

Part One

MAPPING A TERRAIN

Strategic Management: The Strengths and Limitations of a Field

ANDREW PETTIGREW, HOWARD THOMAS
and RICHARD WHITTINGTON

The purpose of this handbook is to present a major retrospective and prospective overview of research and writing on strategy and management. We make the distinction between the established field of strategic management with its notably successful scholarly journal (*Strategic Management Journal*) and society (Strategic Management Society) and the broader interests in the theory and practice of strategy and management. The classic questions of the strategist have been about the purposes, direction, choices, changes, governance, organization and performance of organizations in their industry, market and social, economic and political contexts. No doubt mindful of the old adage that if you try and see everything, you see nothing, many strategy scholars have chosen to limit their observations by specializing through level of analysis, disciplinary frame of reference and research theme. The field of strategic management has developed in a particular way and this has produced notable strengths and weaknesses, preoccupations and blind spots.

Beyond strategic management, other scholars in the management and social sciences and humanities have also engaged with questions about the direction, organization and performance of institutions most notably in the fields of history, philosophy, political science,

sociology, psychology and economics. Our emphasis on strategy and management in the book title (as distinct from strategic management) is not just a play on words. We take the view that the intellectual development of all fields of management is dependent upon an open and reciprocal relationship with the social sciences and humanities. Progress in developing the theory and practice of strategy and management we believe is more likely to happen through inclusiveness than exclusivity. This means various forms of intellectual bridging and transfer should be encouraged. Bridging across the fields of management has, of course, already occurred most visibly in links between organization theory and strategic management. Industrial economics is well embedded in the field of strategic management and more recently cognitive psychology has also had an impact on theorizing about the nature of competitive strategy within and between organizations. The title is our first invitation for other scholars from the management and social sciences to join us in learning more about strategy and management.

Strategy and management is at the moment an aspiration not an accomplishment. In framing, addressing and synthesizing work in the field of strategy we must start with the existing boundary of strategic management. However, in addressing issues about the future shape and

development of the field of strategy we shall be more adventurous. In organizing the chapters into four parts we have set four main challenges for the future. Part 1, *Mapping a Terrain*, seeks to provide a characterization of the historical, practical and interdisciplinary roots of the field of strategic management and is aspirational about the need to broaden the terrain of strategy. Part 2, *Thinking and Acting Strategically*, gathers together a group of nine chapters which assess research in some of the core fields of strategic management. The emphasis on thinking and acting offers a dual challenge. Firstly, the need to counterbalance the bias in the strategy field towards analytical thought with a greater concern with action, and in using the verb forms thinking and acting, to give emphasis to the dynamic aspects of both. Part 3, *Changing Contexts*, provides a range of challenges to the field of strategy from the rapidly transforming settings within which thinking and acting strategically now occurs. Part 4, *Looking Forward*, consolidates many of the book's strands with a range of epistemological, theoretical, historicist and empirical arguments about the future development of the field.

The design principle as a whole is to encourage intellectual development through seeking focus on the one hand – for example, eschewing an objective of comprehensiveness to seek to map the most significant core terrain for the field – while also acknowledging diversity, of contexts, applications and alternative disciplinary and other paradigms and perspectives threading through this terrain, on the other. Thus the critical analysis and synthesis of diverse disciplinary and other inputs and dynamics around a core set of issues and themes is an important feature and contribution. Contributors have been asked to provide historical overviews of the key strands delineating the topography of their particular themes, addressing the central problem and approaches which have characterized these, to undertake rigorous critical assessments of the state and quality of current theory and knowledge – what is known and not known in the area – and to set out agendas for future theoretical and empirical development. Each author has been asked to consider any salient issues of research methodologies and practice in their topics, as well as addressing the range of literatures and traditions within the social sciences relevant to informed and contextually sophisticated research and

theorizing about strategy and management in a rapidly changing world.

In its contemporary form the field of strategic management is a United States invention and export. However, in an increasingly interdependent and multicultural world it is crucial to recognize differences in institutional and cultural context and diversity in intellectual traditions in different societies. We are sure as editors that we have not gone far enough in recognizing available intellectual diversity. However, the reader will notice that the geographical base of the contributors is 50–50 North American and Europe. Many of the contributors have sought to include published work from the worlds of academia and practice and where possible beyond the intellectual traditions of North America and Europe.

Beyond these opening scene-setting remarks, this introductory chapter has five parts. First, we characterize the history and development of the field of strategic management. We identify major points of development, key authors and schools of thought, notable research themes, strengths and weaknesses, and epistemological and methodological biases. Of necessity this characterization is broad-brush. The more detailed assessment of strengths and weaknesses, gaps and forward looking research agendas are offered in the thematic chapters which follow.

Part 2 of this introduction assesses periodic attempts in the field of strategic management to provide integration and synthesis. It concludes that paradigmatic unity is neither present or desirable in a field opening up yet more to new ideas, and being constantly challenged by rapidly changing contexts. Part 3 provides a brief critical assessment of the field of strategic management from authors inside and outside the traditional boundaries of the field. Part 4 catalogues some of the many contextual challenges to the theory and practice of strategy and in Part 5 we offer brief summaries of each of the 20 chapters which make up the core of this volume.

CHARACTERIZING THE FIELD OF STRATEGIC MANAGEMENT

There are many simplifying statements that can be made to capture aspects of the evolution

and development of strategic management. In its contemporary form, but not deeper intellectual history, its roots are in US academia and practice. Most commentators would agree the field began to take shape in the 1960s with the impact of writing by Chandler (1962), Ansoff (1965) and Andrews (1971). Chandler and Andrews were key professors at Harvard Business School. Andrews was part of the powerful general management teaching group at Harvard and Chandler was a Harvard-trained social scientist who virtually single-handedly created a corpus of work using the comparative historical method which had an influence right across the social sciences. Ansoff had a different intellectual and institutional history. He came out of the intellectually dominant Carnegie School, then led by Cyert, March and Simon, but quickly launched himself off into creating a new more interdisciplinary management school at Vanderbilt University before in turn wandering restlessly into Europe and beyond. In *Corporate Strategy*, Ansoff (1965) outlined a more rationalistic and planning orientated view of strategy than the general management focused view of business policy emanating from Harvard.

These early academic roots were complemented by a strong practice element focused around a group of initially US-based strategy consultancy practices. Three of these, McKinsey, BCG and Bain, eventually became world leaders in developing and diffusing the language and techniques of strategy throughout the world. McKinsey quickly picked upon Chandler's path-breaking work on the multidivisional organizational form and his dictum that structure follows strategy. The opening of the London office of McKinsey in 1959 was a key factor in exporting the virtues of the M Form into the boardrooms of Europe. Around the same time, planning functions were building up in many large US and European firms and Shell International became one of the very few European points of influence in developing the technologies of corporate planning (Wack, 1985).

Meanwhile away from the prestige of the Harvard Business School and the consultancy salons of McKinsey, BCG and Bain, an alternative spring of innovation in strategy was taking shape at the Krannert School of Management, Purdue University, Indiana. In 1969 Dan Schendel set up the first doctoral

speciality in strategic management. By 1972 he and a junior colleague, Hatten, were using the Academy of Management proceedings to challenge the Harvard intellectual control of the subject. Their 1972 paper 'Business Policy or Strategic Management: a broader view for an emerging discipline' began to push not just a new name and rallying cry, but a more analytical and economics-based view of the field of strategy than had hitherto existed (Schendel and Hatten, 1972). This early claim for influence was allowed to fester until Schendel and Hofer took the much more visible and politically effective step of gathering like-minded professors together at a conference at Pittsburgh University in 1977. If Harvard was to be challenged, a new paradigm for strategic management needed not just an intellectual rallying cry but also a new institutional form which could compete in a way that Purdue alone could not. By 1979 Schendel and Hofer (1979) had published a book-length manifesto for the new field of strategic management. This was quickly followed in 1980 by the publication of Volume 1 of the *Strategic Management Journal (SMJ)* and the creation of the Strategic Management Society (SMS) whose annual conferences became a forum for the 'a' (academic) 'b' (business) and 'c' (consultancy) fraternities interested in the strategic management of the firm.

Dan Schendel became the first President of the SMS and remains Editor-in-Chief of the *SMJ* to this day. The *SMJ* has become the pre-eminent journal publishing scholarly work in the field of scientific management and is often rated in the top five of academic journals in the broader field of management. The *SMJ* has been crucial in setting the academic tone for the field. Part of the Schendel manifesto was to create a more scholarly, analytical, positivistic and quantitative treatment of the subject than had existed in the 1960s and 1970s Harvard Business School approach to the subject. This has encouraged the rise in importance of economic theories and econometric methods in strategic management paradoxically at the time in the 1980s when another Harvard Business Professor, Michael Porter, brought his Harvard economics training into the field. A combination of Porter's 1980 and 1985 books, plus the export of Harvard MBAs into the major US consultancy firms and executive corridors, maintained Harvard's influence,

but Schendel's energy and presence, together with the new institutions he helped to create, completely altered the intellectual and political landscape of the field of strategic management. The summer and winter special issues of the *SMJ* have been crucial mechanisms to signal major changes and consolidating points in the field. Schendel until very recently used the opening essays in these special issues to put his own gloss on these intellectual developments, and through his selection of the editors of these special issues, reaffirmed and consolidated academic reputations.

Looking back, Schendel has been an enormously skilful surfer of the main waves, never allowing himself or the *SMJ* quite to get left behind. However, this intellectual and institutional trajectory has had its weaknesses as well as its strengths. Both the *SMJ* and *SMS* have had their critics and there have been periodic questions about the lack of critical reflection and narrowness of the epistemological, methodological and theoretical base of writing in the field of strategic management.

It is now commonplace to talk of the post-Porter era in strategy, perhaps as we shall see the more general changes in epistemological and theoretical discourse in the social sciences at the beginning of the 21st century, together with the empirical challenges from the changing world-wide business, economic and social context, will collectively push the field of strategy and management in some fruitful new directions?

March (1996: 278) has recently reminded us that 'the writing of history is a conceit of survivors'. Certainly it is the case that history is shaped by those with the motivation to grasp opportunities to craft narratives. Truth is socially shaped and so are fields of inquiry. Cannella and Paetzold (1994) – commenting on the paradigm wars in organization theory prompted by Pfeffer (1993) and energized by Van Maanem (1994) – have rightly summarized that 'science is not a magnificent march toward absolute truth, but a social struggle amongst the scholars of the profession to construct truth' (1994: 332). Equally well, there is the important issue of how we characterize the trajectory of development in a field of knowledge. Tsoukas and Cummings (1997) suggest that we can look at a field as a process of progress and progression, upwards and onwards to a set of unifying theories, or we can use

Foucault's (1966) metaphor of a kaleidoscope with its implication of discrete fragments falling into patterns as the kaleidoscope is twisted at key cusps in a field's development. This kaleidoscopic view implies that any new pattern is not necessarily any more true or false, but is merely there. When we explore in a moment some of the key content areas of knowledge in strategic management, we will see clearly that strategic management as a field shares many of the characteristics of its fellow travellers in the social and management sciences. Unlike earlier characterizations of the natural sciences, where the stereotype is of knowledge accumulating progressively and linearly like some clear edged and tidy ribbon, in the social sciences knowledge seems to accumulate more as a mosaic, the patterning on an untidy quilt. This quilt-like form is certainly evident in the management sciences and especially so in the field of strategic management, where the fads and fashions of a field living with the duality of theory and practice periodically emblazon a patch with sharp vibrant colours which pushes the other patterns out of sight and mind.

So what patterns are evident in the patchwork quilt of theory, empirical findings and practice we call strategic management? Is there a clearly defined family tree of knowledge, a clear genealogical structure with notable classics, distinct frames of reference and roots into the social sciences and practice? In Chapter 18 Tsoukas and Knudsen argue that since 1980 the bulk of published research in strategic management has been in the normal science tradition. This, they argue, has been a necessary part of legitimating a new field of study which from the outset faced the combined scepticism of other parts of the social and management sciences and the world of practice. The history of strategic management is a story of promiscuous borrowing from other disciplines and sub-fields of management. Many have implied this borrowing has been one way with little reciprocity between strategic management and other disciplines. This is an accusation, however, that could be made against all the disciplines and fields of management. They have liberally taken but so far have given little back to the core preoccupations of the social science disciplines.

But in amongst the at times casual acquisitions from several disciplines there has been a

consistent reaching out to the theoretical apparatus of economics. This approach was part of the manifesto of the new strategic management to give a sharper form of theorizing than that available in the heroic posturing of the Harvard general management tradition laid down in the 1960s and 1970s. Porter's (1980, 1985) enduring contribution has been to bring the language of industrial organization (IO) economics into the field of strategic management. This switched the gaze of the strategist from the firm to the industry structure. The main determinant of firm performance was now to be described and prescribed in terms of industry sector and not the goals, structures, dynamics and leadership of the firm so beloved of the business policy scholars. But this move from an 'inside out' to an 'outside in' paradigm (McKiernan, 1996; Hoskisson et al., 1999) was not deep enough or far enough for some. Camerer (1985) made a sharp plea to economize the field still further and this was followed up by an important paper by Rumelt et al. in 1991 which reaffirmed and blessed the importance of economic thinking in strategy. IO economics had the greatest impact, but the new economics held the greatest promise. This message was softened slightly in the 1994 volume by Rumelt et al. The title of this volume, *Fundamental Issues in Strategy*, switched the focus from the primacy of the discipline as the driver and enabler of intellectual development to the issues and themes in the field, but the tone of the book remained firmly wedded to economics as the core discipline and the section of the 1994 book designed to signal more process-oriented thinking and research was abandoned.

Various other sub-fields of economics other than the original IO approach have continued to have a strong impact on the field. Notable examples include game theory (Camerer, 1991; Saloner, 1991; Nalebuff and Brandenburger, 1997), agency theory (Fama and Jensen, 1983; Hoskisson and Hitt, 1990), transaction costs economics (Williamson, 1975, 1985; Hoskisson, 1987) and evolutionary theories of economics (Nelson and Winter, 1982). These developments have been welcomed by many (for example, Hesterly and Zenger, 1993) but have also led to sharp criticisms about the a priori theorizing of economics, the model of man paraded in both maximizing and satisfying views of economics and the love economists

have of databases rather than seeking direct engagement with phenomena at firm, sector or market levels of analysis (Perrow, 1986; Hirsch et al., 1987; Foss, 1996). Whipp (1996) caught the mood of the sceptics well in arguing that strategy was too important an area of theory and practice to be annexed by a single discipline. Whipp concluded his 1996 review of the field of strategy and organization by noting that a disciplinary takeover of the strategy field was unlikely and there were many encouraging signs of synthetic and boundary spanning work in the field. We will return to issues of integration and cross-fertilization in part 3 of this chapter.

Probably the most comprehensive reviews in article form of theory, research and methods in strategic management have recently been supplied by Hitt et al. (1998) and Hoskisson et al. (1999). The Hoskisson et al. paper on theory and research uses the metaphor of 'swings of a pendulum' to characterize the field's development. The period from the mid 1960s to the late 1990s is portrayed as four eras of development. Throughout what they describe as a period of significant growth in diversity of topics and research methods, Hoskisson et al. see the constant focus being the examination of business concepts that effect firm performance. However, they also see periodic swings from an internal firm focus to external firm focus and then back again. Thus thinking moved from the 1960s and 1970s work in the business policy tradition to an externally focussed era in the 1980s dominated by IO economics, then in the mid 1980s, with the rise of organizational economics, an attempt to mix inside and outside perspectives, and finally with the rise in the 1990s of the resource and knowledge based theories of the firm, a swing back to an internal firm focus in explaining firm performance.

The resource based view of the firm is traceable to the Cambridge based economist Edith Penrose and her classic 1959 book, *The Theory of the Growth of the Firm*. However, the conceptual transfer of this approach into the strategic management literature is generally credited to Wernfelt (1984). Important theoretical developments have also come from Barney (1992) and Grant (1991) but the mass-market popularization of the core competences of the firm had to wait for Prahalad and Hamel (1990) and Hamel and Prahalad (1994). The resource based theory maintained the long-term

preoccupation with the determinants of competitive advantage but switched the focus from industry structure, strategy groups and external competitive dynamics to the particular constellation of tangible and intangible resources developed by the firm. The so-called knowledge based theory of the firm perpetuated this internal resource focus but in elaborating a more process orientated view of the acquisition, maintenance and utilization of knowledge resources gave a further twist to the particularities of firm behaviour and deepened the pendulum swing to internal firm dynamics (Kogut and Zander, 1992; Nonaka and Takeuchi, 1995).

With the pendulum swings in theory and research, Hitt et al. (1998) also argue, have come switches in levels of analysis, theoretical orientation and then research method. They note that the early business policy work tended to use single case studies or comparative case studies. The IO work and its derivatives moved to incorporate the econometric analysis of surveys and databases, and with the switch of interest to the particularities of resource acquisitions and knowledge development processes there has been a return to smaller sample studies sometimes accompanied by surveys of limited samples of firms. Schwenk and Dalton's (1991) paper on the changing shape of strategic management research gives a useful overview of the central tendencies of strategy research up to 1987 and just before the resource based theory of the firm began to take hold. They noted the standard criticisms of strategic management research up to the mid 1980s had been the absence of studies of strategy implementation, and of the determinants of firm performance, the overuse of nominal and single-item scales and the lack of attention to construct validity of scales. Longitudinal studies were very rare, comparative and cross sectional research using surveys and databases was the great preoccupation.

In their survey of published strategic management research in six top US academic journals in the years 1986 and 1987 Schwenk and Dalton (1991) found more continuity than change in the content and methods of strategy research. One-third of the articles used the static metaphor of 'fit' from contingency theory to address issues to do with the content of strategy. Some 72% of strategic management research relied on data derived from surveys and archival material. There was a continuing strong emphasis on

performance as a dependent variable, using hard and soft measures of performance. Seventy-five per cent of studies were cross-sectional. Of the 25% of studies which had longitudinal data, only 12% of these analysed the data in time series terms. In spite of conclusions about the increasing maturity of the field, the picture before the rise of the resource and knowledge based theories of the firm was of little experimentation in theory and method. If strategic management in the mid 1980s was adapting at all it was through low risk exploitation and not through higher risk exploration (March, 1991).

There are at least two important conditioning statements to be made about the otherwise very useful synthetic reviews of the field supplied by Schwenk and Dalton (1991), Hitt et al. (1998) and Hoskisson et al. (1999). The first is the overwhelming geographical bias of all three reviews. Schwenk and Dalton (1991) report only on strategic management research published in top US journals. Scholarship in Europe and beyond is totally ignored. The more recent assessments of work in strategic management by Hoskisson et al. and Hitt et al. is equally partial. Both of these review papers cite around 250 items of published research. The Hitt et al. paper is solely dependent on US-based work and Hoskisson et al. can only manage to incorporate six or seven references to non-US work in their assessment of theory and research. As McKiernan (1996: xiv) in Volume 1 of his *Historical Evolution of Strategic Management* curtly puts it, there is a constant 'need to counter-balance any easy assumption that any single geographical source has monopolized the history of thought in the subject'.

The other major limitation of the Schwenk and Dalton, Hitt et al. and Hoskisson et al. reviews is their lack of critical reflection on the field, including their reluctance to cite authors who have pointed to the limitations of theory and research in strategic management. Thus Hoskisson et al. ignore the gentle epistemological critique of Bourgeois (1984), the more radical reflections of Shrivastava (1986) and the European critical management theorists such as Knights and Morgan (1991) and Alvesson and Willmott (1995). Even harder to understand is the complete disregard of Mintzberg's alternative process view of strategy (1978, 1990, 1994) and the entire field of strategy process and change research (Pettigrew, 1985, 1992; Chakravarthy and

Doz, 1992; Johnson, 1987). But if the field of strategic management more generally has been short on reflexivity (Whipp, 1996), it has not been devoid of thematic and theoretical eclecticism. In the 1990s there has been a clear trend to tidy up the resultant fragmentation by seeking out cross-fertilization, integration and complementary scholarly development.

INTEGRATION AND CROSS-FERTILIZATION IN STRATEGIC MANAGEMENT

In a range of carefully argued papers, Abrahamson (1991, 1996) uses the language of fads and fashions and band wagons to characterize management techniques as fashion commodities. Unlike many aesthetic and cultural forms, Abrahamson argues management techniques emerge and are justified through a combination of rational efficiency (sound means to achieve important ends) and progressive (new as well as improved relative to older management techniques). He cites the rise and eclipse of quality circles in the United States in the 1980s as a contemporary example, but also draws on historical data to show that management fashions about, for example, employee stock ownership schemes have gained and lost popularity since the turn of the twentieth century. Crucially, Abrahamson also argues that management fashions are not cosmetic and trivial. They have done and continue to shape the behaviour of managers all over the world and can have massive – sometimes helpful, but also questionable – impacts on organizations and their people.

All of the fields of management theory and research have been exposed to fads and fashions. The crucial interest and role of senior executives and external consultants in the strategy domain alongside their academic collaborators has made the field of strategic management more susceptible than most to the rise and fall of bandwagons. Academic reputations and consultancy practices in the social and management sciences are often built upon this rise and fall of analytical language and their associated programmatic management techniques. The fact that the field of strategic management has constantly pivoted between concerns for theory and practice has in some sense kept the field

honest and alive. Detached a priori theorizing at some point will be challenged by the practical agenda of the senior executive or consultant – or indeed by the sceptical gaze of the empirical researcher. Nevertheless, in an over-published world the constant drive for recognition and a place in the scholarly and consultancy marketplace has meant that novelty is prized over the careful accumulation of evidence-based knowledge. So apparent innovation may be spurious and ephemeral, a language game just as easily won as lost.

But the resultant fragmentation in strategic management research is not just a product of drives for linguistic novelty. The core of strategy's interest in the direction, purpose, strategic leadership, organization and competitive performance of organizations has created a multidisciplinary melting pot crowded by aspirants claiming 'twas from my loins it came!'. Claims for originality are often expressed initially in didactic terms – this innovation is at the expense of that now fading framework or technique. The past is pushed off-stage by the hard edge of exclusivity of a new paradigm or frame of reference. In time, however, extreme positions get watered down and some scholars look for the supremacy of unifying paradigms (Camerer, 1985; Sanchez and Heene, 1997), or more integration and cross-fertilization between complementary approaches (Barney, 1992; Zajac, 1992; Seth and Thomas, 1994). All these patterns in the social production of knowledge are apparent in the history of development of strategic management.

As we have already argued, the strongest history of the search for a unifying paradigm in strategic management research is associated with the drive for unity through the theoretical and methodological approaches of various strands of economics – from IO economics, through transaction costs economics, agency theory, game theory and latterly organizational economics (Camerer, 1985; Rumelt et al., 1991; Mahoney, 1992). These aspirations remain unfulfilled, indeed the rise of the resource based theory of the firm and its consolidation and development in the knowledge based theory have provided for some an alternative unifying paradigm. One of the strongest statements of this is the recent paper by Sanchez and Heene (1997) which is critical of the 'split personality' in the field with the bi-polar focus on inside-out and outside-in

approaches to strategy. The way forward, argue Sanchez and Heene (1997: 304), lies in unifying the field around the language of competence-based competition which 'requires the effective integration of internal organizational and external competitive dynamics'. This the authors argue is not just because of evident fragmentation and polarization in the field but because the understanding of the ever changing global context of business requires a dynamic, systemic, cognitive and holistic treatment of competence-based competition within and between firms.

In their search for cross-fertilization and complementarity, others have been more circumspect, arguing the jigsaw should be put together piece by piece rather than attempting a Herculean synthesis. Thus Barney (1992) has argued that the rise of the resource-based theory of the firm offers new opportunities to bring more organization theory into the strategy domain but this time not just to deepen appreciation of strategy processes but also to help disentangle the origins and development of socially complex competitive resources such as trust, change and choice capability and creativity. Zajac (1992) has argued for greater cross-fertilization of economic and behavioural science approaches to strategy. He shows how questions reliant on economic thinking alone (from IO economics, agency and transaction costs theories) can be enriched by posing complementary behavioural questions.

Later empirical work in the corporate governance area by Zajac and Westphal (1996, 1998) shows that Zajac has practised what he preached. Seth and Thomas (1994) are also preoccupied with the links between strategy research and economics and, like Zajac, they are wise enough to see that this is only possible where surface posturing is shed to examine incommensurable theoretical assumptions from any perspectives attempting a rapprochement. Zajac (1992) makes the point that any cross-fertilization work is only as strong as its weakest link and that the integrative scholar needs to pass the tough test of being able to speak both languages with equal depth and fluency. Seth and Thomas (1994: 186) have an equally hard-headed message for those still searching for a unifying paradigm. More reflexivity they argue is necessary to create a field where researchers 'are particularly sensitive to the various assumptions underlying

their frames of reference, the utility of those assumptions in framing theory and to communicating these'.

The rise of organizational economics and institutional theory in the 1990s have also benefited theory development and empirical research in strategic management. In a kaleidoscopic treatment of the literature on organization economics and strategy, Mahoney (1992) rests his case on a heightened conversation between the two fields and on the rejection of Kuhn's (1970) incommensurability of paradigms thesis. Mahoney (1992) sharply dismisses incommensurability as an approach designed to legitimate intellectual vested interests, and not much else. However, Mahoney's over-inclusive definition of organizational economics (particularly the inclusion of the behavioural theory of the firm and the resource-based theory of the firm) displeased Eisenhardt and Brown (1992).

Sociological insights into the strategic analysis of firms have come from a variety of directions. Pettigrew (1985, 1992, 1997) has brought Giddens' (1979) theory of structuration and Sztompka's (1991) theory of social becoming to deepen theorizing about strategy processes. These theoretical developments have been enriched and illustrated by a whole series of empirical studies at the Centre for Corporate Strategy and Change, Warwick University (Pettigrew, 1985; Pettigrew and Whipp, 1991; Pettigrew et al., 1992; Ferlie et al., 1996; McNulty and Pettigrew, 1998; Pettigrew and Fenton, 2000). Oliver (1997) has sought to link developments in institutional theory with the resource-based theory of the firm. Her argument is a complementary one, that a firm surely needs resource capital and institutional capital for longer-run competitive advantages. The dynamic perspective now available in institutional theory brings a welcome temporal perspective to earlier variants of the resource-based theory which were curiously uncurious about how resources were created, modified and utilized over time.

A third and most recent dialogue between sociological theory and strategic management is now occurring between economic sociologists and strategists (Dobbin and Baum, 2000). Again, the way forward is to eschew vague notions of unity of perspective and to start by disentangling common research themes and questions; identifying similarities and

differences in core assumptions and then search for complementarity rather than integration.

In spite of periodic cries for a unifying paradigm in strategic management, one has not appeared, it is unlikely to do so and it would be creatively destructive if it did arrive. As Schendel (1994) and many others have pointed out, the sheer complexity of the subject matter of the strategy field, the historical pathway of eclecticism of theory and method, and the field's roots in multiple disciplines and in practice, have all created a rich body of theory and practice. Recent attempts to look for bridges across complementary traditions, combined with an increasing visibility of sociological and psychological approaches, have counter-balanced earlier dependence on economic theorizing about firms, markets and industries.

Further maturity is likely to come from the cross-fertilization of theories in big empirical studies using multiple methods, and from an increasing willingness to critically reflect on old assumptions and novel developments. It is to this historical predisposition not to critically reflect that we now turn.

STRATEGIC MANAGEMENT: SOME CRITICAL REFLECTIONS

As Whipp (1996) and others have commented, one of the most serious and intellectually debilitating aspects of strategic management as a field is its lack of reflexivity. There are a few notable iconoclasts (Camerer, 1985; Shrivastava, 1986; Mintzberg, 1990, 1994) but by and large it is a field unendowed with a developed critical tradition. We can speculate why this might be so. One reason is certainly the theory and practice duality and the need to meet the often conflicting expectations of the various stakeholders interested in the subject. Few senior executives and consultants we have met have much tolerance for academic deconstruction, which is often seen as a form of irritating self-abuse. Practitioners of strategy are much more likely to be impressed by creative problem-solving ability – 'if it works for me, it works' is the emblem for that form of pragmatism. The stage of development of the field in a constantly changing context is also a factor.

In its contemporary form the field is only 40 years old and that has been an era of laying down foundations, building a reasonably consistent intellectual language and trying to establish some basic patterns in what is known and not known. In this pioneering era it may not be surprising that few scholars have been prepared to challenge the core beliefs and assumptions of the field whether they are about the concept of knowledge, rules of evidence, levels of analysis or mode of human action grounded in the field. Until recently it has been very difficult to identify any epistemological writing on strategic management, but the critical reflections from this source are now beginning to flow, most especially from European-based scholars such as Pettigrew (1992), Calori (1998), Tsoukas and Knudsen (see Chapter 18) and Cummings and Wilson (in press).

One form of meta level critical reflection which is evident is the preparedness of some authors to organize writing in the field into classificatory schemes in order to clarify central tendencies in the field and thereby identify areas of focus and relative inattention. Whittington's (1993) book *What is Strategy And Does it Matter?* is one such contemplative assessment of the field and the more recent book by Mintzberg et al. (1998) is another. The forthcoming book by Cummings and Wilson (in press) is a further attempt to offer critical reflection through juxtaposing alternative frames of reference and illustrations of strategy making in action.

Bourgeois (1984) was a very early questioner of the determinism of IO economics in the field of strategic management and at a time when the Porter era was beginning to take hold. He noted the reductionist and determinist character of IO economics, the failure to examine mutual causal links between industry structure and firm behaviour, the need to study the content and process of strategy reciprocally and simultaneously and the obvious value of combining qualitative and quantitative data in a field which constantly needed to move between the particular and the general. This was dangerous stuff in 1984.

However, Shrivastava outmatched Bourgeois in his direct attack on the uncritical ideological bias of the field of strategic management. His stated aim was to encourage researchers to 'examine their unstated managerial values and

assumptions, and to encourage them to generate less ideologically value-laden and more universal knowledge about the strategic management of organizations' (Shrivastava, 1986: 364). This criticism by Shrivastava, which ended by concluding that the ideological character of strategic management research was a crucial legitimator of existing power structures, has since been used as a totem and point of entry for critical management theorists such as Alvesson and Willmott (1995), but Shrivastava has been virtually ignored in most other subsequent reviews of the field.

Mintzberg has been a much more sustained, effective and creative critique of the field of strategic management. After some early high-impact empirical work on the nature of managerial work (Mintzberg, 1973) and the character of strategic decision processes (Mintzberg et al., 1976), Mintzberg has found his place in a series of iconoclastic challenges of the nature of strategy and of strategy processes (Mintzberg, 1978, 1990, 1994; Mintzberg and Walters, 1985; Mintzberg et al., 1998). At a time when IO economics was dominating the field, with the black box of the firm bobbing helplessly around the economic bath tub, it could be said that Mintzberg's writing began to humanize the field of strategic management. He used his energy and Herculean reading, combined this with great skills in conceptual pattern recognition and evocative writing, to attack one cherished belief after another.

Strategy was no longer to be seen just as rationally intended purposeful thought. The strategists were no longer to be portrayed as heroic general managers, but might involve actors in and outside the boardroom. Even the unthinkable was articulated, action might precede thought. Strategizing could now be thought of as reconstructions after the fact, rather than rationally intended plans. Strategies could be intended, emergent and unrealized. The linear view of process explicit in the old cartesian dichotomy of strategy formulation and strategy implementation was questioned. With increasing interest in the enduring characteristics of structural and strategy context, Chandler's dictum that structure followed strategy was modified to include the possibility that strategy may indeed follow structure. These criticisms carried on unabated and had a tremendous influence on establishing the field

of strategy process research and strategy change research. Mintzberg's 1994 book further dissected the notion of the purposeful organization and provided an extended obituary for planning as a technology to aid processes of strategy development.

Meanwhile a similar challenge to some of the core rationalist and contingent thinking in organizational analysis and strategic management was developing in Europe. Pettigrew (1973, 1977) drew attention first of all to the significance of the distribution and use of power as a shaper of decision and strategy outcomes, a perspective at that time virtually ignored in the literature on strategic management. Later work (Pettigrew, 1979) then brought in the language of culture in organizational analysis, and then a combined political and cultural view of process was fashioned around a large-scale empirical study of strategic change processes in ICI (Pettigrew, 1985). The latter study and subsequent theoretical writing (Pettigrew, 1992) attempted to break the two absurd dichotomies then so accepted by strategy scholars (strategy formulation and implementation and strategy content and process). The series of large-scale empirical studies at Warwick Business School (Pettigrew, 1985; Pettigrew and Whipp, 1991; Pettigrew et al., 1992; Ferlie et al., 1996; Pettigrew and Fenton, 2000) articulated and developed a new approach to strategy process research which combined the content, process and context of change with longitudinal data collected at firm, sector and economic levels of analysis. Parallel empirical research by Johnson (1987) and Smith et al. (1991) helped to establish a European tradition of research on strategy process, but the key to the door of strategy process research had been provided by Mintzberg.

The field of strategy process research has helped to open up the black box of the firm and humanize the field of strategic management. The old dichotomies of strategy process and content and formulation and implementation have withered away and a new impetus has been given to time, agency and dynamics in addressing important issues to do with the strategic development of the firm alongside processes of choice and change. Strategy process has also brought a new epistemological and methodological tradition into the field of strategic management. The examination of

processes over time has demanded longitudinal data often collected through retrospective and real time analyses (Pettigrew, 1990, 1997). The exploration of processes in their contexts has required skill in collecting time series data over multiple levels of analysis. Power, politics, culture, learning, evolution and development are now appropriately at the centre stage of the discourse of the field. However, there is still much to do in developing this dynamic tradition in the study of strategy processes and we will suggest some important lines of development in the concluding chapter.

Before we leave time and process it is important to discuss the potential of history and the historical method for the development of strategic management. Many of the strategy process researchers already mentioned have shown a strong interest in the relationship between the past, the present and the future. They have been interested in the heavy hand of the past, the tensions between free will and determinism in historical explanation and many have tried to collect retrospective data sometimes over 30 years to underpin their analyses of changing (for example, Pettigrew and Whipp, 1991; Miles and Cameron, 1982). Notwithstanding this tradition of process research, as Hendry (1992: 207) has rightly argued, 'despite the early influence of Aldred D. Chandler's *Strategy and Structure* on the field of business strategy, the interaction between business history and business strategy research has been minimal'. This is surprising since the two fields, with their interest in change and the responses of organizations to changing environments, appear to have much in common. Given the interest in the discipline of history in European scholarship and the tremendous impact across the social sciences of Chandler's earlier work, it is also surprising that aside from Chandler's home base at the Harvard Business School, no other US or European business school has strongly invested in the field of business history or of historical investigation.

Of course, some of the shine has been taken off Chandler's reputation by the much more critical stance taken to some of his later work. As Chandler moved from the within country analyses of the rise of the 'M' form in the USA to the cross country analyses of business history in the United States, Britain and Germany (see *Scale and Scope*, 1990) so his

evident qualities as a grand theorist were taxed more and more by the twin perils of universal theorizing and the disentangling of very long chronologies set in quite different social, cultural and institutional contexts. Some examples of this respectful criticism of Chandler's later cross-country comparative work include Supple (1991), Church (1993), Hannah (1999) and Whittington and Mayer (2000). As Hannah (1995) has succinctly put it 'only the exceptionally gifted and brave (or the criminally foolhardy) venture into both grand theory and comparative empirical work!'. Hannah (1999) later did try this comparative historical research himself, and rightly so, for with the sociological tradition of similar comparative historical research, history is well placed to ask big questions over a long time series and thus act as another counterpoint to the largely ahistorical field of strategic management.

Hannah's (1999) analysis of the survival and size mobility of the world's largest 100 industrial corporations, 1912–1995, is, of course, the link with the other two fields of work – economic sociology (Baum and Dobbin, 2000) and evolutionary perspectives of strategy (Barnett and Burgelman, 1996) – where there has been a serious treatment of history and the historical method. The evolutionary perspective as summarized by Barnett and Burgelman is a humanized variant of population ecology theory now itself facing revisionist critiques (Aldrich, 1999). Barnett and Burgelman (1996) return to the old strategy management theme of what determines the success and failure of organizations, but do this in an altogether more dynamic and historical fashion. They explore how variation in firm strategy impacts on variation in the rate and pace of innovation and thereby affects firm performance. They also offer the much needed connection to another long held bias in the strategy field, the preference for studying apparent high performers in isolation from a comparable set of lesser performers in similar product market and economic conditions. This kind of analytical approach is helping to keep history in the field of strategic management but we concur with Hendry (1992) that the field will continue to be found wanting without more comparative and historical studies. Jeremy (see Chapter 19) develops the theme and importance of business history and strategy.

In the 1980s probably the two dominating figures in strategic management research and writing were Mintzberg and Porter. Given the quality, visibility and impact of their work it is surprising how few critical assessments have appeared in writing. Aside from the well known acerbic dialogue between Mintzberg (1990) and Ansoff (1991), we can find no comprehensive review and evaluation of Mintzberg's work. Tsoukas (1994) offers a partial assessment of Mintzberg's writing. Aside from an excellent published assessment by Foss (1996) and an unpublished conference paper in the critical management tradition by Harfield (1999), we can find no other penetrating review of Michael Porter's work. Foss's (1996) paper is valuable because it assesses the Porter contribution alongside the wider debate about the contribution of economists to strategy research.

Foss portrays the strategy field as a fragmented adhocracy, a field which has become too pluralistic, idiosyncratic and uncoordinated. He also sees Porter's work becoming more eclectic over time with practice issues in the 1985 and 1990 Porter books driving the increased variety of sources away from the more pure exposition of IO economics evident in the 1980 book. However, the distinctive and additive teaching and consultancy message was evident in each book. The 1980 Porter book provided the five forces framework, the value chain analysis came in 1985, and Porter's 1990 book provided the diamond framework. Between them these three frameworks for a time dominated the classrooms and consultancy salons where strategy was on the agenda, but their impact on the research agenda for strategy were not as deep or enduring. As Foss (1996: 16) concludes 'Porter turned IO economics into a normative framework for strategy' and this is where his enduring reputation will be counted.

Thus far we have discussed some of the critical reflections of authors largely from within the mainstream of the strategy literature. However, we would be partial if we did not recognize the critical management community and their more radical critique of the field of strategic management. Mainstream strategy research and writing is a particular target for the critical management theorist. Why? One factor has been its rapid growth and pervasiveness as a language system within

management studies. As Whipp (1996) cogently argues and illustrates, the strategy word has become not just a keyword but an all-embracing buzzword signifying not just generalship and competitive behaviour, but also significance. The fact that the strategy literature is embedded in many literatures, but also emphatically looks to theory and practice and the thoughts and actions of the very top organizations, incites accusations of non-reflective managerialism. The association with the worldwide consultancy industry and a cadre of often senior and presumably well-paid academic consultant professors attracts envy and cries of intellectual superficiality. We can all think of examples which warrant these sort of barbs.

The critical management research community is, of course, still developing (there is now even a group within the US Academy of Management). However, it is as broad a church and just as ideologically conscious and fragmented as the field of strategic management itself. Looking for central tendencies in this critical treatment of strategy management is not easy. The fact that only a limited amount of the output of critical theorists is published in established journals and monographs is understandable but regrettable. Much of the writing from critical theorists still circulates around as working papers and conference papers or in unofficial issues of unofficial journals. Thus far the net effect is an extremely uneven quality of debate and little empirical work to substantiate critical assessments of mainstream work, or any new empirical possibilities raised by this developing tradition of censorious writing.

Much of the critical theory now available originates in Europe. The most developed is associated with scholars much influenced by Foucault and other discourse analysts (Knights and Morgan, 1991; Knights, 1992). Another tradition also evident in Europe is based on philosophical and epistemological critiques of management studies as a whole as well as that most visible of targets – strategic management (Alvesson and Willmott, 1995; Tsoukas and Cummings, 1997; Calori, 1998). However, Shrivastava's (1986) contribution in the US pre-dates much of this publishing and has been developed further a decade later in Shrivastava and Stubbart (1995). Knights and Morgan (1991) articulate views about the all-embracing

power of strategy discourse, an influence attached to language alone, which is hard to empirically justify but nevertheless offers a well-developed and novel critique of the field of strategy.

Shrivastava (1986) had also picked up the domination possibilities of strategy ideas, techniques and language but wanted to extend this to strategy praxis. In an unpublished paper, Thomas (1999) carries this tradition forward and begins to pin down a developing research agenda which would incorporate three levels of analysis. These are the discourse analysis of discursive texts in strategy, the behavioural analysis of discursive practices and also the social contexts in which strategic management occurs in organizational settings. A similar agenda is being developed by Whittington (2001) who argues that studying strategy as a social practice (taking strategists and their work seriously) is a long overdue research theme in strategy which fits well the qualitative research traditions in European management research and the bigger theme of strategy process research.

However, in amongst these more considered developments in the critical management theory tradition is a host of polemical, accusatory and loosely written discourse about mainstream strategic management. We are variously informed that strategy discourse is systematically distorted by power and leads to false ideologies and needs myth makers (Ehrensell, 1999); that strategic management is a medium and outcome of domination (Alvesson and Willmott, 1995); and that 'strategy is there to serve the narrow sectional interests of those who can make the claim that they are strategists' (Alvesson and Willmott, 1995: 600). And most messianic of all, 'critical theory values sociological analysis for its capacity to yield insights into the dynamics of domination and oppression, and thereby to provide a stimulus for emancipating change' (Alvesson and Willmott, 1995: 103). Even Alvesson and Willmott admit this is value laden posturing of the most utopian sort, arguing that 'this may appear utopian and unrealistic given the present corporate context' (1995: 605).

But what of the present corporate context and the challenges it may present to the development of a more reflexive, critical and empirically attuned field of strategic management?

SOME CONTEXTUAL CHALLENGES TO THE FIELD OF STRATEGIC MANAGEMENT

The history of the social production of knowledge in the natural and social sciences shows clearly that ideas appear and are blessed or sidelined in particular social, political and economic contexts (Gibbons et al., 1994; Ziman, 1994). Scientists are also citizens of particular settings and cannot readily disengage from the settings that have helped to create them. This view of the social construction of knowledge applies to all forms of knowledge creation but it is a particularly powerful way of interpreting the rise and fall of ideas in the social and management sciences. In a recent assessment of the characteristics of the European Corporation, Whittington and Mayer (2000) debate the veracity of Chandler's (1962) universal propositions about the character and evolution of the modern corporation. They place Chandler's important work on the M Form as an archetype of assumed American managerial superiority and as a form of practice other nations would do well to appreciate and then practise. Indeed, Whittington and Mayer (2000: 23) make the additional and all-embracing point that 'the social sciences made America the universal pattern'.

Not any more, argue Whittington and Mayer. Time and place are a constant reminder of the rise and fall of managerial practices and the patterns of organizational behaviour associated with those practices. Temporal oscillations are clear enough. Think of the anxiety in America with the rise in the power and performance of Japanese corporations in the 1980s. Consider also the revisionist onslaught on this view in the late 1990s as the Far East economic bubble was deflated. Recent research on the variation in governance arrangements across different societies (Scott, 1991; Davis and Useem, see Chapter 11), divergent forms of capitalism throughout the world (Whitley, 1999) and new forms of organizing in Europe and Japan (Pettigrew et al., 2000) indicate how and why national cultures and institutions can shape the strategy and behaviour of corporations. So the management sciences in the past may have been 'not culture free but culture blind' (Whittington and Mayer, 2000: 31). But this contextualist challenge to managerial and

corporate universalism does not mean a retreat to cultural or institutional relativism. Patterns may still be observable within and between corporations in and between different societies, but in an age of globalization and multiculturalism it is a wise scholar who thinks hard about why and how various levels of context shape the strategies and behaviour of firms and the people who work in them.

The present and future development of the field of strategic management is likely to be driven by two compulsions. Firstly, contemporary developments in social and economic theory, some of which have been mentioned in this chapter and to which we will return in the concluding chapter. And, secondly, recent changes in the nature of the business and economic context which we address here. In recognizing the significance of business context as a shaper of research agendas in strategic management we are, of course, acknowledging the inevitable. By and large the social and management sciences follow events rather than create them. This means that one crucial input into deliberations about what are the key researchable questions should be the big themes and issues around us.

The view that strategic management research should be theme-driven rather than theory or technique driven does not, of course, mean that strategy research can make real progress without adequate techniques of data collection and analysis. Neither is it conceivable that knowledge development will flow in the absence of accessible theoretical languages and insights. The question is the balance and reciprocal relation between themes, theories and techniques. Themes are very important in a field like strategic management, which pivots between theory and practice. The themes are the initial problem framer, the necessary condition which exposes the problem, opens up the possibility for interdisciplinary engagement and draws in partners, co-funders and co-producers from the worlds of policy and practice. Crucially, the themes have to meet the double hurdle of embeddedness in the social sciences and the worlds of policy and practice. The aim is also to meet the complementary double hurdle of scholarly quality and relevance (Pettigrew, 1997). In a field such as strategic management it should be possible to meet the challenge and opportunity of these double hurdles to make sure that practical

and academic knowledge work together and mutually raise rather than lower the standards.

The writing on the new competitive environment of business at the end of the 20th century is at times apocryphal and dramatic. The new context for strategic management has been variously described as a 'silent industrial revolution' (Prahalad and Hamel, 1994) or as hyper-competitive (D'Aveni, 1994), brought about by a linked set of market, technological, global competitive, deregulatory and environmental changes. Thus instead of long, stable periods in which firms can achieve sustainable competition advantages, the hyper-competitive context allows only short periods of advantage, punctuated by frequent interruption. In these circumstances, stable end-states are illusory and the re-thinking of strategy and form of organization becomes more or less continuous (Fenton and Pettigrew, 2000). In these conditions Teece et al. (1997) argue the resource based theory of the firm needs to move from a static view of existing stocks of resources, towards an appreciation of innovation and renewal implied by 'dynamic capabilities'. Under contexts of fast-paced change, particular resources and strategies are soon redundant. These changes in form and strategy of organizations have encouraged Whittington et al. (1999) to drop the nouns of organization and strategy and to revert to the more active language of organizing and strategizing to capture the new dynamism in the field of strategic management. We will return to develop this theme of the dynamic character of strategy in the concluding chapter.

Prahalad and Hamel (1994) give a comprehensive overview of the forces impacting on the nature of the competitive environment within industry in Europe, the US and Japan. They cite deregulation, structural changes due to technological and customer expectations, excess capacity, increasing merger and acquisition activity, environmental concerns, less protectionism, technological discontinuities, the emergence of trading blocks and global competition. These, or a subset of these, have impacted upon almost all industries during the 1990s.

The importance of technology as a driver for strategy and organization change is another dominant theme in the management literature. The evolution of technology towards an 'information age' and 'the knowledge age' are

having multiple effects on organizing and strategizing. I.T. is having a widespread impact on information flows within and between firms, on the structural configuration of the firm and management's ability to integrate changes in strategy and organizational form (Pettigrew and Fenton, 2000). Organizing of knowledge resources is now also seen as a key component of market-place competition with the continuous generation and synthesizing of knowledge regarded as key sources of organizational advantage. Knowledge cannot be controlled centrally and is continually changing. The exploitation of knowledge within the firm requires a continuous chase after shifting properties, a process better captured by the dynamics of organizing than the finality of organization (Whittington et al., 1999). All these considerations and more mean that the interface between strategizing and organizing represents one of the key themes in the future research agenda for strategy and management.

Lowendahl and Revang (1998) present a complementary analysis of the challenges to strategic management from what they describe as a post-industrial society. However, they crystallize their analysis around the need for scholars to study the ever more demanding customer with highly individualized needs and the powerful and knowledgeable employee ever more prepared to challenge traditional hierarchical arrangements in older, mechanistic forms of organization. The core strategic issue in the new world they characterize as 'the ability to build and maintain relationships to the best people for maximum value creation both internally to firm representatives and externally to customers' (1998: 757).

Lowendahl and Revang (1998) see the professional service firm as the archetype of the resultant high external and internal complexity. Such firms have dispersed critical competences, loosely coupled internal units with tight coupling to external actors through long-term relationships and increasing complexity in terms of both types and numbers of interaction. With the growth of such firms heavily dependent on recruitment of high calibre people, often with multiple career options, professional service firms are also located at the centre of the war for talent in the new economy. These considerations have many implications for the analytical approach of strategic management. One is, of course, a

new concern for people and relationships in the context of the information and knowledge age. More generally, the trend for strategy frameworks to look simultaneously outside and inside the firm will be reinforced by the rise of the duality of internal and external complexity.

The strategy consultancy industry is normally faster to see and to exploit contextual changes than academic observers. Recently McKinsey have published an anthology 'On Strategy' as a special issue of the *McKinsey Quarterly* – June 2000. The anthology is organized into three parts, corresponding to the main phases in the development of their work over the period 1978–2000. 'Foundations' covers the period 1978–1989, the early years of McKinsey strategy practice. 'The Changing Landscape' contains articles from the mid 1990s and 'Strategy in the New Economy' represents work in progress. This anthology is an extremely valuable archive and source of linguistic analysis for the discourse analysts of the practice of strategy. It is also a useful compendium of consultant views of the changing analytical challenges faced by strategy analysts in the front line of practice.

The first three articles in the changing landscape section discuss the increasing complexity and uncertainty of the strategy task, deficiencies in the five forces model of Porter in the new competitive structures of many industries and the new challenges of conceptualizing the strategy task in 'webs' – clusters of companies that collaborate in and around particular technologies (Coyne and Subramanian, 2000; Courtney et al., 2000; Hagel, 2000).

Coyne and Subramanian (2000) convincingly show how the rise of co-dependent systems cross industry structures such as alliances, networks and economic webs in both the new and old economies are undermining three of the core assumptions of the now dated five forces model of Porter (1980). Prescriptively Coyne and Subramanian (2000) argue that higher levels of uncertainty than those envisaged by Porter in the early 1980s requires not just static notions of posture or position but the active management of an evolving strategy. This they argue can be facilitated by the highly situational use of quantitative and qualitative game theory and an enhanced sensitivity to skills in the execution of strategy.

The Hagel paper in the changing landscape section and the Hagel and Singer paper on 'unbundling the corporation' in the new economy section provide pointers to the strategic impact of webs and networks on the modern corporation. These papers characterize the big strategic issues in webs as which webs to participate in and which role to play in the chosen web. Again there is a close overlap between the strategic issue and organizational choices of the firm. A combination of highly porous firm boundaries, denser information links, pervasive issues of trust and reciprocity and the constant imperative to find, retain and motivate talent make the interface between strategizing and organizing a constant challenge.

The theme of continuous adaptation is picked up in the paper by Beinhocker originally published in 1997 and titled 'Strategy at the Edge of Chaos'. This paper builds on theoretical developments in chaos and complexity theory. The practice focus is the need for robust strategies capable of performing well in a variety of possible future environments. Again flexibility and agility of strategy is explicitly linked to dualities in organizing, this time the need for firms to be simultaneously conservative and radical.

These perspectives from the McKinsey knowledge bank resonate with our own experience of the innovating organization in Europe, the USA and Japan (Pettigrew et al., 2000; Pettigrew and Fenton, 2000; Whittington et al., 1999). They have also been acknowledged by other scholars interested in competitive processes in strategic networks. Bettis (1998) has noted that the units of analysis in the strategy field have not kept up with either available analytical or methodological tools or the nature of competition and strategy in the late 20th century. He argues that the usual units of analysis, the business unit, firm and industry, surely now need to be complemented by work on dynamically changing network or webs of firms. (For a development of this theme see Venkatraman and Subramaniam, Chapter 20.)

Again never quite allowing itself to get left behind, the *Strategic Management Journal* in March 2000 published a special issue on Strategic Networks. In their editorial introduction to this special issue Gulati et al. (2000) push the argument for a new relational approach to strategy. Location in, access to

and skill in operating in networks are now seen to be key additional factors in explaining the old strategy questions of 'why do firms differ in their conduct and profitability?'. The traditional way into network analysis has, of course, been to measure the structural characteristics of these forms (Burt, 1992). Increasingly scholars are now interested in the role of networks as depositories of resources. This approach is now much more processual and relational with skill in entering, bargaining, reciprocating and utilizing the network seen to be fateful for the future development of the firm and the network.

This brief view of some of the contextual challenges to the theoretical, empirical and practical developments in strategic management has emphasized a number of key prompts for future work. We have emphasized the power of context as a shaper of research themes and the analytical languages and methods used to study them. With the growth of management research communities throughout the developed world, there is a greater variety of scholarship and a new awareness of variation in the form and conduct of business throughout the world. This is producing within the USA and other academic communities an increasing challenge to the implied universalism and hegemony of the US social and management sciences. The rise of internationalism and multiculturalism is also heightening awareness of diversity in the strategies, structures, cultures and systems of organizations. However, progress in developing knowledge in any field of the social sciences is dependent upon a balancing act between universalism and contextualism and the retreat into relativism will be self-defeating. In the field of strategic management, which historically has tried to live with theory and practice, there is a further special challenge in meeting the double hurdles of scholarly quality and relevance, and embedding this research agenda in the social and management sciences and the world of policy and practice.

Our contextual review has also mentioned a variety of new and complementary research themes which arise naturally from the emerging context around us. We have dwelt on the new forms of competition arising in both the old and new economies. These provoke challenges to the units of analysis in our studies, the concept we have of strategy and organization,

or as we would prefer strategizing and organizing. They also require an approach to strategic analysis which is more dynamic, processual and contextual than hitherto. But this is a sketch, a taster of what is to come. In what follows we now go on to describe the main menu of this Handbook. From Chapter 2 onwards, detailed analyses are then provided, theme by theme, of the past, present and future of strategy and management.

STRUCTURE AND
INTELLECTUAL LOGIC
OF THE HANDBOOK

The field of research and practice we call strategy is too important to be annexed by a single discipline. One way forward intellectually lies in nourishing and exploiting its pluri-disciplinary roots across the social and management sciences. Openness about epistemology and the social production of knowledge is surely also essential. Strategy may at last be on the point of breaking free from the constraints of its origins in the modernist social sciences of the mid 20th century. As we argue in the concluding chapter, strategy research is now reaching for new directions that are, if not ‘post modernist’, most certainly ‘after modernism’ (Clark, 2000). Throughout this introductory chapter, we have also emphasized the crucial significance of spatial and temporal contexts as shapers of the past and future direction of the field of strategy. The importance of strategy in a contextually dynamic and plural world makes the US bias in the literature not only internationally myopic, but theoretically and empirically restrictive. The double hurdle of scholarly quality and relevance so emblematic of the field of strategy also requires a heightened sensitivity to variations across national cultures, company cultures and within the inner contexts of firms.

The main purpose of a scholarly handbook should be to literally attempt to map the terrain of a field. However, the map should not just be a historical synthesis, but should offer a sharp critical reflection on past and present work. We have encouraged all the authors to criticize, entice and provoke. Handbooks also need to address the question ‘what of the future?’.

All the authors, to varying degrees, have articulated a future research agenda for the sub-field of strategy research they have synthesized and reviewed.

Many of the authors take the view that strategy research is at a ‘cross-roads’. There is ample praise and critical reflection of past and present contributors, but also an impatience for innovation and creativity in the field of strategy. As editors, our own view about the justification for a strategy handbook at this time is also based upon a sense that the field needed reconsideration and renewal. But how might this intellectual development be encouraged? We took the view that in a disparate field progress was more likely to be encouraged through a combination of focusing on the one hand, and acknowledging diversity of contexts, themes, paradigms and perspectives on the other. We have not sought comprehensiveness of coverage for its own sake. The reader will notice, for example, there is no chapter assessing the important strategy research using game theoretic approaches. The four parts do assess many of the key themes in strategy, and do this sensitive to the past, present and the future, and always aware of spatial and temporal context. We have also encouraged intellectual diversity by choosing authors on a 50–50 basis from North American and European intellectual traditions. As ever, the book is limited by its restricted awareness and use of strategy writing emanating from beyond North America and Europe. A challenge for authors in Asia, Central and South America and Africa is to educate us all.

Chapter 2 is written as a testament to the intellectual contribution of Edward H. Bowman who has written many seminal pieces on strategy (Bowman, 1974, 1990) and who died in 1999. Ed Bowman, Harbir Singh and Howard Thomas provide a complementary personal view of the history and evolution of strategic management to stand alongside this introductory chapter. They emphasize and value the field’s essential theoretical pluralism and outline the varying contributions of institutionalists, economists and behaviourists. Throughout their overview they note the reciprocal relationship between the development of theoretical ideas and empirical themes in strategy, and the changing economic, political and social context of organizations between 1960 and 2000.

Bowman, Singh and Thomas locate practical and intellectual contributions in a review of the major tools of strategic analysis and some of the classic books in the field published over the 40-year development of strategic management. In sketching some future directions for strategy research they highlight the need for more longitudinal, processual analysis and the value of taxonomic schemes to simplify, order and give emphasis to the proliferating and sometimes confusing analytical languages in strategy. They share their concerns about the endless march of novel but imprecise conceptual terminology by asking us to be more exact in the operational definition of terms, and by being more open to using a wider range of methodologies, and to combine them more imaginatively in any particular study.

In Part 2, *Thinking and Acting Strategically*, we move on to consider some of the core themes evident in strategy research. Here we juxtapose the words thinking and acting to emphasize their essential duality and dynamic character, whilst recognizing that historically many fields of strategy have divided these terms in simple dichotomies such as formulation and implementation, or content and process. Karel Cool, Luis Almeida Costa and Ingemar Dierickx review research and writing in one of the central questions of strategy research – what are the sources of a firm's competitive advantage? In discussing this core question they juxtapose and interrogate the complementary approaches of the currently fashionable resource based view of the firm and the market-position view of competitive advantage long established in industrial economics. Crucially, Cool, Costa and Dierickx pose their question not just across two levels of analysis – the firm and the market – but also over time. How can firm specific resources and/or privileged market positions be accumulated and sustained over the medium and long term?

Cool, Costa and Dierickx provide many useful analytical categories and mechanisms to discuss the sustainability of unique resources and privileged market positions. For the protection and development of internal firm resources issues of 'perfectly immobile' and 'imperfectly mobile resources' stand alongside more familiar discussions in the literature about the imitability of scarce resources. Privileged market positions as a source of

competitive advantage are discussed through the making of 'strategic commitments' in products, processes and production capacity as well as through the proliferation of product varieties and absolute cost advantages. They note that in the resource-based view, conceptual language development has far outstretched the careful empirical analysis that might have underpinned the proliferating vocabulary. Future research on competitive advantage, Cool, Costa and Dierickx suggest, requires a blending of the complementary strengths of the industrial organization research on market interaction and competition with the strategic management research on firm specific resources. At this time such integrative research endeavours are limited only by the tendency of scholars to specialize in levels of analysis and the evident failure to develop theories and research methodologies which comfortably cross different perspectives and levels of analysis.

One of the longest and deepest traditions in strategy research is concerned with the scope and content of strategy. With admirable clarity, Robert Grant's Chapter 4 first defines the central questions of corporate strategy, then draws a map of the key literature, reviews the quality of empirical work and subsequent theoretical developments, and then points to the content and direction of future research. Grant helps to focus reader attention by identifying the three main questions pursued by scholars interested in corporate strategy. These are:

- 1 What determines the scope of a firm's business, how diversified or specialized should it be?
- 2 What is the linkage between scope and firm performance? and
- 3 What do we know about the management of multi-business firms in terms of structure, management systems and leadership?

In Chapter 4, Grant pursues the first two of these questions. Constantinos Markides addresses the third question in Chapter 5.

Grant critically addresses the well-established work in strategy on growth and diversification. Importantly, he does this against the backcloth of the changing economic and political context in the period 1960–1980, and recognizing the varying experiences of large firms in North America and Europe compared with those in emerging-market economies.

Strangely, research on growth and diversification persisted long after firms had had to adjust their broad goals and incorporate pressures towards risk reduction, survival and profitability (see also Webb and Pettigrew, 1999). Grant saves his sharpest scalpel for research seeking to link diversification to firm performance by arguing that more than 100 academic studies have failed to determine if diversification enhances profitability or whether related diversification outperforms unrelated diversification. Interestingly, Grant contends that a tradition of empirical inconsistencies may be a spur for theoretical innovations. He notes that research on corporate strategy has been greatly assisted by parallel theoretical developments in transaction costs economics, the resource based theory, agency theory and financial theory. Like Cool, Costa and Dierickx, Grant sees the way forward in posing the more sophisticated (and in this case) practically relevant questions which arise from work at the boundaries of strategy and organization. Grant is particularly encouraging of future research on corporate strategy which looks both to the characteristics of the resources and capabilities that underlie corporate strategy and to the organizational structures and mechanisms that implement it.

Constantinos Markides addresses the related and narrower question in corporate strategy: What is the role of the centre in a multi-business firm? He then breaks this broad question into its descriptive form: What does the centre do? And its prescriptive form: What should the centre do? Both questions, he argues, have generated theoretically uninformed laundry lists of various sizes and perceptivity. Markides then contends that theoretical guidance may be offered by re-stating the descriptive and prescriptive questions as: What is the economic rationale for the multi-business firm? He answers this question by drawing on literature on the exploitation of economies of scope and the creation of efficient internal capital markets. These two economics-based answers are then complemented by an answer from management and organization theory. The multi-business firm has the potential to create the efficient sharing and transfer of core competences across divisions so that the divisions can accommodate new strategic assets more quickly and cheaply than competitors. Markides' guidance for future research is built

around four themes which bear upon the strategy, structure, processes and leadership of the multi-business firm. He is particularly interested in the degree of freedom claimed and negotiated by different divisions within the same firm and the implications of such a differentiated approach for the leadership and performance of such firms.

After two chapters on corporate strategy which productivity bridged into the field of corporate structure, Richard Whittington takes us firmly into the deep tradition of research and writing on corporate structure. Whittington argues that over the last 40 years structure has maintained its interest and significance to practitioners and consultants as a key area of managerial choice, but as a field of research it declined in importance after the heyday of the 1960s and 1970s. More recently, a new research agenda has emerged amongst scholars which is altogether more contextual, dynamic, holistic and practical than 'the general and lifeless reifications' that previously had dominated debates on structure.

Whittington organizes his statement of the evolving theoretical and empirical perspectives on structure using the categories of policy, proxy, periphery and practice. The foundational work which examined appropriate structures for diversification and internationalization he labels as policy work. Whittington defines proxy studies where the structure becomes a proxy variable in other debates, for example, in the literature on strategic choice. The periphery option is when structure was effectively marginalized in studies of organizational culture, change and control. Whittington argues that it is in structure as practice that a more holistic treatment of the subject, drawing on elements of the policy, proxy and periphery traditions, may yet emerge. The practical perspective would start from the apparently mundane but largely unstudied territory of the choices and changes made by practitioners of structure: what makes structure work? But this general question could be located within any of the other emerging streams of structural investigation which treated organization and organizing as the interdependencies between strategy, structure, processes, systems and people. Thus holistic work in the policy tradition is now attempting to define and discover innovative forms of organizing in the post-industrial

knowledge economy. Some of this work is using large scale international surveys to compare and contrast the differential pace of change across different dimensions of organizing in different countries and regions whilst also incorporating contextually sensitive case study work to discover the practitioners of structure in action (Pettigrew and Fenton, 2000; Pettigrew et al., 2002).

In Chapter 7, we move from corporate structure, one of the oldest areas of inquiry in strategy, to knowledge and knowing, one of the newest. Kathleen Eisenhardt and Filipe Santos pose some tough questions about the so-called knowledge based view (KBV) of strategy and organization. Is the KBV a passing fad? Is it a new theory of strategy or a new theory of organization, or neither? What is the evidence, if any, for the oft-repeated assertion that knowledge is a source of competitive advantage?

Eisenhardt and Santos draw on North American, European and Japanese scholarship, and the diverse epistemological and theoretical assumptions they entail, to underpin an appropriately balanced yet sceptical treatment of the theme of knowledge, knowing and strategy. In their review of the limited empirical work on knowledge and strategy they examine research findings on knowledge processes of sourcing, internal transfer, external transfer and integration. In conclusion, Eisenhardt and Santos assume that the knowledge-based theory is neither a new theory of strategy or a new theory of organization, and that the evidence to link knowledge resources to competitive advantage is questionable. They suggest three important areas for further research. Firstly, there is an important need for conceptual ground-clearing to produce a more consistent and coherent language to guide research. Secondly, they call for more bridging between static and reified treatments of knowledge and the more processual and contextually sensitive exploration of knowing now evident in European scholarship. Finally, they contend that the KBV would usefully develop alongside insights now available in various branches of social psychology, sociology and evolutionary biology. The knowledge area is one of many instances in the book where a reciprocal relationship between developments in the social sciences and management can underpin real intellectual progress in the broad field of strategy and management.

Historically, economics has been the dominating perspective in the development of strategic management. Latterly, through their influence in organization theory, sociological and political concepts have also become influential. Only recently have psychological ideas risen in significance. The chapter by Joseph Porac and Howard Thomas on cognition and strategy brings those psychological ideas to the fore. Porac and Thomas discuss the literature on cognition and strategy around three central questions: What are cognitive structures? How do such structures develop and with what measurable consequences? Their focus is on the upper echelons of the firm where the assumption is that the beliefs and mental maps of key players are likely to have the biggest impact on processes of strategic choice and change. Their review appropriately crosses levels of analysis considering in turn the existence, causes and consequences of strategic cognitive structures at the individual, top management team, organization and population of top management teams in industries. Their chapter concludes by providing an agenda for future research on managing cognition and strategy.

Chapters 9 and 10 comprehensively document and assess research on the linked areas of strategy process and strategic change processes. Bala Chakravarthy and Roderick White focus on one of the central questions in the theory and practice of strategy: how are strategies formed, implemented and changed? They frame their review by making a number of strong assertions which they then return to in their concluding comments about the future direction of research on strategy process. They insist that strategy process cannot be studied in isolation from the content of strategy and the outcomes of strategy making. Theorizing about strategy process, they argue, has been partial in particular studies, with authors favouring one or other of the various theoretical lenses (boundedly rational, emergent, political, or incremental). They call for future approaches to attempt more holistic theorizing and to embed their conceptual approach in studies which combine a long time series, multiple levels of analysis and expose process variation in comparative analysis. Such empirical studies are likely to demand high levels of resourcing with teams of researchers being kept together for long enough to build a

truly cumulative set of findings. They note the existence and impact of such teams in North America and Europe and suggest that further scale commitments of that sort may be the more appropriate stepping stone to building research of scholarly quality and practical relevance on how strategies are made and executed.

Research on strategic change processes has perhaps been misguidedly equated with the broader category of work on strategy process. Change processes are now regarded as at the centre of the strategy field, but are only one of the key elements in strategy process research. Raghu Garud and Andrew Van de Ven review existing studies of strategic change process against the backcloth of four broad social science theories of changing: teleological, life cycle, dialectical and evolutionary. They then point to the strengths and limitations of each theoretical lens and suggest that the character of change in contemporary organizations now demands the increased explanatory power evident in non-linear dynamics. Like Chakravarthy and White, they focus their observations about future research on strategic change processes more on the character and style of future research than its content. They recommend, however, that the central question of strategic change process research should switch from a concern with the antecedents or consequences of strategic changes to the analysis of how strategic change processes emerge, develop, grow or terminate over time.

Scholarly and policy attention to issues of comparative governance has flowered in the 1990s, and no handbook of strategy could be complete without acknowledging the important developments which have taken place. Gerald Davis and Michael Useem introduce us to the pluri-disciplinary work on top management, company directors and corporate control. This research is theoretically eclectic, drawing on theories from financial economics, agency theory, sociology, political sciences and a variety of traditions in management studies. Davis and Useem note how work in the governance area has crossed multiple levels of analysis. Over time the central questions have broadened from narrow issues of board composition and control – Is it better for shareholders for a corporation's board to have more 'outside' directors? – to the characteristic question of the early 21st century: What ensemble of institutions best situates a nation

for economic growth in a post-industrial information-based economy? Thus research on corporate governance has come to span levels of analysis from within the organization to the nation-state and beyond.

Respecting the breadth of work in the field in the US and beyond, Davis and Useem organize their extended review around a number of core questions: One capitalism or many? Who are top managers and company directors? How do shareholders and other stakeholders influence the corporation? Do top managers really make a difference? How do corporations shape society? Is a worldwide model for top management and corporate governance emerging?

They conclude that research in the corporate governance area will be energized by work which clarifies what governance arrangements and leadership styles work well both within national settings and across cultural divides. They recommend further research in five areas: boards and directors, corporate governance, financial globalization, inequality in a globalized world and business and political leadership.

Bruce Kogut introduces us to the field of international strategy with its interests in the international activities of firms and their interaction with foreign governments, competitors and employers. Kogut also uses the device of posing and answering key researchable questions to structure his presentation of the highlights in past and present research and what is known and not known about international management and strategy. Historically, the major question in this field has been 'why do firms go overseas and how and when do they do it?'. This broad question has opened up a plethora of sub-fields of research concerned with foreign entry modes, the strategies, structures and systems of multinationals, the factors which shape location decisions, and the increasing importance of new forms of organizing and new technologies in shaping the possibilities and processes of international firm behaviour.

For future research Kogut identifies a range of possibilities, many of them influenced by a heightened interest in the way societal and institutional factors are creating differential responses of international firms to regional and national conditions. As research capabilities in management are being developed

throughout the world, and North American and European scholars are investing more in international comparative investigations, so variations in business practices, levels of technological investment and pace of change in implementing innovative forms of organizing are all being recognized. There are rich possibilities to examine how and why the internet economy may be bringing about fundamental changes in strategic thinking and action about location and place.

The major purpose of a handbook is to review, assess and develop existing themes in an intellectual terrain. However, it should have the licence to add to a map and not just reinforce what is there. We take two opportunities to re-configure the conventional map of strategic management. In Chapter 13 Ewan Ferlie signals the enormous potential of studying strategy in the changed conditions of the contemporary public sector. In Chapter 15 Winfrid Ruigrok begins the process of articulating the scholarly and policy agenda for strategy research on the myriad of international institutions which now shape the political, economic and industrial landscape around us.

By and large business and management schools throughout the world are private sector ghettos. This is surprising because as late as 1997 32% of GDP in the USA, 35% in Japan, 39% in the UK and 62% of GDP in Sweden was accounted for by government revenues. Irrespective of the intrinsic interest in strategic change in the contemporary public sector throughout the world, it seems somewhat perverse of business schools to virtually ignore 35% or more of their potential market. Ewan Ferlie argues the case for why the analysis of public sector organizations should be taken more seriously within the strategic management literature. Drawing on research in Europe, North America, Australia and New Zealand, Ferlie documents the widespread and profound re-structuring now underway in public services throughout the world. The rise of this new public management, he contends, is drawing the public sector context closer to management practices in the private sector (whilst also retaining important differences), thereby opening up the public sector to its own brand of strategic analysis. The rise of quasi markets in the public sector, increased pressure for performance, the managerialization of the public sector and the move from

maintenance management to the management of strategic change have all re-shaped the conduct and process of strategic management.

Ferlie proposes a research agenda to explore the extent of convergence between the public and private sectors. He asked penetrating questions about the extent to which politicians have ceded policy making to senior managers in the public sector and raises an important research theme about the evolving role of public sector professionals in the changing context for the delivery of public services. This is an area wide open for intellectual leadership for a new generation of scholars leaving doctoral programmes in strategic management.

Rita Gunther McGrath raises the flag of entrepreneurial research and signals its importance and overlap with research on strategic management. McGrath's chapter is premised on the assumption that the fields of strategy and entrepreneurship may be merging. She argues that the changing competitive context of large enterprises towards greater uncertainty, complexity and volatility mirrors well the conditions faced by those engaged in entrepreneurial action. Throughout Chapter 14, McGrath uses the metaphor of real options reasoning to explore some of the core entrepreneurial processes of option identification, formulation of a new business, growth development and profit, and business termination. These processes she explores across five levels of analysis: the individual entrepreneur, the network, the organization, the region, and the institutional context of entrepreneurial action. In each cell in her 4 by 5 matrix she identifies novel and interesting researchable questions which could frame a new research agenda at the boundary of strategy and entrepreneurship.

Since the Second World War, international institutions such as the European Union (EU), the International Monetary Fund (IMF) and the World Trade Organisation (WTO) have acquired a crucial role in the international, political and economic arena. With the increased interdependency of the world and the recognition and exploitation of nation state, regional and international interests, so such international institutions have acquired greater material and symbolic significance. But with this rise in visibility has come ever-rising expectations for policy delivery from such institutions. Many of these expectations lie unfulfilled. International institutions are

regularly criticized for their lack of transparency, accountability and for the gaps between their policy statements and their capacity to deliver on them.

Winfried Ruigrok's chapter on the strategy and management of international institutions raises the prospect of a new area of inquiry for strategy scholars and for stronger links to be made between strategic management researchers and colleagues in political science and international relations. Ruigrok notes that international institutions are notoriously difficult to manage. They rarely have strong and omnipotent chief executive officers or coherent executive teams who can define strategic direction. It is rare for any length of time for one nation state in such international bodies to impose their will. Diverse, conflicting and sometimes irreconcilable demands are often the order of the day. The challenges of strategy and management in international institutions are pervasive, real and under researched.

Ruigrok's chapter begins to prepare the ground for strategy research in this important area. In turn, he traces the evolution of international institutions and reviews the literature on domestic and international institutions developed by sociologists, political scientists, economists and management scholars. His research agenda for the future targets the effectiveness of international institutions. This will require a pluri-disciplinary approach combining the skills of strategy scholars with those of political scientists, sociologists and economists. Here, as in many other areas of this Handbook, our future lies not just in the concepts and methods of strategic management but in the even more pluri-disciplinary field of strategy and management.

At the beginning of the 21st century we cannot ignore the relationship between technology and strategy. Keith Pavitt and W. Edward Steinmueller draw on their extensive knowledge of technology in corporate strategy and in so doing place it at the heart of the future research agenda on strategy and management. They argue that the effective mobilization of technology for competitive advantage depends on the orchestration, integration and application of increasingly specialized knowledge from both inside and outside the firm. In spite of rapid improvements in underlying scientific understanding, they contend that corporate

innovation activities remain complex and uncertain in their outcomes. Rapid improvements in information and communication technologies (ICTs) will generate potentially revolutionary and disruptive changes in a number of corporate functions, including transactions, distribution, technological development and strategy. Yet the revolution will be incrementalist in nature, since experimentation based on past experiences (including past failures) will remain the norm. Key subjects requiring further research include the changing nature of professional networks, co-evolution of technology (including ICTs) and organizational practices and the management of change in increasingly complex systems.

Our final chapter in Part 3 bounded by the theme of 'changing contexts' embraces the relationship between business and society. David Whetten, Gordon Rands and Paul Godfrey argue that in spite of its long history and now contemporary visibility, the research and policy theme of business and society has developed somewhat in isolation from the more mainstream approaches to the study of strategy and management. Their chapter suggests that now is the time to bring business and society relationship into centre stage in the strategy field. Whetten, Rands and Godfrey offer an historical overview of business and society scholarship and then discuss future directions for research and study. Important areas of inquiry for the future include: the study of effective and/or moral practices of firms; issues of organizational reputation, image and identity; the links, if any, between firm ethical behaviour and competitive advantage; collaborative strategies between firms and other societal stakeholders; and the important role played by human resource strategies in firms on the choices and changes made in business and society relationships.

Part 4 looks forward through four lenses. In Chapter 18 Haridimos Tsoukas and Christian Knudsen look to the future through the twin lenses of philosophy and epistemology. David Jeremy consolidates the temporal and contextual agenda of the book by signalling the importance of history to the development of strategy. N. Venkatraman and Mohan Subramaniam suggest how and why the changing shape of the knowledge economy may require new forms of theorizing in strategy. The fourth lens is offered by the editors

who draw together many of the scholarly and policy threads and then articulate their own view about some of the future directions for the conduct of strategy research.

Earlier in this chapter the editors noted that one sign and symptom of a lack of maturity in the strategy field was the absence of critical reflection. Haridimos Tsoukas and Christian Knudsen provide some of this missing reflexivity by their critical examination of the ontological, epistemological and praxeological foundations of strategic management. Following their epistemological exploration of the theoretical foundations of strategy research, Tsoukas and Knudsen outline a meta-theoretical framework which allows them to interrogate the field's answer to two core questions: how is thinking related to action, and who sets strategy? Their thesis is that strategic management has been dominated by one particular mode of explanation (the covering law model) and one particular view of how thinking is related to action (representationalism) both of which have their problems. They argue that strategy research will become more relevant, encompassing, and subtle if it moves closer toward a process-oriented view of the firm and opens itself to a constructivist view of strategy making.

For many, Alfred Chandler and Douglas North represent the peak of recent scholarship in business and economic history. Although both have been much quoted and admired, and Chandler is widely regarded as one of the founders of strategy research, their success so far has not brought the investigative capabilities of the historian and the subtlety and power of historical explanation to the forefront of strategy research. Indeed, beyond the Harvard Business School it is difficult to identify a business school throughout the world which has sustained any form of teaching and research in the history of business in its economic, social and political context.

David Jeremy carries the flag of history most explicitly into this volume, although it is note-worthy that many other authors acknowledge the importance of history, and the historical perspective to the further development of strategy research (see especially Chapters 1, 6, 9, 13, 15, 16, 17 and 18). Recognizing that business history draws upon several disciplines, including history, economics and management, Jeremy articulates many of the key

requirements and stepping-stones to bring business history and strategy closer together. In turn, he marshals the evidence to reveal a history of business history, to explore the existing relationship between business history and strategy; to identify what business history has already contributed to thinking about strategy, and the value of historical methods for investigating strategy. He concludes by offering a wide agenda for future research in strategy demanding historical investigation.

The penultimate chapter encourages strategy research to look forward through the challenges of the knowledge-based economy. N. Venkatraman and Mohan Subramaniam place the changing context of the knowledge-based economy in historical perspective by arguing that thinking about strategy may be considered over three eras. First, when strategy was viewed as a portfolio of businesses, second, as a portfolio of capabilities, and third, as it is now surfacing, as a portfolio of relationships. They emphasize that throughout these three eras cumulative development has occurred with each era supplementing rather than supplanting earlier periods of thinking.

Venkatraman and Subramaniam argue that each of the three eras has had a distinct paradigmatic focus. The portfolio era was associated with economies of scale, the capabilities era with economies of scope, and the relationship era, economies of expertise. Theorizing about strategy in the current expertise era they see bounded and enabled by four key questions: How do we conceptualize economies of expertise? Do we need a new unit of analysis for theorizing about strategy? Does economies of expertise lead to differential performance and do we need new organizing principles in this latest era of relationships and expertise?

Finally, we conclude with a thematic overview. This is not a search for uniformity, which is neither available or desirable. However, the editors pick up some important threads and overlay them with a number of personal observations about the future direction of research on strategy and management. We take the view that strategy research may be at the point of breaking free from the constraints of its origins in the modernist social sciences of the mid 20th century. After modernism, research in strategy will search less for universal laws, give more due to temporal and spatial context; it will admit the possibility of

holistic analysis; and where once it fixed on the static and detached, now it will seek change and action. In all these ways the new strategy research will move beyond simple reliance on scientific abstraction and at last meet the double hurdles implied in seeking scholarly quality and relevance.

REFERENCES

- Abrahamson, E. (1991) 'Managerial fads and fashions', *Academy of Management Review*, 16: 586–612.
- Abrahamson, E. (1996) 'Management fashion', *Academy of Management Review*, 21(1): 254–85.
- Aldrich, H. E. (1999) *Organizations Evolving*. London: Sage.
- Alvesson, M. and Willmott, H. (1995) 'Strategic management and domination and emancipation: from planning and process to communication and praxis', in P. Shrivastava and C. Stubbart (eds), *Advances in Strategic Management: Challenges from Outside the Mainstream*. Greenwich, Conn: JAI Press. pp. 85–112.
- Andrews, K. R. (1971) *The Concept of Corporate Strategy*. New York: Dow Jones-Irwin Inc.
- Ansoff, H. I. (1965) *Corporate Strategy*. Homewood, Illinois: Dow Jones-Irwin.
- Ansoff, H. I. (1991) 'Critique of Henry Mintzberg's The Design School: reconsidering the basic premises of strategic management', *Strategic Management Journal*, 12: 449–61.
- Barnett, W. P. and Burgelman, R. A. (1996) 'Evolutionary perspectives on strategy', *Strategic Management Journal*, 17 (Special Issue): 5–19.
- Barney, J. B. (1991) 'Firm resources and sustained competitive advantage', *Journal of Management*, 17: 99–120.
- Barney, J. B. (1992) 'Integrating organizational behaviour and strategy formulation research: a resource based analysis', in P. Shrivastava, A. Huff and J. Dutton (eds), *Advances in Strategic Management*. Greenwich, Conn. JAI Press, Vol. 8: pp. 39–61.
- Baum, J. A. C. and Dobbin, F. (eds) (2000) 'Economics meets sociology in strategic management', *Advances in Strategic Management*, volume 17. Greenwich, Conn: JAI Press.
- Beinhocker, E. D. (2000) 'Strategy at the edge of chaos', *The McKinsey Quarterly*, Anthology on Strategy, June: 109–18.
- Bettis, R. A. (1998) 'Commentary on Redefining Industry Structure for the Information "Age" by J. L. S. Sampler', *Strategic Management Journal*, 19 (Special Issue): 357–61.
- Bourgeois, L. J. (1984) 'Strategic management and determinism', *Academy of Management Review*, 9(4): 586–96.
- Bowman, G. H. (1974) 'Epistemology, corporate strategy and academe', *Sloan Management Review*, 23(4): 33–42.
- Bowman, G. H. (1990) 'Strategy changes: possible worlds and actual minds', in J. W. Fredrickson (ed.), *Perspectives on Strategic Management*. New York: Harper Business. pp. 9–37.
- Burt, R. S. (1992) *Structural Holes: The Social Structures of Competition*. Cambridge, MA: Harvard University Press.
- Calori, R. (1998) 'Essai: philosophizing in strategic management', *Organization Studies*, 19(2): 281–306.
- Camerer, C. F. (1985) 'Redirecting research in business policy and strategy', *Strategic Management Journal*, 6: 1–15.
- Camerer, C. F. (1991) 'Does strategy research need game theory?', *Strategic Management Journal*, Special Issue, 12: 137–52.
- Cannella, A. A. and Paetzold, R. L. (1994) 'Pfeffer's barriers to the advance of organizational science, a rejoinder', *Academy of Management Review*, 19(2): 331–41.
- Chakravarthy, B. S. and Doz, Y. (1992) 'Strategy process research: focussing on corporate self-renewal', *Strategic Management Journal*, 13, Special Issue: 5–14.
- Chandler, A. R. (1962) *Strategy and Structure: Chapters in the History of the Industrial Enterprise*. Cambridge, Mass: MIT Press.
- Chandler, A. D. (1990) *Scale and Scope: The Dynamics of Industrial Capitalism*. Cambridge, Mass: Harvard University Press.
- Church, R. (1993) 'The family firm in industrial capitalism', *Business History*, 35: 17–43.
- Clark, P. (2000) *Organisations in Action: Competition between Contexts*. London: Routledge.
- Courtney, H. G., Kirkland, J. and Viguier, S. P. (2000) 'Strategy under uncertainty', *The McKinsey Quarterly*, Anthology on Strategy, June: 81–90.
- Coyne, K. P. and Subramanian, S. (2000) 'Bringing discipline to strategy', *The McKinsey Quarterly*, Anthology on Strategy, June: 61–70.
- Cummings, S. and Wilson, D. C. (eds) (In Press) *Images of Strategy*. Oxford: Blackwell.
- D'Aveni, R. (1994) *Hypercompetition*. New York: Free Press.
- Dobbin, F. and Baum, J. A. C. (2000) 'Introduction: economics meets sociology in strategic management' in J. A. C. Baum and F. Dobbin (eds), *Advances in Strategic Management*, Greenwich, Conn: JAI Press.
- Ehrensell, P. A. L. (1999) 'Calling the Shots: How Elite Discourses Constrain Local Strategic Actors', *Critical Management Studies Conference (Strategy Stream)*, Manchester, UK, July 14–16.
- Eisenhardt, K. and Brown, S. L. (1992) 'Commentary on Mahoney, J. T. "Organizational Economics Within the Conversation of Strategic Management"', *Advances in Strategic Management*, 8: 157–62 Greenwich, Conn: JAI Press.
- Fama, G. F. and Jensen M. C. (1983) 'Separation of ownership and control', *Journal of Law and Economics*, 26: 327–49.
- Fenton, E. M. and Pettigrew, A. M. (2000) 'Theoretical perspectives on new forms of organizing', in

- A. M. Pettigrew and E. Fenton (eds), *The Innovating Organization*. London: Sage. pp. 1–46.
- Ferlie, E., Ashburner, L., FitzGerald, L. and Pettigrew, A. M. (1996) *The New Public Management in Action*. Oxford: Oxford University Press.
- Foss, N. J. (1996) 'Research in strategy, economics and Michael Porter', *Journal of Management Studies*, 33(1): 1–24.
- Foucault, M. (1966) *The Order of Things: An Archeology of the Humanities*. London: Tavistock/Routledge.
- Gibbons, M., Limoges, C., Notwotny, H., Scharman, S., Scott, P. and Trow, M. (1994) *The New Production of Knowledge*. London: Sage.
- Giddens, A. (1979) *Central Problems in Social Theory*. London: Macmillan.
- Grant, R. M. (1991) 'The resource-based theory of competition advantage', *California Management Review*, 33(3): 114–35.
- Gulati, R., Nohria, N. and Zaheer, A. (2000) 'Strategic networks', *Strategic Management Journal*, 21, Special Issue: 203–16.
- Hagel, J. (2000) 'Spider versus spider', *The McKinsey Quarterly*, Anthology on Strategy, June: 71–80.
- Hagel, J. and Singer, M. (2000) 'Unbundling the corporation', *The McKinsey Quarterly*, Anthology on Strategy, June: 147–56.
- Hamel, G. and Prahalad, C. K. (1994) *Competing for the Future*. Boston, Mass: Harvard Business School Press.
- Hannah, L. (1995) 'The American miracle, 1875–1950, and after: a view in the European mirror', *Business and Economic History*, 24(2).
- Hannah, L. (1999) 'Marshall's "trees" and the global "forest": were "giant redwoods" different?', in R. Naomi, D. Lamoreaux, M. G. Raff and P. Temin (eds), *Learning by Doing in Markets, Firms, and Countries*. Chicago: University of Chicago Press.
- Harfield, T. (1999) 'Strategic management and Michael Porter: a postmodern reading', *Critical Management Studies Conference (Strategy Stream)*, Manchester, UK, July 14–16.
- Hendry, J. (1992) 'Business strategy and business history: a framework for development', *Advances in Strategic Management*, 8: 207–25 Greenwich, Conn: JAI Press.
- Hesterly, W. S. and Zenger, T. R. (1993) 'The myth of monolithic economics: fundamental assumptions and the use of economic models in policy and strategy research', *Organization Science*, 4(3): 496–510.
- Hirsch, P., Michaels, S. and Friedman, R. (1987) 'Is sociology in danger of being seduced by economics?', *Theory and Society*, 16: 317–38.
- Hitt, M. A., Gimeno, J. and Hoskisson, R. E. (1998) 'Current and future research methods in strategic management', *Organizational Research Methods*, 1(1): 6–44.
- Hoskisson, R. E. (1987) 'Multidivisional structure and performance: the contingency of diversification strategy', *Academy of Management Journal*, 30: 625–44.
- Hoskisson, R. E. and Hitt, M. A. (1990) 'Antecedents and performance outcomes of diversification: a review and critique of theoretical perspectives', *Journal of Management*, 16: 461–509.
- Hoskisson, R. E., Hitt, M. A., Wan, W. P. and Yin, D. (1999) 'Theory and research in strategic management: swings of a pendulum', *Journal of Management*, 25(3): 417–56.
- Johnson, G. (1987) *Strategic Change and the Management Process*. Oxford: Basil Blackwell.
- Knights, D. (1992) 'Changing spaces: the disruptive impact of a new epistemological location for the study of management', *Academy of Management Review*, 17(3): 514–36.
- Knights, D. and Morgan, G. (1991) 'Strategic discourse and subjectivity: towards a critical analysis of corporate strategy in organizations', *Organization Studies*, 12(2): 251–73.
- Kogut, B. and Zander, H. (1992) 'Knowledge of the firm, combinative capabilities and the replication of technology', *Organization Science*, 3: 383–97.
- Kuhn, T. (1970) *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
- Lowendahl, B. and Revang, D. (1998) 'Challenges to existing strategy theory in a post industrial society', *Strategic Management Journal*, 19(8): 755–74.
- McKiernan, P. (ed.) (1996) *Historical Evolution of Strategic Management*, Volume I. Aldershot: Dartmouth.
- McKiernan, P. (ed.) (1996) *Historical Evolution of Strategic Management*, Volume II. Aldershot: Dartmouth.
- McNulty, T. and Pettigrew, A. M. (1998) 'Strategists on the board', *Organization Studies*, 20(1): 47–74.
- Mahoney, J. T. (1992) 'Organizational economics within the conversation of strategic management', *Advances in Strategic Management*, 8: 103–55.
- March, J. G. (1991) 'Exploration and exploitation in organizational learning', *Organization Science*, 2: 71–87.
- March, J. G. (1996) 'Continuity and change in theories of organizational action', *Administrative Science Quarterly*, 41: 278–87.
- Miles, R. M. and Cameron, K. S. (1982) *Coffin Nails and Corporate Strategies*. Englewood Cliffs: Prentice Hall.
- Mintzberg, H. (1973) *The Nature of Managerial Work*. New York: Harper & Row.
- Mintzberg, H. (1978) 'Patterns in strategy formation', *Management Science*, 24: 934–48.
- Mintzberg, H. (1990) 'The design school: reconsidering the basic premise of strategic management', *Strategic Management Journal*, 11: 171–96.
- Mintzberg, H. (1994) *The Rise and Fall of Strategic Planning*. New York: Free Press.
- Mintzberg, H. and Walters, J. A. (1985) 'Of strategies, deliberate and emergent', *Strategic Management Journal*, 6(3): 257–72.
- Mintzberg, H., Raisinighani, D. and Theoret, A. (1976) 'The structure of unstructured decision processes', *Administrative Science Quarterly*, 21: 246–75.
- Mintzberg, H., Ahlstrand, B. and Lampel, J. (1998) *Strategic Safari: A Guided Tour Through the Wilds of Strategic Management*. New York: The Free Press.

- Nalebuff, B. and Brandenburger, A. M. (1997) *Co-opetition*. London: Harper Collins Business.
- Nelson, R. and Winter, S. (1982) *An Evolutionary Theory of Economic Change*. Cambridge, MA: Belknap.
- Nonaka, I. and Takeuchi (1995) *The Knowledge-Creating Company: How Japanese Companies Create the Dynamics of Innovation*. Oxford: Oxford University Press.
- Oliver, C. (1997) 'Sustainable competitive advantage: combining institutional and resource-based views', *Strategic Management Journal*, 18(9): 697–713.
- Penrose, E. T. (1959) *The Theory of the Growth of the Firm*. New York: John Wiley.
- Perrow, C. (1986) 'Economic theories of organization', *Theory and Society*, 15: 11–45.
- Pettigrew, A. M. (1973) *The Politics of Organizational Decision Making*. London: Tavistock Publications.
- Pettigrew, A. M. (1977) 'Strategy formulation as a political process', *International Studies of Management and Organization*, 7(2), Summer 1977: 78–87.
- Pettigrew, A. M. (1979) 'On studying organizational cultures', *Administrative Science Quarterly*, 24(4): 570–81.
- Pettigrew, A. M. (1985) *The Awakening Giant: Continuity and Change in ICI*. Oxford: Basil Blackwell.
- Pettigrew, A. M. (1987) 'Context and action in the transformation of the firm', *Journal of Management Studies*, 24(6): 649–69.
- Pettigrew, A. M. (1990) 'Longitudinal field research on change: theory and practice', *Organization Science*, 1(3): 267–92.
- Pettigrew, A. M. (1992) 'The character and significance of strategy process research', *Strategic Management Journal*, 13, Special Issue: 5–16.
- Pettigrew, A. M. (1997) 'What is processual analysis?', *Scandinavian Journal of Management*, 13(4): 337–48.
- Pettigrew, A. M. and Whipp, R. (1991) *Managing Change for Competitive Success*. Oxford: Blackwell.
- Pettigrew, A. M., Ferlie, E. and McKee, L. (1992) *Shaping Strategic Change*. London: Sage.
- Pettigrew, A. M. and Fenton, E. M. (eds) (2000) *The Innovating Organization*. London: Sage.
- Pettigrew, A. M., Massini, S. and Numagami, T. (2000) 'Innovative forms of organizing in Europe and Japan', *European Management Journal*, 18(3): 259–73.
- Pettigrew, A. M., Whittington, R., Melin, L., Ruigrok, W., Sanchez-Runde, C. and Van den Bosch, F. (eds) (2002) *Innovative Forms of Organizing: Complementarities and Dualities*. London: Sage.
- Pfeffer, J. (1993) 'Barriers to the advance of organization science: paradigm development as a dependent variable', *Academy of Management Review*, 18: 599–620.
- Porter, M. E. (1980) *Competitive Strategy*. New York: Free Press.
- Porter, M. E. (1985) *Competitive Advantage*. New York: Free Press.
- Porter, M. E. (1990) *Competitive Advantage of Nations*. New York: Free Press.
- Prahalad, C. K. and Hamel, G. (1990) 'The core competence of the corporation', *Harvard Business Review*, 68(3): 79–91.
- Prahalad, C. K. and Hamel, G. (1994) 'Strategy as a field of study: why search for a new paradigm?', *Strategic Management Journal*, Special Issue, 15: 5–16.
- Rumelt, R. P., Schendel, D. and Teece, D. J. (1991) 'Strategic management and economics', *Strategic Management Journal*, 12: Special Issue: 5–29.
- Rumelt, R. P., Schendel, D. and Teece, D. J. (eds) (1994) *Fundamental Issues in Strategy: A Research Agenda*. Boston, MA: Harvard Business School Press.
- Saloner, G. (1991) 'Modeling, game theory and strategic management', *Strategic Management Journal*, Special Issue, 12: 119–36.
- Sanchez, R. and Heene, A. (1997) 'Reinventing strategic management: new theory and practice for competence-based competition', *European Management Journal*, 15(3): 303–17.
- Schendel, D. (1994) 'Introduction to the Summer 1994 Special Issue – strategy: search for new paradigms', *Strategic Management Journal*, Special Issue, 15: 1–5.
- Schendel, D. and Hatten, K. J. (1972) 'Business policy or strategic management: a broader view for an emerging discipline', *Academy of Management Proceedings*, 99–102.
- Schendel, D. and Hofer, C. W. (1979) *Strategic Management: A New View of Business Policy and Planning*. Boston: Little Brown & Co.
- Schwenk, R. R. and Dalton, D. R. (1991) 'The changing shape of strategic management research', *Advances in Strategic Management*, 7: 277–300.
- Scott, J. (1991) 'Networks of corporate power: a comparative assessment', *Annual Review of Sociology*, 17: 181–203.
- Seth, A. and Thomas, H. (1994) 'Theories of the firm: implications for strategy research', *Journal of Management Studies*, 31(2): 165–91.
- Shrivastava, P. (1986) 'Is strategic management ideological?', *Journal of Management Studies*, 12(3): 363–77.
- Shrivastava, P. and Stubbart, C. (eds) (1995) *Advances in Strategic Management: Challenge from Outside the Mainstream*. Greenwich, Conn: JAI Press.
- Smith, C., Child, J. and Rowlinson, M. (1990) *Reshaping Work: The Cadbury Experience*. Cambridge: Cambridge University Press.
- Supple, B. (1991) 'Scale and scope: Alfred Chandler and the dynamics of industrial capitalism!', *Economic History Review*, 44: 500–14.
- Sztompka, P. (1991) *Society in Action: The Theory of Social Becoming*. Chicago: University of Chicago Press.
- Teece, D. J., Pisano, G. and Shullen, A. (1997) 'Dynamic capabilities and strategic management', *Strategic Management Journal*, 18(7): 509–33.
- Thomas, P. (1999) 'Ideology and the discourse of strategic management: a critical research framework', *Critical Management Studies Conference (Strategy Stream)*, Manchester, UK: July 14–16.

- Tsoukas, H. (1994) 'Refining common-sense: types of knowledge in management', *Journal of Management Studies*, 31(6): 761–80.
- Tsoukas, H. and Cummings, S. (1997) 'Marginalization and recovery: the emergence of Aristotelian themes in organization studies', *Organization Studies*, 18(4): 655–83.
- Van Maanem, J. (1994) 'Style and theory', *Organization Science*, 6: 2–12.
- Von Tunzelman, G. N. (1999) 'Big business, growth and decline: review article', *The Journal of Economic History*, 59(3): 787–94.
- Wack, P. (1985) 'Scenarios, uncharted waters ahead', *Harvard Business Review*, Sept–Oct: 73–90.
- Webb, D. and Pettigrew, A. M. (1999) 'The temporal development of strategy: patterns in the UK insurance industry', *Organization Science*, 10, 5, 601–21.
- Wernefelt, B. (1984) 'A resource based theory of the firm', *Strategic Management Journal*, 5: 171–80.
- Whipp, R. (1996) 'Creative deconstruction: strategy and organization', in S. R. Clegg, C. Hardy and W. R. Nord (eds), *Handbook of Organization Studies*. London: Sage. pp. 261–75.
- Whitley, R. (1999) *Divergent Capitalisms*. Oxford: Oxford University Press.
- Whittington, R. (1993) *What is Strategy, and Does it Matter?* London: Routledge.
- Whittington, R. (2001) 'The practice of strategy: a European agenda', paper presented to the *European Academy of Management Conference*, IESE Barcelona, April 20–21.
- Whittington, R., Pettigrew, A. M. and Melin, L. (1999) 'Organizing/strategizing', paper presented to *Academy of Management Conference*, Chicago, August 1999.
- Whittington, R., Pettigrew, A. M., Peck, S., Fenton, E. M. and Conyon, M. (1999) 'Change and complementarities in the new competitive landscape: a European panel study 1992–1996', *Organization Science*, 10(5): 583–600.
- Whittington, R. and Mayer, M. (2000) *The European Corporation: Strategy, Structure and Social Science*. Oxford: Oxford University Press.
- Williamson, O. E. (1975) *Markets and Hierarchies: Analysis and Antitrust Implications*. New York: Collier Macmillan.
- Williamson, O. E. (1985) *The Economic Institutions of Capitalism*. New York: Free Press.
- Zajac, E. (1992) 'Relating economic and behavioural perspectives in strategy research', *Advances in Strategic Management*, 8: 69–96, Greenwich, Conn: JAI Press.
- Zajac, E. and Westphal, J. D. (1996) 'Who should succeed? How CEO/board preferences and power affect the choice of new CEOs', *Academy of Management Journal*, 39: 64–90.
- Zajac, E. and Westphal, J. D. (1998) 'Toward a behavioural theory of the CEO–board relationship', in D. C. Hambrick, D. A. Nadler and M. C. Tushman (eds), *Navigating Change: How CEO's, Top Teams, and Boards Steer Transformation*. Boston: Harvard Business School Press. pp. 256–77.
- Ziman, J. (1994) *Prometheus Bound: Science in a Dynamic Study State*. Cambridge: Cambridge University Press.