decision making is rarely planned and mostly emergent, with people reconstructing a story of what happened afterwards. This means that decision making is more about making sense of what has happened (such as reconstructing a pattern in the process of decision making) and less about planning in advance (e.g. Weick, 1979; 1995). Both the chaotic and the sensemaking view pose many conceptual problems to codify and understand process.

## *Conceptualizing decision making*

The framing noted above gives decision-making research efforts their direction. The frame points a researcher down a particular path and suggests how key factors are to be imaged. This has led to many very different conceptualizations. When different frames are used, it complicates attempts to integrate the action taking undertaken by a decision maker (what decision makers do). First, the frame imposes a perspective that influences how the procedures followed by a decision maker are conceptualized. Different actions would be sought (and then measured) if the investigator sets out to uncover steps suggested by bounded rationality, observe how a decision maker reacts to chance events, or follow a negotiation. In each case, the frame suggests a conceptualization that dictates what kind of action-taking steps will be recognized. In addition, approaching a decision-making study as a description leads the researcher away from codifying procedure and toward describing the action taken. Finally, investigations seldom look for a frame that allows both emergent and chaotic features of a process to emerge.

Second, researchers have approached the conceptualization of action very differently. Some draw on philosophy of science (e.g. Dewey, 1910) to gain insight into how decisions should be made. This has led to prescribing procedures (e.g. Simon, 1977; Perrow, 1967; Thompson, 1967; Nutt, 1989; and Daft, 1995). There have been many of these efforts, which has prompted some to seek hybrid processes that integrate procedural elements, seeking an underlying process, and others to suggest processes for particular applications, such as decision making (e.g. Havelock, 1973; Nutt, 2004a). Another kind of effort has investigated what decision makers do, looking for underlying logic (e.g. Witte, 1970; Soelberg, 1967; Mintzberg *et al.*, 1976). Such studies have examined decision-maker action by means of on-site observations, interviews, and surveys to uncover the procedures that are used in practice (e.g. Nutt, 1984; Fredrickson, 1985; Hickson *et al.*, 1986, 2001, 2003; Dean and Sharfman, 1996; Miller *et al.*, 2004).

In many of these efforts, the aim is to document 'process'. Such studies attempt to identify the steps followed to make a decision (Bell *et al.*, 1998). Other researchers go further, looking for steps that seem essential (Nutt, 1984). Related research combines prescriptive and behavioural perspectives to uncover what decision makers do

and how this deviates from recommendations (Nutt, 1999). Finally, some add cognition and measure process features (Rajagopalan *et al*., 1998; Nutt, 2002). This asserts that cognition determines the kind of process selected. All this has led investigators to conceptualize process very differently. As a result, research efforts seldom specify action elements in a way that allows for integration. These research efforts have identified some features of a process, or its motivation, but not how the decision was made. For example, Dean and Sharfman (1993) classify a process by procedural features such as rationality (systematic collection and interpretation of information), political behaviour (using power), and flexibility (adaptability). Hickson *et al*. (1986) use process descriptors such as sporadic (with delays and negotiation), fluid (formalized process), or constricted (restricted to a very small number of senior executives). Fredrickson (1985) classified process by its comprehensiveness. Bell *et al*. (1998) identifies rational, comprehensive, political action, and subunit involvement processes. Others treat process as coalition formation or social process control and focus on measuring decision-maker attributes such as tolerance for ambiguity, uncertainty, or risk aversion (Poole and Van de Ven, 2004). Although interesting, such research says little about how decisions are and should be made. Classifications, such as comprehensive, analytical, or political, fail to explain how a decision maker acts comprehensively, conducts analyses, or engages politically. They characterize the process not the actions that take place within it.

We call for studies that treat decision-maker action taking as a process with several steps that embrace intelligence gathering and implementation, in addition to choice, and allow for emergent ideas and messy recycling among key ideas such as formulation and search.



### *Contingency theory*

Contingency approaches have dominated management thinking for decades. According to this view, a research hypothesis must include situational factors that can influence a main effect under study, such as action taking steps. Justifications stem from assertions by methodologists, who call for studies to account for plausible outside influences (Hitt *et al*., 2009) and by the many contingency models found in organizational behaviour textbooks in the past two decades. This gives the appearance that contingency models have considerable empirical support. Surprisingly, few contingency models have been subjected to empirical testing and many journals resist publishing this kind of research, arguing that the models are old or that widespread use provides sufficient justification. Nevertheless, a comprehensive research effort will have considerable difficulty being published without including a number of widely accepted contextual factors, such as importance and urgency.

In all such models, contingencies lay out boundary conditions that identify *when* a particular kind of action is preferred. The boundary conditions are often suggested situational factors. Environmental stability, time pressure, novelty, complexity, and resource dependency may identify when a given decision approach (a set of action taking steps) works best. Researchers empirically test such assertions by including both the action taking steps and the contingency as factors in a study that assesses each independently, and as a statistical interaction (Nutt, 2008). This allows the

researcher to determine if one kind of action taking works best under a particular set of conditions.

Two kinds of contingencies are recommended for such studies: content and context.

*Content.* Content identifies the decision type. A variety of decision types has been studied. Some focus on the crucial but infrequent decisions made by top managers that select core businesses offering competitive advantage (e.g. Hitt *et al*., 1997). This limits the purview to core business choices made by top management teams. As noted above, Mintzberg *et al*. (1976) in their seminal work call strategic decisions important choices that have long-term consequences due to the resources required and precedents set. The Mintzberg position takes a much more inclusive view that sweeps in a greater variety of somewhat smaller scale decisions, which have both top and middle manager involvement. This opens the door to a wide scope of decisions. The Bradford studies (Hickson *et al*., 1986, and Nutt 2002) have adopted this view, as have many others. All this suggests that researchers claim to study 'strategic decisions', but define strategic quite differently. Some focus on the decisions made to select a core business, which may involve patterns, position, or perspectives. The study of such is a sadly neglected topic. As a result, we know little about strategic decisions defined in this way. More work in this area is needed.

Calling a decision 'strategic' to suggest importance and significance has become poor practice. We call for strategic decisions to be accounted for and characterized much more precisely in future work. This can be done in at least two ways. One approach is to classify decision by the degree to which it has strategic implications. There is no doubt that different decisions in organizations will vary in terms of their importance to the organization, the degrees of risk and novelty involved, and the amount of resources that need to be committed. The Bradford studies revealed that organizations have between five and seven such decisions being processed at any one time. Such decisions can be ranked in terms of their 'strategicness'. Many of the less-strategic decisions in organizations are not strategic at all – they are merely costly, for example, and have no connection to a strategic action in the sense understood by writers in the field of strategic management. To illustrate, decisions made about positioning, a core business selection, or about securing competitive advantage are called 'strategic' in strategic management. Each is essentially a one-off decision. Organizational level decisions that follow, however resource hungry or risky, would be viewed using this perspective as operational. Effectively, they capture some of the many actions that put a strategy into practice. What is defined as strategic depends upon your frame of reference.

An alternative approach is to account for the distinction between the strategic and organizational decisions in empirical analyses. Strategic decisions would be classified as those that deal with a new line of business. Organizational decisions would be defined as strategic decisions that have been defined: important, precedent-setting, high-resource, and large-commitment decisions. Furthermore, an organizational decision can be either subjective or objective. Subjective choices involve agenda setting, selecting topics for future decisions (Bell *et al*.,1998), and ethical considerations, value positions to be taken when making a decision (Nutt,

2002). Hickson *et al.* (1986) looked at decisions 'objectively', stressing action, and identified several types: products/services, financing, personnel policy, marketing, buildings, technologies, and reorganizations. Nutt (2001b) found internal operations/control systems to be a separate type.

Content is believed to influence the choices made, the benefits realized, and the processes applied (Butler, 1998). Clarity about the kind of decisions being addressed is essential. In addition, decision scope, as indicated by the level of the managers involved, can be confounded with type and should be included as a study factor. Top executives are more apt to be involved with strategic matters and others with control systems, inputs, etc. Thus, we call for studies that consider the decision type differentiating the strategic from the organizational as well as maintaining the subjective and objective distinction and specifying the span of the study (the number of decision types included). Together, conclusions about such factors should provide a test of the generalizability of the study findings.

*Context.* Context identifies the environment in which a decision is made, providing a second set of boundary conditions. Both internal and external environmental factors are believed to influence what is decided as well as how a decision is made (e.g. Thompson, 1967; Perrow, 1967; Bell *et al.*, 1998). Internal factors include surprise, confusion, and threat (March and Simon, 1958), organizational features, such as approaches to communication and control and resistance to change (e.g. Nutt, 2002), as well as decision importance (Bell *et al.*, 1998), complexity (Nutt, 1998), and uncertainty (Thompson, 1967). Decision-maker attributes such as the propensity to take risks, tolerance for ambiguity, creativity, decision style, intelligence, need for control, power, experience, education, and values have been suggested (Bell *et al.*, 1998). External factors include organizational differences, such as public or private (Hickson *et al.*, 1986; Nutt, 2004b), as well as prevailing economic conditions (Bell *et al.*, 1998).

Context, like content, is believed to influence choices, benefits realized, and processes applied (Nutt, 1998a; Bell *et al.*, 1998). Thus, clarity about the situation in which a decision was rendered is required as well. As with content, certain kinds of managers may be involved when certain conditions arise. Top executives are more apt to be involved with high stakes efforts or costly ones. We call for studies that consider the managerial level, identifying both level and contextual factors, since these are needed to deal with the influence of situation (or context). A test of this contingency argument determines whether context influences how a choice is made, and its outcome. Such studies also provide greater generalizability to finding out about process and other matters of concern to decision-making research.



## *Decision outcomes*

Identifying decision outcomes has been a particularly difficult challenge for decision researchers. Decision outcomes are frequently multifaceted and often difficult to fully grasp and quantitatively measure. For example, a resort may measure occupancy, sales by cost center, and commissions. It is often difficult to codify the effects of outstanding customer service or to create a metric that translates increases in

occupancy, due to discounts and other factors that influence room revenues, to profits.

Relevant measures identify decision benefits and whether the benefits can be justified given the cost, disruptions, and distractions required by a decision. Determining benefits requires documenting outcomes and measuring their effects (Hickson *et al*., 1986; Nutt, 1986; Bell *et al*., 1998; Papadakis and Barwise, 1998; Hickson *et al*., 2003; Miller *et al*., 2004). These effects can take many forms. Bower (1970) argues that training is important. Others call for determining changes in people's behaviour and interpretations (Bryson *et al*., 1990), measuring process outcomes (timeliness, commitment, and learning), documenting features of action taking, such as disruption and scope of negotiations (Hickson *et al*., 1986), or developing indicators of success (Nutt, 2002; Hickson *et al*., 2003). The Bradford Studies and Nutt (1998c) specifically examined the relationships between implementation and outcomes. Using the extent to which stated objectives had been achieved (as a surrogate for performance), these studies revealed the importance of not only the structural and cultural aspects of organization, (such as, do structures impede or facilitate outcomes?), but also the knowledge base of the organization (can managers accurately specify and assess both information and resources?). These studies also indicated that outcomes were directly associated with the above factors and were also influenced by intervening variables, namely how acceptable a decision is to key stakeholders and to what extent the decision was made a priority in the organization. These measures are surely only the tip of the iceberg in terms of identifying factors that influence outcomes. Researchers must push their measures further toward documenting actual benefits.



### *Relationships explored*

Astute investigators call for studies that examine decision making within an organization in which managers, facing an important concern or difficulty, take action to make choices that produce outcomes with immediate and downstream effects. How a decision maker takes action appears to influence the choices made and their benefits (Nutt, 1984; Dean and Sharfman, 1996; Hickson *et al*., 2003). Context and content are also believed to influence both the choices made and their benefits (Hickson *et al*., 1986; Nutt, 1998a; Bell *et al*., 1998).

To do this research, a relationship must be posited between process (action taking steps), context (importance, urgency, etc.), content (e.g. strategic and nonstrategic; the eight Hickson types), and the costs and benefits of a decision. Several relationships have been suggested in which process is causal, mediating, or an outcome. For example, Butler (1998) identifies relationships among what he calls problem (content), solution (outcome), and choice (process) in which each can be a cause, an effect, or an interaction; linking them to computation, expertise, negotiation, and inspirational kinds of decisions (context is not considered). In expertise decisions, outcomes dictate content and process with process and content interacting. Negotiation calls for process to be causal with content and outcome interacting. Bell *et al*. (1998) posits a relationship in which context is causal, first influencing process and content, and then outcomes. Downstream effects are

acknowledged, contending that a choice influences the host of tangential interpretations (Bryson *et al*., 1990) and that benefits can be delayed (Nutt, 2002). Rajagopalan *et al*. (1998) contend that context (made up of environmental and organizational factors) and content jointly influence decision-maker cognitions and the process that is embraced, with the outcome stemming from process as well as being influenced by context and content. Drawing on such relationships, researchers speculate about how outcomes are influenced by process, the situation, the type of decision, or by combinations of these factors.

### *Methodology*

How data are collected in decision-making studies is important as well. The conclusions drawn from decision-making studies can be incompatible due to the wide variety of methods that are applied. Research approaches in such studies have varied from qualitative to quantitative, simulation to case study, interviews to surveys. The result has created something of a hodge-podge of investigations with little insight into amalgamating the findings to help build a coherent theory. In this way, decision making is similar to many areas of social science investigation with different paradigms, different mother disciplines, different data collection methods, and analytical coding schemes being the norm rather than the exception, which has created two major difficulties for the field of decision making as well as social science. The first concerns rigour and the second, relevance.

Rigour measures the quality of the research effort, asking how good the research is. This is often presumed synonymous with how 'scientific' the research is. A great deal of decision-making research is qualitative. Qualitative research is often criticized as lacking rigour. Once termed 'unscientific', the findings from such studies become suspect because, at least in many Western countries, the highest form of knowing has demonstrated scientific rigour. Decision-making researchers, who use qualitative research methods, tend to reject formal quantitative methods because they associate formal modelling with positivism or, worse still, with over-quantifying. An emphasis on quantification coaxes the researcher to exclude things that are difficult to measure. Often these exclusions matter – factors difficult to measure can have an overriding importance. Nevertheless, qualitative research is subject to interpretive biases that reduce objectivity, lessening rigor. In addition, qualitative efforts often produce a limited number of cases. This can lead to limited generalizability of the findings. Researchers may see what they want to see in the analysis, and little else. Critics contend that qualitative research is often little more than an assembly of anecdotal and personal impressions, both subject to observer biases. This can make qualitative research nonreproducible. It is unlikely another researcher would come to a similar set of conclusions.

However, qualitative and quantitative research approaches have many striking similarities. Claims that they are fundamentally different have been successfully challenged. For example, May and Pope (1995) and Dingwall (1992) argue that looking for the distinctions in qualitative and quantitative approaches create methodological fallacies. They believe that research is about a state of mind and the particular conditions that allow such an attitude to be expressed. As May and

Pope (1995) argue, quantitative analysis allows the generation of numerical representations of research that give the impression of solidity and a factual base, but in essence they are highly dependent on the skills and judgement of the researcher (as well as to what extent the measures used were appropriate to the issues studied). This chimes with the view that qualitative research is also dependent upon skill, judgement, and the interpretive frame of reference of the researcher or researchers.

Britten and Fisher (1993) argue that quantitative methods are more apt to be seen as reliable but not valid and that qualitative methods are viewed as valid but not reliable. Such positions are often expressed when assessing decision-making research. Those who have advocated mixing quantitative and qualitative methods (Nutt, 1999, 2008; Hickson *et al.*, 1986, 2003; Jick, 1979; Brannen, 1992) must justify sacrificing reliability to improve validity. Critics implicitly favour one or the other. Researchers are forced to collect additional data to comply instead of dealing with the many difficulties inherent in combining the two approaches.

Finally, it should be noted that there are cultural differences in what is considered rigorous science, particularly between Europe and the United States. An in–depth case study or series of comparative case studies can be hailed as 'insightful, rigorously researched, and sophisticated' by European scholars, while the same piece of work can be dismissed as 'sloppy science' by scholars from the United States. This can be seen in the anonymous statements from European and American reviewers, offered in a peer review of the same paper.

In our view, decision-making research must balance rigor and relevance. Decision making is arguably *the* key activity of a senior manager and poor decisions can lead to the demise of an organization. Relevance stems from confronting the phenomena of interest, a decision, and not some artificial simulation with naïve participants. Dealing with a decision and not an abstraction makes it more likely that the research finding will be useful in practice.

Like much of Social Science, decision-making research suffers from questionable relevance (Jarzabkowski and Wilson, 2006). The lack of relevance arises in several ways. First, conditions change. Today managers face globalization, the advent of new technologies, as well as deregulation and re-regulation, recession, and competition in what has become a knowledge-based economy. Decision-making theory and empirical research say little about these factors. When much of the extant decision-making research was carried out, different technologies were foremost in people's minds. Data that were collected from noncomparable sectors of the economy under different conditions can not be generalized to current conditions. Furthermore, what constitutes organizational performance in a global knowledge driven economy must be rethought.

Still another complication arises from managers who make decisions by drawing decision-making research findings without considering the theories underpinning the research (see Baldridge *et al.*, 2004; Jarzabkowski and Wilson, 2006). This creates a fundamental problem for the relevance of much research. The creation and design of a study, from the researcher's perspective, also constructs the interpretation and implications of the data. If a practitioner just applies the findings as if they

were objective and generalizable, it disconnects context from findings making the applicability of the findings questionable and possibly irrelevant.

## Our Approach

The Wiley Handbook series has gained considerable recognition as a primer for research in management-related fields. When we were contacted to write a Handbook for decision making, it was agreed that we would follow this well-established tradition. To do so we first ruminated about what was out there and what was needed, much the way one would put together a seminar series. After some reflection, we selected some topics and set out to recruit people in the field who, in our judgement, could make novel and cutting-edge contributions in the discussion of each topic as well as addressing the issues that we identify in the previous section. Our authors were invited to confront these topics but to do so by expressing their own ideas. We made no effort to edit what was submitted for content, only offering suggestions regarding extensions and elaborations to the arguments made that seemed needed to clarify and justify. In this way, each author was offered the freedom to approach his or her topic, as each believed was appropriate, including taking positions that we may not support. We leave it up to the reader to sort this out as differing framing, approaches, and world-views are inherent in decision making, as in any field of study.

We are pleased with the result. Our authors represent a cross-section of the scholars interested in decision making on both sides of the Atlantic. We believe that each has provided insight into the issues that we raised They include many who have written important summative or integrative discussions of decision making as well as those who have been leaders in reporting a stream of original research. We believe our contributors offer many original ideas and new directions for decision-making research that will guide research effort for some time. This should make the Handbook a valuable adjunct to faculty and students interested in the study of decision making. We see the Handbook as providing a useful compendium for PhD programmes in business schools around the world with an interest in decision making. We believe that the decision-making researcher will find the ideas for research projects that offer a jumping off point for their inquiries into this most interesting and challenging field of study. It should offer a reference point for new ideas and topics of interest for years to come. In addition, managers will find many of the processes offered in the Handbook to have immediate application. We believe that the forward-looking leaders in public and private organizations will find many ideas in the Handbook on how to improve the practice of decision making in their organizations.

The Handbook is organized by sections that present what we believe are the key issues facing decision makers and researchers. In Part I, we offer two chapters that introduce the volume. We identify trends and issues in decision making in Chapter 1. We discuss some of the dilemmas and conundra that have plagued the field. From these we identify what we believe must be attended to if the field of decision

making is to move forward. Here we also consider the normative and prescriptive approaches taken to decision making that have, regrettably, become mutually exclusive. We show how this has become counterproductive and pose issues that must be resolved to formulate a viable theory of decision making. In Chapter 2, Vassilis Papadakis, Ioannis Thanos, and Patrick Barwise offer a summary of research that has been undertaken in the decade following their taking stock of the field (Papadakis and Barwise, 1998). Previously, Papadakis and Barwise argued for empirical work with more emphasis on outcomes, inclusions of context, conceptualizing decision making broadly to include more of the intellectual effort required, focus on the actions of top managers, and finally including implementation and related topics. Their assessment of the field from these perspectives provides an appraisal of empirical efforts, limitations of these efforts, and suggests some of what remains to be done.

Part II provides some key theoretical perspectives that lie behind much of the past work in decision making. In Chapter 3, Henry Mintzberg and Frances Westley discuss decision-making approaches found in their classic see-first, do-first, think-first typology. These distinctions have become a trilogy that now make up how many writing with a prescriptive intent have framed their action theory of decision making. Interestingly, Mintzberg has come full circle on this, beginning with a process-like structured action theory (Mintzberg *et al*., 1976) followed by a shift to calling decision making idea-driven (Mintzberg and Waters, 1982), and then moving to a more chaotic view (Mintzberg and Westley, 2001). In this piece, the trilogy is fitted to a kind of contingency representation, along with improvising. In Chapter 4, Karl E. Weick, Kathleen M. Sutcliffe, and David Obstfeld offer sensemaking as an explanation for how decision makers act in practice. This recent rendition of Weick's work discusses a theory-like conceptualization of decision making (sensemaking) that has captured the attention of many researchers. As a result, sensemaking has become one of the key foundations of action theory based on improvising. Many embrace such an approach. In subsequent chapters we will identify these and other ways that decision making has been conceptualized, such as chance (e.g. Cohen *et al*., 1972), and present theoretical and empirical arguments that support each. In Chapter 5, John Child, Said Elbanna, and Suzana Rodrigues discuss the political aspects of decision making. This chapter reminds us of the centrality of power and interests and the key parts they play in influencing both the processes and the outcomes of decision making. In Chapter 6, Dennis Gioia and Aimee Hamilton consider organizational identity. These authors highlight the importance of central concepts, such as who we are and what the organization really is. Because these drive both normative assessments of what decisions should be made (or are considered right to be made in a moral sense), they strongly influence what happens in decision-making processes.

Part III offers several of the many unique conceptualizations of decision making that must be appreciated to see how the field has been developed and the task ahead to find a way to integrate it. In Chapter 7, Paul C. Nutt offers the beginnings of a process-based action theory. He summarizes 30 years of empirical work that collected and then analysed decisions made by top managers. Drawing on analyses of more than 400 decisions, he formulates an action theory that identifies the key

steps taken in successful and unsuccessful processes used by top managers. He illustrates traps and how to avoid them, drawing on four cases taken from his database. In Chapter 8 Andre Delbecq, Terri L. Griffith, Tammy L. Madsen, and Jennifer L. Woolley provide an approach that facilitates innovation, showing key steps and illustrating each with cases and demonstrations. In Chapter 9, Colin Eden and Fran Ackerman illustrate how group decision making draws on similar principles and faces similar difficulties as those identified in Chapter 7, by using causal mapping and group decision support.

In Part IV, we take up a series of special topics with importance in studying and understanding decision making. Tim Morris, Royston Greenwood, and Samantha Fairclough in Chapter 10 provide a discussion of strategic decision making in professional firms. Their arguments are that both the structural forms and the strong value bases of individuals within professional service firms provide a context in which both decisions and decision making are distinctly different from other types of organization. Phillip Bromiley and Devaki Rau, in Chapter 11 discuss risk as viewed through the lenses of prospect theory, information processing, and similar approaches favoured by psychologists. They examine treatments of risk by three key decision-making approaches: the behavioural theory of the firm, behavioural decision theory, and agency theory. They conclude with a discussion of the implications of this research for managers, and identify some future areas of study in risk in decision making. In Chapter 12 Kim Boal and Mark Meckler discuss what they call errors of the fourth, fifth, and sixth kind to complement classic type one and type two errors, which arise when interpreting evidence to determine problem causes, and a type three error, which arises when a false problem is addressed. Action errors of the fourth and fifth kind arise when deciding whether and when to act. A compound error of the sixth kind can arise from combinations of the other errors that produce interactions with unforeseen consequences. These errors call attention to looking at actions beyond those surrounding a choice. In Chapter 13, Hal Rainey, John Ronquillo, and Claudia Avellaneda consider a key feature in the context of decision making, offering a discussion of how public sector decisions provides special challenges and difficulties. In Chapter 14, Hari Tsoukas discusses strategic decision making and knowledge, emphasizing the key roles played by knowledge as both a form of rationality in decision making as well as a basis for making sense of, and helping reduce, uncertainty and encouraging learning. In Chapter 15, Michael Barrett and Eivor Oborn discuss using information technology (IT) to support knowledge sharing in decision making. Using empirical data from a multi-disciplinary healthcare team these authors reveal the complex role of IT in supporting knowledge sharing between different professional groups during the decision-making process.

Part V offers some recent empirical findings that support theories and views presented thus far. In Chapter 16 Sue Miller presents an overview of the Bradford Studies. Summarizing 20 years studying decisions and drawing on the evidence of nearly 200 decisions, she locates the Bradford process studies both in the context of decision-making research and organizational approaches to understanding decisions. The chapter outlines the earlier work of the Bradford group that concentrated on the decision process up to the point of authorization. Miller then

examines recent work by the team on the implementation and performance of strategic decisions. The work finds strong linkages between how decisions are implemented and their success, a combination of organizational context and managerial action. Miller shows that implementation and formulation are inter-connected in distinct ways, lending support to the view that, although difficult to separate completely, deciding and implementing are two distinct phases of decision making. Next, in Chapter 17, Paul C. Nutt provides an empirical look at four decision-making processes used by top managers, drawing on analyses of more than 400 case studies. He distinguishes between successful and unsuccessful processes, and then identifies how particular steps in each process, carried out by tactics, influence the success of each process. This study considers context and content as well and differentiates between the process and tactics, using the success produced by the decisions to determine best practice. Several key practice-based recommendations emerge from the study. Lori Ferranti, Steven Cheng, and David Dilts, in Chapter 18, offer an interesting study of medical decision making that departs from traditional topics. They consider decision-making issues with respect to numeracy and focus on how this influences informed consent, patient knowledge, and healthcare provider communication. In Chapter 19, Lori Franz and Michael Kramer provide a new methodology to study decision content. They offer a comprehensive approach that can be extended to real decisions and real decision makers. This provides another way to treat decision content that complements the approaches offered by Nutt (2002) and the Bradford Studies (e.g. Hickson *et al.*, 1986).

Part VI offers methodologies for the study of decision making. In Chapter 20 Scott Poole and Andrew Van de Ven provide a comprehensive summary of approaches that have been applied to or recommended for the study of change, which they adapt to decision making. Poole and Van de Ven contrast variance theory with process theory, examining how each can be productively applied to the study of decision making. They also consider several other issues that include levels of analysis, the impact of perspective, and the treatment of time. In Chapter 21, Paul C. Nutt offers several new approaches to the study of process, applied to decision making. Nutt contrasts process with structure and shows how research into both is required before an action theory of decision making can be formulated. The focus of the chapter is on investigating process, which has been neglected in the past. Discussion links process research in decision making to several emergent theories found in other fields that consider process. Standards for process research are identified and compared with the traditional standards offered for structural studies. A new research paradigm is proposed that treats process and structure (the decision) as two sides of the same coin. Finally, in Chapter 22 David Wilson discusses the research approach followed in the Bradford Studies. These studies used a plurality of methods ranging from participant observation through intensive case studies to quantitative, multi-variant characterizations of process. Wilson assesses both the strengths and limitations of these approaches and discusses how future research in decision making might benefit from these observations.

The book concludes with Part VII, which provides directions and perspectives in Chapter 23. We sum up what we have learned to date and where research must go from here to begin to formulate a viable action theory for decision making.

## Challenges Facing Researchers

In this final section of our chapter, we outline what we believe to be the crucial challenges facing researchers. Challenges arise because, after almost four decades of research, the concept of a 'decision' remains ambiguous and ill defined. After all this effort, there is no consensus about basic questions, such as whether decisions can be planned or must emerge. Additionally, ambiguity surrounds whether decisions contribute to organizational strategy, and if so, how. The approaches used to study decision making range from the study of individual choice drawing on cognitive or psychoanalytic insights, through decisions as processes with identifiable characteristics, to the practice of decision making that considers implementation, action taking, and performance. A researcher searching for a research topic might view all this as overly complex and look elsewhere. Yet, there is something seductive and intuitively interesting about studying how decisions are made, who makes them, and what happens as a result. Human activity of all kinds is derived from commonsense notions of decision making such as what to do today, what to buy, whom to invite, whom to exclude. Decisions have passed into common parlance – we know a decision when we see one. When one speaks of a decision, there is little uncertainty about meaning. Nevertheless, many challenges arise when attempting to conduct a scientific study of decisions. We address a few of the more important ones.

Decision-making research has become 'de-humanized' over the past few decades. We know more about the characterization of decision processes, such as fast/slow, continuous/nonlinear, comprehensive/simple, and the like, than we do about the behaviours of individuals carrying out the decision-making process. To explain what people do and how they behave during decision making poses a significant challenge.

We would argue that much of the early work on decision making began by attempting to study behaviours but, instead, characterized decision processes (e.g. Cyert and March, 1963; Mintzberg *et al.*, 1976; Nutt, 1984; and Hickson *et al.*, 1986). Such an assertion creates debates that our authors address in their various ways. In this chapter, we emphasize the need for what we call action research to uncover and identify both the behaviour and the process. Such research would give us insight into what decision makers do when they engage in decision activities, but also would enable more fundamental and more closely aligned relationships to performance. As a result, decision research could begin to address the more normative aspects of decision making, such as what managers should do when facing a particular kind of decision situated in a particular way. Such an 'ought' perspective has not been popular or indeed considered proper science for some time. Currently, the preference of organization theorists is to describe what 'is' and then account for what they find using variables found in well known organizational contingencies, such as urgency and risk. Certainly, the relatively recent development in the field that has become known as 'strategy as practice', described earlier in this chapter, has made substantial moves towards rich behavioural description and has made more tentative moves toward more normative approaches. Situating a decision in the actions taken enables the researcher to comment on practice and its relationship to outcomes, such as measures of performance.

The study of decision making as action becomes the study of the localized exercise of judgement within the organization (Tsoukas and Cummings, 1997). As Jarzabkowski and Wilson (2006) note, this involves viewing decision makers as getting things done within situational demands. Decision makers become reflexive actors, situating activities in the context of past actions, current organizational context, and future aspirations. They can also become innovative decision makers, changing these situated activities to suit their needs both now and in the future (Garud *et al.*, 2002). In both cases, simply characterizing the decision process will not reveal how decision makers make do, improvise, and adapt their actions and behaviours in what de Certeau (1984: xviii) has termed 'artisan-like inventiveness'.

We realize there are always counter arguments. One often cited argument asserts that context matters more than individual action. This leads proponents to claim that an understanding of context (and history) will yield a greater understanding of decision making than efforts spent drilling deep into managerial action and behaviour. Micro foci have their limits too. Wilson presents these micro/macro arguments in Chapter 22 and we capture these debates and the various contributions in Chapter 23 that spell out in detail directions and approaches for future decision-making research.



## Notes

1. Parts of this introduction are based upon Wilson (2007).

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