

CHAPTER 5

BUSINESS HISTORY AND MANAGEMENT STUDIES

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5.1 INTRODUCTION

THE purpose of this chapter is to give an overview of the changing relationship between business history and management studies since the 1950s, with a particular focus on the contributions made by business historians to management research. We also assess the potential for future collaboration among scholars from the two fields. Our main argument is that while both were close at the beginning of the period, they subsequently moved apart. As we will show, it was particularly Chandler's (1962) book *Strategy and Structure* that had a profound and lasting influence on research in management or administration as it was known at the time. But what looked like a promising start never developed into a more fully fledged interaction. Management studies moved in the direction of "scientization",

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especially in North America, while much of business history remained rooted in its own legacy of (narrative) history, economic history, and economics. This divergence is mirrored in the current institutional set-up of both fields. Most business historians are working in history or, less frequently, economic history departments rather than business schools or management departments. Business historians have their own academic associations, which are not affiliated with the major learned societies in the management field, such as the Academy of Management. The same is true for publications in academic journals, where there has been little crossover.

Within this framework, there have been some exceptions. Business historians have continued to contribute to research in a few sub-fields outside the core specialties within the broad field of management studies. These sub-fields have primarily been International Business and, perhaps most notably, what might be termed Management History, addressing issues such as the development of production systems from Taylorism and Fordism to flexible specialization, the "Americanization" of European and Japanese businesses after World War II, or the history of management education and management consulting. We will detail these contributions—and their limitations—in the body of this chapter. In terms of institutions, the most prominent exception is the Harvard Business School (HBS), which has had a Chair in Business History since the 1920s. Chandler spent the later part of his career there and since then HBS has continued to attract a number of well-known and well-respected business historians. The phenomenon is somewhat more widespread in Europe, where business historians are affiliated with business schools or management departments at a larger number of institutions.

In general, the last ten years have seen calls from both sides for a closer interaction between business history and management studies and some more pronounced attempts at dialogue and even cooperation. As we will argue in the final section of the chapter, these have yet to reach the central areas of management studies—strategy and organization—which would finally bring business history back to its auspicious beginnings.

5.2 CLOSE ORIGINS: CHANDLER'S CONTRIBUTION TO MANAGEMENT STUDIES

There are few business historians, if any, who left as important a mark on management practitioners and management scholars as Alfred Chandler. His work on the development of the multidivisional form of organization (Chandler 1962)

and, to a lesser extent, on the emergence of the large-scale vertically integrated enterprise (Chandler 1977) are still seen as pioneering efforts in the management and in particular the strategy literature (e.g. Whittington 2001; Ghemawat 2002; Jeremy 2002; Micklethwait and Wooldridge 2003). Chandler was not the first to study the large-scale diversified manufacturing enterprises which had become a dominant feature of the American economy at the time—something for which Drucker's (1946) *Concept of the Corporation* probably deserves the credit. Neither did he discover the importance of managers compared to owners and entrepreneurs—a phenomenon already examined by a variety of earlier studies, including Berle and Means (1932), Burnham (1941), Schumpeter (1942), and Penrose (1959).

But in his book on *Strategy and Structure*, he explicitly aimed to make a historical contribution to the study of large-scale firms: "Historians have provided social scientists with little empirical data on which to base generalizations or hypotheses concerning the administration of great enterprises. Nor have the historians formulated many theories or generalizations of their own" (Chandler 1962: 1). Based on a survey of the largest industrial firms in the United States and their organizational structure, Chandler selected four companies that had—independently of each other—pioneered a multidivisional or decentralized form of organization in the 1910s and 1920s (DuPont, General Motors, Standard Oil, and Sears Roebuck) and studied them in detail. He investigated the internal conditions that led to this organizational innovation (prior administrative history and growth patterns) and the context in which the organizational changes took place (changes in the overall market demand for their products and "the state of the administrative art in the United States at the time") (ibid. 3). From the in-depth comparative analysis of the four cases, he deduced his major thesis "that structure follows strategy": companies change their organization to meet the administrative demands created by their different types of—planned—growth, for example an expansion of volume, geographical dispersion, or a diversification of product lines (ibid. 14–15).

For management scholars, *Strategy and Structure* remains one of the "classic" case studies, widely seen as exemplary for comparative, theory-building management research (e.g. Eisenhardt 1991). And it has even earned him the status of a management "guru", as one of the writers who, according to Micklethwait and Wooldridge (1996: 142), "spent the 1950s and 1960s insisting that all companies needed a corporate strategy"—which was actually quite far from being his primary concern, if it was a concern at all. More specifically, in conceptual terms his work on the development of the M-form is still regarded as one of the prime footings in the evolution of the field of strategic management and the pioneer of the processual approach to strategy (Bowman *et al.* 2002). His 1962 book has also been seen as one of the foundational studies in early attempts to develop contingency theories of organization and management (e.g. Kast and Rozenzweig 1970) and has since been

regarded as the basis for pointing to the significance of strategy as a contingency factor (e.g. Donaldson 1995). At the same time, his work has also been used as a justification for those perspectives that have questioned the deterministic and overly rationalistic versions of contingency theory and have claimed that the designs of organizations involve an element of choice (e.g. Hall 1987).

Moreover, from an empirical point of view, Chandler's study sparked a research program at the Harvard Business School in the late 1960s and early 1970s, when a group of doctoral students under the supervision of Bruce Scott—and with Chandler as a member of the doctoral committees—examined diversification and divisionalization in the largest 100 industrial firms in the United States (Wrigley 1970; Didrichsen 1972; Rumelt 1986), the United Kingdom (Channon 1973), France and Germany (Dyas and Thanheiser 1976), and Italy (Pavan 1976). Subsequently, Channon (1978) also studied the 100 largest service firms in Britain. Whittington and Mayer (2000: 12), who extended the research on British, French, and German firms into the 1990s, not only call this "the first systematic research program in the strategic management discipline", but also claim that "in its international scope, its historical perspective, and standardization of national data-bases, it still has few peers".

But what looked like a promising, mutually stimulating, and beneficial relationship between historians and other scholars of business and management remained exactly this—a promise. Neither Chandler's later research nor any of the subsequent studies and debates in the business history literature (related or not to his work) have had an influence on management research even mildly comparable to that of *Strategy and Structure*. What happened? Why did what seemed like a fruitful interaction during the 1960s not continue? The answer lies in the evolution of both fields and particularly the introduction of neo-positivist research methodologies in mainstream management studies.

5.3 MOVING APART: SCIENTIZATION IN MANAGEMENT RESEARCH

Flourishing after World War II as a separate discipline housed in business schools, predominantly in North America (Augier *et al.* 2005), the study of management and organizations has in a rather abrupt fashion turned away from history. This has been due to the scientization route that management studies took initially in the United States from the late 1950s onwards and the accompanying early penetration of disciplines such as psychology, social psychology, and sociology. The broad pattern that was set in motion in the field of management in the 1950s was epitomized by the founding of the *Administrative Science Quarterly* (ASQ)

in 1956 and of *Management Science* in 1954—though the latter moved and settled on a formalistic and technique-orientated route. Early articles in both of these journals attest to the quest and the aspirations at the time for building a “science of managing” or “administrative science” that would essentially be based upon the emulation of the natural science model. What research should look like in management was clearly spelled out, for example, by Delany (1960: 448–9). It had to meet, as much as possible, the “usual scientific canons of validity, reliability, generality, parsimony, explanatory power and usefulness for purposive control”. In stating the means for achieving these aims Delany was also clear about the role of historical research in the emerging science of administration or management (*ibid.* 449): “An emphasis upon current and immediately observable organizations in the interests of full and rigorous data. Historical research, while not ruled out, is given second-level priority and rigorous comparative studies substituted at the first-priority level.”

Scientization did not remain without its critics. Boddewyn (1965: 261), for example, explicitly recognized, as a part of what he saw as the problem of ignoring the international, that in US management studies there was “widespread disinterest in business history”. But such views remained a minority and the scientific orientation clearly triumphed, as Daft (1980) showed in his study of articles in the *ASQ* between 1959 and 1979. While almost all early contributions were qualitative studies, by the late 1970s the majority of articles employed some kind of quantitative analysis. The field had clearly moved in the direction of scientific precision and rigor—a trend that has increasingly continued since, notably in North America (Augier *et al.* 2005). Moreover, the growing importance of scholarly journals as research outlets for management research to the detriment of the earlier in-depth studies published as books (*cf.* Dyer and Wilkins 1991) made it increasingly difficult for historians to convey the richness of their material to management scholars.

On the way, the field that was originally defined as “management” or “administration” became divided into what were to develop later as separate disciplines. As a result, “management” or “management studies” increasingly became an umbrella term to include a division between what has broadly been characterized as a “macro” as opposed to a “micro” orientation. The former is often regarded as incorporating strategic management and organization theory as the major sub-fields or disciplines, whereas the latter includes organizational behavior and human resource management. These sub-fields have moved in the way of developing institutional structures providing distinct identities in the way of doctoral training programs, journals, and associations, though companion umbrella structures are also available, such as the American and other academies of management.

There have been some time lags in the pace of scientization, with organizational behavior and organizational theory taking the lead. These developments

took somewhat longer to reach the sub-fields of human resource management and business strategy, where until about the mid-1970s the predominant approach in the literature continued to be based on studies of the histories of individual or a small number of firms—Chandler’s work constituting the prime example. But from the late 1970s onwards, the sub-field of strategy also turned towards the empirical-quantitative tradition, influenced by the economics of industrial organization (*e.g.* Porter 1980) and other social sciences (*cf.* Whittington 2001). The concern with history in management studies itself became confined to the study of early management and organizational practices and the literatures on them. In the United States much of this research has been concentrated around the Academy of Management’s Management History Division. The division is based on a model combining professional teachers and business practitioners (prominent in the early years of the business schools). It neither followed the Chandlerian synthesis in business history nor espoused the scientization of management studies and remained rather marginal as a result.

This brief account of the way management studies unfolded in the second half of the twentieth century largely represents the trajectory in North America. There has been a strong American influence on management studies in Europe in the post-World War II period and a considerable transfer of research, content, and institutional models, though the penetration of the latter in particular has been partial. Nevertheless, the dominant research traditions have been different in almost all sub-fields of management, European research leaning more towards inductive, processual, and qualitative case-study methods as opposed to the primacy of the natural science model in North America (Collin *et al.* 1996). Additionally, management research in Europe has sustained stronger ties with social theory at large and more specifically with sociology and anthropology (Üsdiken and Pasadeos 1995). There is in particular a largely European-based literature on comparative/national business systems (with few North American exemplars such as Hamilton and Biggart (1988)), which grew out of organizational and economic sociology, often practiced in the emerging European business schools. It drew explicitly on a long-term perspective—and partially on the historical literature—to explain the differences in institutional contexts in accounting for international variation in dominant forms of big business organization (*e.g.* Whitley 2002).

Despite these methodological and disciplinary leanings and openness, however, even European management research has not developed until recently a strong engagement with history at large or business history in particular. This is partially due to a lack of institutional overlap—with few business historians located in business schools or management departments. At the same time, it is also due to a lack of interest by many business historians, who preferred to remain closer to their intellectual origins in history, economic history, and economics rather than engaging with management studies.

5.4 STUCK ELSEWHERE: BUSINESS HISTORY BETWEEN HISTORY AND ECONOMICS

Chandler's influence on business history was even more important than his impact in management studies. It is probably no exaggeration to say that his work created business history as a serious field of academic study in the 1960s and 1970s (cf. McCraw 1988). He moved the field decisively away from narrative accounts and from a focus on individual entrepreneurs, which had dominated the earlier business historical literature (cf. Galambos 2003). His work on the emergence and transformation of large-scale managerial enterprise in the United States set the research agenda for several decades to come. Even for those who extended or criticized his findings it became and remains a crucial reference point (cf. John 1997 and the contribution by Cassis in this volume). But while Chandler had suggested that business historians should either provide social scientists with empirical evidence as a basis for generalizations or develop these generalizations themselves, little progress seems to have been made in that direction. There are a number of reasons why the dialogue between business history and management studies stalled.

First of all, despite Chandler, much of business history continued to provide narrative histories of companies, entrepreneurs, and industries. The intention here is not to criticize such an approach *per se*. Case studies were and continue to be an accepted method of research in the social sciences (Yin 2003)—with some debate about the balance between rich description of a single case (Dyer and Wilkins 1991) and the need for comparison among several cases (Eisenhardt 1989). The point here is that much of the business historical literature insisted on the singularity of the events it described—thus rejecting possible generalizations. Some more recent company histories use existing concepts from the social sciences to provide additional insights into their specific findings or relate them to broader debates (e.g. Jones 2005b; Fear 2005). But the vast majority does not. This is not surprising given that most business historians were trained as historians, worked in history (or economic history) departments, and therefore had little exposure to other social sciences. Moreover, most company histories—whether commissioned or not—are directed at non-academic audiences. In general, these audiences have little knowledge of and interest in the conceptual and theoretical developments in the social sciences, including management studies—where academia has grown increasingly apart from practitioners and practice (cf. Kieser 2002; Whittington 2004).

Second, even if business historians did try to interact with other academic audiences, on the whole they have tended to look at economics, in terms of both their empirical focus and conceptual frameworks. This interest is partially mutual. Chandler's work actually found some echo among the so-called transaction cost

economists. In particular Oliver Williamson (e.g. 1985) drew heavily on Chandler's account of the rise and divisionalization of large-scale enterprise to support his argument that the modern corporation in its various manifestations over time was a device to save on transaction costs. Chandler (1992) distanced himself from this approach, as had others before him (Lazonick 1991). Instead, he endorsed the so-called evolutionary economics (Nelson and Winter 1982), since it also had the firm as its basic unit of analysis, and examined organizational capabilities and organizational learning (cf. also Chandler *et al.* 1998). Other business historians in the economic history tradition and economic historians have nevertheless been promoting transaction cost and agency theory, broadly defined as economics of information, which they have recently put forward as an alternative to the Chandlerian framework (Lamoreaux *et al.* 2003, 2004, and this volume).

There is nothing inherently wrong with using concepts from economics, such as information costs, or interacting with evolutionary and institutional economists. Economics has also influenced many important "schools" within the management literature, such as the resource-based view in strategy for example. But it seems to have done business history little good in terms of its relevance outside its own field. First, mainstream economic history at least in the United States and partially in the UK had already moved into different—cliometric—directions from the late 1970s onwards and showed little interest in business historical research. Second, business historians left much of Chandler's original research agenda for others to complete, in particular with respect to the M-form (see above). They did little to participate in the more theoretical debates or provide additional empirical evidence—with a few recent exceptions (e.g. Toms and Wright 2002; Binda 2004). Last not least, the "fixation" of many business historians on market mechanisms and firm performance (cf. Kipping 2003) did little to maintain or generate interest in their work among organizational and economic sociologists as well as neo-institutionalists—in principle more predisposed towards historical approaches. Many of them came to lump together the work of Chandler and others with neo-classical and new institutional economists à la Williamson under the label "efficiency theory", i.e. sharing "the assumption that there is a selection process that ensures that more efficient economic forms will prevail over less efficient forms" (Roy 1997: 7; cf. also Abrahamson 1991).

From the 1980s onwards, many of these scholars carried out their own research on the rise and evolution of the corporation—with interests and power as the major driving forces. Thus, Fligstein (1990) attributed the transformation of corporate control in the US to the legal/political impact of changing anti-trust regulation as well as the shifting educational/functional backgrounds of managers—a view partly accepted by Chandler (1994). Roy (1997) tried to show how the corporate form originated around the turn of the twentieth century in the government-owned utilities and became generalized due to pressures from financial markets rather

than as the result of changes in technology and organizational capabilities (cf. also Perrow 2002). Freeland (2001) re-examined the case of General Motors and, based on his own archival research, argued that Chandler's account in *Strategy and Structure* and in Sloan's (1963) autobiography, on which he collaborated, was inaccurate and misleading. He highlighted the role of power struggles and middle managers in the emergence of the M-form, rather than competitive pressures and visionary leadership. But regardless of what appeared like a widening gap between management studies and business history, the latter continued to have some impact on the former—albeit in more marginal areas.

5.5 REMAINING RELEVANCE: INTERNATIONAL BUSINESS AND MANAGEMENT HISTORY

The area where the contribution of business historians is probably most apparent and widely recognized is International Business. This might partially be due to the fact that International Business is a fairly well-delineated field of academic research, with its own conferences (the Academy of International Business and its regional replicas) and scholarly journal (the *Journal of International Business Studies*). Scholars in the field have continuously stressed their interest in history and the importance of the historical dimension—even if much of this seems to have been lip-service (Jones and Khanna 2006). Historians of international business have nevertheless provided extensive empirical evidence, both overviews and specific case studies, many of them internationally comparative in nature. And, while drawing on Chandler, they moved beyond his framework by looking not only at large-scale manufacturing enterprises, but also at natural resources and the service sector as well as small firms. Historians also engaged with part of the prevalent theories in International Business, and even contributed at least one important concept to this literature: the idea of the “free standing” multinational.

This concept was developed by Wilkins (1988) based on a comparison of British and US multinationals. For most of the latter, foreign expansion grew out of a significant domestic business and included subsidiaries in many countries. Instead, many of the pre-1914 British multinationals had only a small head office in the UK, usually in London, where they were registered, but operated exclusively abroad—in general in a single country, where they owned one or more plantations, mines, railroads, or utilities. As subsequent historical research showed, these free-standing companies were confined neither to Britain, nor to the pre-World War I period (Wilkins and Schröter 1998). More importantly, the concept was taken up by other

scholars in international business, who discussed the use of the most appropriate economics-based frameworks to explain this particular form of organization (cf. Casson 1994). Jones has also combined conceptual frameworks with historical evidence in his extensive work on multinationals—including his survey text on the historical development of international business and global enterprise (Jones 2005a). Aiming more broadly, in a recent article, he highlighted the reference to an idealized or stylized vision of the past in the current international business literature and tried to promote instead the use of real historical data and cases (Jones and Khanna 2006).

Wilkins and Jones are but the tip of the iceberg. Business historians have made and continue to make important empirical contributions to the study of international business, covering more countries each time, both in the developed and the developing world, and more cases (for a comprehensive overview see Jones 2003 and his contribution in this volume). But it is important to mention at least one major limitation, which is due to the general orientation of business historical research discussed above and might be one of the reasons why historians of international business have not achieved an even wider recognition in the field. Thus, among the available frameworks from the international business history literature, business historians have largely used those broadly based in economics, in particular Dunning's (2003) eclectic paradigm. The same is true when it comes to organizational capabilities and learning or the knowledge-based view of the firm, where the heritage of evolutionary economics is again undeniable. By contrast, concepts closer to management studies have made few inroads into the historical research. Thus, while sometimes mentioning them, business historians have not yet used frameworks such as the internationalization process model, originally proposed by Johanson and Vahlne (1977), which are more dynamic and process oriented and thus should fit their research particularly well. Again, there are some exceptions, like a recent case study of a UK-owned multinational which uses concepts from organizational sociology and the national business systems literature to explain the overall advantages of the company and the local adaptation of its Danish, American, and British subsidiaries (Kristensen and Zeitlin 2005).

This kind of collaboration and mutually beneficial interaction can also be observed in a few other areas, which one might summarize broadly under the heading of Management History. Here, business historians have also made a number of recognizable—and recognized—contributions to broader debates. We will look at three of these areas: the transformation of production systems, which also includes the role of industrial districts; the American influence on the development of European and Japanese companies; and the evolution and role of management education and management consulting.

Since the early 1980s, there has been an important debate in the popular and academic management literature about the future of the scale-intensive Western production system, broadly characterized by the labels “Taylorism” and “Fordism”.

This debate was driven in large part by the competitive success of companies originating from Japan and (somewhat later) other Asian countries as well as Italy, which clearly defied the logic of mass production and vertical integration that had been highlighted by Chandler and others as the driving force behind the superior performance of American corporations. Some of the scholars involved in this debate took an explicitly comparative and historical approach suggesting that the previously dominant mass production was giving way to a new system that they characterized as “flexible specialization” (Piore and Sabel 1984). Historians of business and technology also made an important contribution by looking at the origins of the mass production system and its historical alternatives. They also showed that developments of the corporate economy and organizational forms were not teleological, as much of the earlier historical literature had explicitly or implicitly assumed (for a detailed overview, see the chapter by Zeitlin on the “Historical Alternatives” approach in this volume).

A particular focus of the debate about flexible specialization and the contribution by historians was the development of production methods in the automobile industry (cf. Shiomi and Wada 1995), for example about the tiered supplier system in Japan as an alternative to the vertical integration prevalent in the United States. In general, business historians helped elucidate the reasons behind the success of Japanese companies (e.g. Yuzawa 1994). In many cases, their work stressed the role of entrepreneurial initiatives, while downplaying the importance of government intervention and protection. This clearly contradicted widely held views in the United States at the time—some of them fomented by other historical studies (McCraw 1986). The same is true for the Italian case, where scholars from comparative politics, political economy, economic geography, and management had drawn attention to the role of localized trust-based production networks, sometimes referred to as “clusters” or “industrial districts” as the basis for international competitiveness. Business historians played an important part in this research by providing in-depth case studies of the evolution of industries and companies within these districts (e.g. Colli 1998). The historical research on more flexible alternatives to large-scale mass production eventually also reached the homeland of the latter, with Scranton (1997) providing ample evidence for the persistence and success of specialty production even in the United States (see also Zeitlin’s chapter on industrial districts in this volume).

Partially following on from the flexible specialization debate was a broader concern among management scholars about the international transfer of management ideas or models. Whereas the previous debate had been driven by comparative politics, political economy, and economic geography, this one mainly involved organizational and economic sociologists. Starting with a focus on Japanese production and management methods (e.g. Kenney and Florida 1993), it soon broadened to encompass the overall evolution of ideas about how to manage (e.g. Guillén 1994), which had so far been left to historians of management thought (e.g. Wren 2005).

Once again, a number of business historians made significant contributions to this debate with their in-depth case study research and some attempts at generalization. This concerned in particular what is now widely known as “Americanization”, the transfer and transformation of US technology and management models to other parts of the world. Partially originating in the political history of US hegemony after World War II, this literature originally looked at American efforts to spread its “productivity gospel” (Maier 1977; McGlade 1995) and its labor relations model (Carew 1987).

Subsequent work examined how these ideas were actually received and transformed in particular companies. An important part of the business historical contribution consists in highlighting the active role played by companies, managers, and engineers in the receiving countries (e.g. Kipping and Bjarnar 1998; Kleinschmidt 2002) and the actual reworking of these ideas themselves, often leading to unique hybrid solutions (e.g. Zeitlin and Herrigel 2000). Originally confined to Europe, this research more recently also looked at the Japanese cases in comparison (ibid.; Kudo *et al.* 2004). This work in business history parallels similar work in historical sociology (Djelic 1998), which has drawn attention to the geopolitical dimension and the conformity pressures exercised by the US authorities in post-World War II Europe. Both the historical and sociological research have left their imprint on a growing number of studies on the dissemination of management ideas—sometimes conducted in collaboration between management scholars and business historians (e.g. Engwall and Kipping 2004).

From the dissemination of ideas, it is only a short step to the different ways in which they are disseminated. The role of what was alternatively referred to as the management knowledge industry (Micklethwait and Wooldridge 1996) or management fashion setting communities (Abrahamson 1991) became an important topic in management research during the 1990s. Here, historians have probably left their most important imprint in studying the evolution of management education (for details see the contribution of Amdam in this volume). Important to note, and in many respects exemplary, is the fact that more and more of this work has been done in collaboration between business historians and management scholars. The same is true for historical work on management consulting, where the management literature saw a significant increase in interest and publications from the 1990s onwards (cf. for overviews Clark and Fincham 2002; Kipping and Engwall 2002). On the one hand, business historians provided a long-term perspective to a research that was predominantly focusing on the “explosive” growth of the industry at the end of the twentieth century. Thus, McKenna (2006) has charted the rise of the McKinsey-type strategy and organization consultants since the 1930s, while others have tried to explain the transformation of consultancy services and service providers over time, including scientific management and large-scale IT systems in their studies (e.g. Wright 2000; Kipping 2002). There are also detailed historical accounts of consultancy development for a growing number of countries (e.g. the contributions

in Kipping and Engwall 2002) as well as the expansion of US consultancies to Europe (Kipping 1999).

Again, this work was presented not only to the business history community, but also within management studies (e.g. McKenna 1997) and is increasingly conducted jointly by business historians and management scholars. Also, it addressed broader debates such as the development of knowledge-intensive firms (e.g. Kipping and Kirkpatrick 2007). The second strand of historical research on consulting was a growing number of in-depth company case studies looking at the interaction between consultants and their clients (cf. the contributions in Kipping 2000; Kipping and Engwall 2002). These cases provide important insights. On the one hand, they confirm the tendency, stressed in the neo-institutional management literature (DiMaggio and Powell 1983), for companies to imitate their peers and the role consultants played in this process. On the other hand, they highlight the complexity of the consultant–client relationship and its uncertain outcome, which modifies the widespread view about the superficial and even dangerous nature of consulting advice.

There is some hope that this successful model of interaction and collaboration in international business and management history, outlined above, can be extended to other areas of management studies—in particular core areas of strategy and organization. We discuss some of the examples and possibilities for increasing dialogue and interaction in the following section.

5.6 A NEW DEPARTURE: RETURN TO THE CENTRE

It seems that there is now a new chance for business history to reconnect with management studies on a larger scale. There are two powerful reasons: One is necessity because the previous intellectual position of business history between history and economics has become untenable due to the evolution of these fields. The other is opportunity because there have been repeated calls for historical approaches in studies of management and organization (e.g. Kieser 1994; Zald 1996). We will first briefly discuss the former and then more extensively develop the latter.

First of all, history in the United States—and increasingly elsewhere—has taken a post-modern, post-structuralist or culturalist turn (cf. Galambos 2003). Taking such a relativist view is clearly at odds with the truth claims and the efficiency-orientation of most business historians. A move in this direction is not totally out of the question. Fields adjacent to business history, such as the history of technology, have taken it. Some business historians have advocated taking a similar path, and part of business historical research has been developing in this direction (see the

contribution of Lipartito in this volume). Incidentally, incorporating culture and a post-modernist stance does not mean that business history would be moving away completely from management studies—at least not all of it. There has actually been a strong post-modernist trend in management studies itself, influenced in particular by the work of Foucault and Derrida. Several management scholars have therefore argued quite forcefully in favor of a historical turn in organization studies (e.g. Clark and Rowlinson 2004).

Mainstream economics has also evolved, moving squarely in a neoclassical direction. Incidentally, this has also led to the marginalization of economic history within economics. As seen above, some business historians together with a minority of economists have sought refuge in transaction cost and more broadly in institutional and evolutionary (“Schumpeterian”) economics (cf. Lazonick’s chapter in this volume). Taking such a direction does not mean that business history would automatically move away from management, because evolutionary and institutional economics are in many respects closer to management than to neoclassical economics. A recent edited volume on *Constructing Corporate America* has sought to combine several of these trends by incorporating ideas (and authors) from institutional economics, the cultural perspective, and economic sociology (Lipartito and Sicilia 2004).

But one does not need to take the route through the post-modernist history or institutional economics to find a growing appreciation of history and historical approaches—mainly, but not exclusively, outside the United States. As mentioned, the literature on comparative forms of economic organization or national business systems that is based in organizational and economic sociology has already shown an interest in historical approaches and drawn on some historical research. The emphasis in this literature has been on social and political institutions and their development along with histories of industrialization in specific countries and their impact in turn on forms of business organization. Although some of the later work in these traditions considered the histories of individual business firms or specific industries, most approaches focused on trajectories of industrial development at the national level (Whitley 2002). Again, as noted above, they have drawn partially on existing historical research and also inspired some work by business historians. A companion area that has attracted historically oriented studies as well as the interest of business historians has been comparative corporate governance. Among the more recent historical studies in this respect or those that have involved business historians are the work by O’Sullivan (2000) on corporate governance in the United States and Germany and by Maclean *et al.* (2006) on business elites and corporate governance in France and the UK (see also Herrigel’s chapter in this volume).

Another possible area for future interaction is studies of entrepreneurship and innovation. Despite the plea to the contrary from one of the pioneers in the field (Schumpeter 1947), there has been little cross-fertilization between the historical research on entrepreneurship and similar work in the other social sciences. Over

recent years, attention within the latter has increasingly focused on entrepreneurial behavior and cognition, while less and less importance has been granted to the historical context of entrepreneurial activities (cf. Jones and Wadhvani 2006 and this volume). As a result, the contributions made by business historians in this field have often gone unnoticed. Again, there are some attempts to cross the existing divide. Thus, a recent study on the development of the British outdoor trade draws explicitly on concepts from economics and management to examine the role of entrepreneurs and “communities of practice” in innovation (Parsons and Rose 2004). This study is also worth mentioning, because it is based on close cooperation between an academic and a practitioner.

In addition to these encouraging developments, we would argue that it is now time for business historians to reconnect with the two core macro-orientated sub-fields within management studies: strategy and organization theory—which would help bring business history back to the more central position where it began with Chandler’s work half a century ago. This opportunity exists because over the last 10 or 15 years particular streams of research have demonstrated interest in and involvement with history and historical approaches. However, it should be noted that this greater interest has involved a turn towards history more broadly rather than drawing upon business history specifically—though some of the studies have involved investigations of the histories of particular business firms. Moreover, the recourse to history in these theoretical perspectives has been in the form of supplementing the social scientific enterprise (using history for theory/hypothesis development and/or as testing ground for general theories) or seeking to integrate history in constructing historical theories of organization and/or employing concepts embodying historical effects (Üsdiken and Kieser 2004). Nevertheless, we suggest that business historians can benefit from the concepts developed in this literature and might contribute rich evidence, based on their in-depth, archive-based studies.

There are two particular strands in the strategy literature that have engaged with history. One has been the extension of the processual tradition pioneered by Chandler and later found in the work of Mintzberg and his colleagues (e.g. Mintzberg and McHugh 1985) and that of Pettigrew (1985) on ICI. Rosenbloom (2000), for example, studied the history of NCR over the period 1938–78 to provide a counter-example of how an established firm could successfully cope with and prosper within a context of radical technological changes. Coming closer to treating history as an integral element in a key concept is Coté *et al.*'s (1999) study where the notion of dominant logic is treated as rooted in the history of the firm and its linkages with the firm’s acquisition strategies and business performance are examined over time. Of particular relevance is an article by Farjoun (2002a), where he distinguishes two perspectives in the strategy literature: a dominant mechanistic one, based on planning and design, and an emergent organic one, more evolutionary and process-oriented. Among other things, he shows that, while widely used by

the mechanistic tradition, Chandler’s *Strategy and Structure* contains a significant number of organic themes.

A second theoretical perspective in the strategy literature that stands out in the role it accords to history is the resource-based view of the firm (e.g. Barney 1991). This view argues that competitive advantage is based on unique resources of companies, finding its more managerialist expression in the idea of core-competencies and dynamic capabilities—concepts some business historians have found appealing. Thus, apart from more recent studies by strategy scholars on the historical development of capabilities (e.g. Tripsas 1997), a good example of how a business historian can participate in these debates and publish in a leading strategy journal is Raff’s (2000) study of two book superstores in the United States (Borders and Barnes and Noble) over the period 1975–95, which also draws upon evolutionary ideas.

Within organization theory two perspectives where history does feature are population ecology and neo-institutional theory, which constitute two of the research programs that have gained prominence especially in North America over the last two decades or so (Üsdiken and Pasadeos 1995). Population ecology is an archetypal example of using histories of organizational populations and organizational life histories for testing theoretical arguments. Although some of the studies in this genre cover very long time-frames such as 100 years or more (e.g. Miner *et al.* 1990), the ultimate purpose is to contribute to the testing and refinement of a timeless and spaceless theory of organizational diversity, founding, survival, and change. Little is made therefore of history other than to provide stylized data on organizations that have and do constitute the population. Within this broader framework, however, work on co-evolutionary processes appears particularly promising for more specifically business historical approaches. Thus, Jones’ (2001) study of the early history of the American film industry in the period 1895–1920 provides an illustration of co-evolutionary processes involving the external context of the industry, institutional rules, competitive dynamics, and firm practices. Another more recent example is Murmann’s dissertation (2003), which has looked at the co-evolution of national institutions—namely those generating and protecting scientific knowledge—and the synthetic dye industry before 1914.

Concern with history has occupied a more prominent role especially in particular strands of institutionalist thinking in organizational analysis. The emphasis on history has had to do with the predominant institutionalist emphasis on stability and the recognition that institutional arrangements are likely to be path dependent and therefore not flexible. Moreover, there has also been the accompanying recognition that organizations are not only influenced by current pressures but also by past circumstances (Scott and Christensen 1995). Likewise, the need for a historical perspective has been acknowledged in studying inter-organizational fields (e.g. Scott 1983). A historical approach has also been necessary in the greater attention devoted to studying the process of institutionalization and the creation and development of

institutions as well as of institutional change. These issues have led to studies with a historical perspective as well those that have been historically located, though especially in the latter case, despite incorporating historical narratives, the primary concern has been with testing more general institutionalist theses. Farjoun (2002b), for example, examined on-line pricing conventions and used historical analysis to investigate the “motivation and process of building institutions”, which are then used to empirically test the hypotheses that are derived from the historical account.

Narrative historical accounts of institutional development and change are still relatively rare and perhaps remain more of a European specialty. Notable examples are Borum and Westenholz's (1995) historical study of the Copenhagen Business School and the change in institutional models that are adopted and Holm's (1995) study over the period 1930–94 of the rise and fall of a particular institutional form, the mandated sales organization in Norwegian fisheries. Leblebici *et al.*'s (1991) study of changes in institutional practices in the US radio broadcasting industry over the period 1920–65 is exemplary of the limited work in this tradition coming out of North America, though very recently US-based work of this genre appears to be increasing as well. Notable, for example, is Hargadon and Douglas's (2001) study, which develops the notion of “robust design” to account for the penetration and acceptance of Edison's electric lighting within an institutional field. Other examples are studies on the US business incubator industry (Leblebici and Shah 2004) and the 140-year history of Major League Baseball in the US (Chacar and Hesterly 2004), which have appeared in a recent special issue of *Business History* on History in Organization Studies.

As these examples show, there is considerable overlap of interest among certain areas in strategy and organization theory and parts of business history. But there are also significant differences, namely in terms of research methodology and dedication to theory building and/or testing. To us, this suggests that in many respects these perspectives complement each other, which provides ample room for a closer interaction in the future.

5.7 CONCLUSIONS

Business historians have entertained close, mutually beneficial relationships with other fields of research, as numerous chapters in this volume show in some detail. In this respect, the relationship with management studies is somewhat of a paradox. Having been very close in the formative period of both fields during the 1950s and 1960s, it subsequently became confined to a few, rather marginal sub-fields, namely International Business and Management History, where business historians have been making and continue to make empirical and, albeit to a lesser extent,

theoretical contributions. The lack of a closer interaction in the more central areas of strategy and organization has largely been due, we have argued, to the adoption of a neo-positivist social science methodology in management studies, which has relegated more narrative case studies—typical of (business) historical research—to the margins. But at the same time, a majority of business historians have sought wider recognition and conceptual frameworks elsewhere, in particular within economics—with rather mixed success.

Over the last decade or so, there have been calls from both fields for a renewed dialogue. As we have tried to show, there has already been more interaction in a number of the underlying disciplines of management research, in particular with areas of economics outside the neo-classical mainstream and with organizational and economic sociology. Based on an examination of recent research in strategy and organization theory, we have argued that similar opportunities now exist in these two core areas of management studies. To exploit these opportunities, the first step is a better understanding of what actually constitutes historical research, which—as we have tried to show—is more than the quantitative analysis of longitudinal data sets and, even if closer, also different from process-type studies, where the past remains largely an instrument for present-day concerns. Almost half a century later, Chandler's *Strategy and Structure*, despite its limitations, probably remains the best starting point for both business historians and management scholars to find such a common understanding and a platform for future cooperation.

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