Advances in Strategic Management

Emerald Book Chapter: Marrying History and Social Science in Strategy Research

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MARRYING HISTORY AND SOCIAL SCIENCE IN STRATEGY RESEARCH

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ABSTRACT

Purpose – *This chapter is intended to encourage comparative-historical research in strategy by articulating a framework for the study of industry and firm evolution.*

Design/methodology/approach – *Strategy research at its core tries to explain sustained performance differences among firms. This chapter argues that one, out of the many ways to create a productive marriage between strategy research and historical scholarship, is to carry out historically informed comparative studies of how firms and industries gain and lose their competitive position. While much of current strategy research adopts a large* N *hypothesis testing mode with the implicit assumption that one discovers generalization just like a Newtonian law such as* $F = m \times a$ *that applies across all space and time, an historically grounded methodology starts from the opposite direction. It assumes that a process or event may be idiosyncratic and therefore seeks to establish with detailed evidence that a 2nd (and later 3rd, 4th, … nth) process or event is indeed similar before generalizing across observations.*

Findings/originality/value – *The chapter argues that the field of strategy would benefit from allocating more effort on building causal generalizations*

History and Strategy
Advances in Strategic Management, Volume 29, 89–115

ISSN: 0742-3322/doi:10.1108/S0742-3322(2012)0000029008

inductively from well-researched case studies, seeking to establish the boundary conditions of emerging generalizations. It articulates a comparative research program that outlines such an approach for the arena of industry and firm evolution studies.

Keywords: Industry evolution; firm evolution; comparative history

INTRODUCTION

Ever since sociology was institutionalized as a distinct academic discipline in the late 19th century, sociologists and historians have debated how these two fields and their methods differed. A prominent early example is the debate between Emile Durkheim, one of the founding fathers of sociology, and the historian Seignobus (Durkheim & Seignobus, 1982[1908]). The present volume attempts to make historical scholarship and its methods again more prominent in the field of strategy. Although Alfred Chandler's major writings, *Strategy and Structure* (1962), *The Visible Hand* (1977), and *Scale and Scope* (1990) are regarded widely as core books in the field of strategy, historical methods had become marginalized in the typical doctoral student education as the field was striving for scientific respectability in universities over the past four decades (see McKenna, 2012, in this volume on the parallel development of strategy as a consulting business).

In many ways the strategy field is now repeating a similar development that took place in sociology a few decades earlier. In the 1940s and 1950s, sociology tried to import a positivist philosophy of science with its focus on finding universalist laws among a few variables just as Newton had done so successfully in physics. In the context of this movement, historical research – with its emphasis on identifying how behavior differs across time and place – seemed to many academics antiquated at best and scientifically flawed at worst. However, when, after trying hard for a long time, sociology had not delivered powerful "universal laws," the positivist project itself became suspect. Leading sociologists such as Charles Tilly (1984) and Theda Skocpol (1984) sharply criticized the positivist agenda in sociology, arguing that macro sociology is fundamentally a historical science that tries to explain dissimilarity in outcomes across space and time as much as it looks for similarities. This paved the way for the emergence of the subfield of historical sociology in which Tilly, Skocpol, and Arthur Stinchcombe (1978, 1997) among others played an important role.

When I arrived in graduate school in 1991 to study for a Ph.D. in management, the new generation of researchers seemed to believe what historians do is both unsystematic and completely atheoretical: in short, the exact opposite of what a good social scientist would aspire to engage in. I spent my first couple of years as a "typical" doctoral student, running econometric analyses on large datasets about whose underlying empirical reality I knew little, learning all the reasons why what I was doing was so much more sophisticated than what those storytelling historians were engaged in. Trying to find stable relationships between variables, we were seeking the Newtonian laws of the social universe, while the intellectually feeble Ph.D. students in history, who did not even understand basic sample selection strategies, would at best learn how to become journalists of long-gone times and places. Their work would be totally useless for managing the affairs of today and tomorrow. While this description may appear to be a bit of an exaggeration, I think it captures well the spirit of how we were socialized to think about history.

My book *Knowledge and Competitive Advantage: The Coevolution of Firms, Technology and National Institutions* (Murmann, 2003) shows how dramatically I changed my view about the value of history for the social sciences in general and the study of how industries and firms develop in particular. One fundamental difference between an ahistorical natural science such as Newtonian physics and social science is that physical facts exist independent of the observer, whereas many social facts depend fundamentally on the agreement between human beings. Let me give an example: A $100 bill does not obtain its causal power from the paper it is printed on but from the fact that presently there is agreement in U.S. society that a $100 bill issued by the government serves as a storage unit of value and can be exchanged for any good in the economy. If no human being existed on this planet, the $100 bill would lose its power. Similarly, it is not difficult to imagine a future U.S. society that no longer knows any paper currency and instead relies solely on electronic currency. In this case the $100 bill would also have lost its causal power as a currency. There is a second key difference between a Newtonian ahistorical science and any science of society. While the financial crisis of 2008 may have some causes in common with the one that occurred in 1929, the world changed substantially in the intervening eight decades. This means that an explanation of the 2008 crisis will need to incorporate that the global financial systems had become more interconnected in part because of cheap computers and telecommunications technology that did not exist in 1929.

Historians take it as a premise that social conditions and the way society views the world change over time. For historians, to explain social action one needs to describe in some detail the larger conditions in which the action occurred. If one reads a bit of the historical literature describing how dramatically economic conditions changed in the course of human history – but especially since the industrial revolution in the 18th century – one realizes that the ahistorical Newtonian physics is the wrong model to imitate for any social science and the strategy field in particular. The historical natural sciences such as biology and geology are much better models but even they often don't capture the additional complexity that is created by self-aware human beings who have amazing capabilities to change their behavior in different conditions.

In the present context, it seems useful to briefly describe how my change of heart about the usefulness of history came about and why the field of strategy would benefit from a similar change in orientation that occurred in sociology earlier. Working with Michael Tushman on the question of how different types of technological innovations would affect the development of industries, I came across Hugh Aitken's books on the history of radio (1976, 1985); Thomas Hughes' (1983) history of the development of electric power networks in Chicago, Berlin, and London; and Walter Vincenti's (1990) work on the development of airplanes and the discipline of aeronautical engineering. It was simply not true that historians were merely telling one damn fact after another (see Gaddis, 2002, for a recent articulation of how historians work). The best historians don't shy away from abstractions and theory. Aitken, for example, in his history draws heavily on role theory from sociology, Hughes on general system theory, and Vincenti on evolutionary theory that my 2003 book builds on and tries to develop a little further. Joel Mokyr articulates in his book *The Gifts of Athena* (2002) an abstract theory of different kinds of knowledge and he then uses this theory to explain why and where the industrial revolution occurred. But historians always pay careful attention to formulating precisely the causal story that led to the outcome they are trying to explain. I also found that the thick descriptions—to use a term coined by the anthropologist Clifford Geertz (1973)—historians are using to lay out a causal sequence would, unlike summarizing one's key findings in a regression table, make it much easier to imagine and try alternative theoretical explanations for the phenomenon at hand. One's mind can more readily accept alternative explanations as compelling if one is presented with enough detail about what empirically happened.

I also discovered that the field of history includes an institutionalized practice that is important for any good empirical science: In their quest for

professional recognition historians compete over who comes up with the more accurate description of what actually happened in the world. The conclusions regarding what the facts mean and how they should be interpreted from a causal point of view always come after trying to get the facts right. How much attention is the average reviewer in strategy journals presently paying on ensuring that an author got the facts right rather than ensuring that the statistical techniques used are valid? How many published articles in the field of strategy can you name whose main purpose is to overturn the empirical foundation that was used to confirm theoretical interpretations?[1] Would the field of strategy not benefit from imitating the field of history in this regard?

In this chapter, I will first describe the historically informed methodology that I used in my 2003 book and discuss how this methodology is conducive to making progress on many of the central questions in strategy research. It is useful to recall that if one tries to develop causal theories of performance differences among firms and claims that these differences are not random but are created through an intentional process on the parts of managers, then the burden of proof is quite steep. Given that most firms fail (Murmann, Aldrich, Levinthal, & Winter, 2003), the question arsises, how much managers can control the fate of their firms. When we want to argue that managers were responsible for the firm's superior performance, we ideally demonstrate the entire causal chain from the intentional action to the later performance differences. I will argue that causality about performance is easier to demonstrate in a comparative longitudinal research design that stays close to the historical phenomenon rather than a single case study.[2] There are many other ways to carry out research with historical sensibility and in no way do I want to give the impression that my proposal is the only approach to marrying strategy research and history. To get a sense of the many ways to use history, I refer the reader to other contributions in this volume, to the articles by O'Sullivan and Graham (2010) and Jones and Khanna (2006), and to the monographs by Charles Tilly (1997, 2008), Arthur Stinchcombe (1978, 2005), and Andrew Abbott (2001, 2004).

I will focus on formulating a call for collecting comparable data on firm and industry development that will provide a stronger foundation for constructing, refining, and testing theoretical ideas in strategy. (The contribution of Ingram, Rao, & Silverman, 2012, in this volume, outlines a complementary approach based on analytic narratives.) While my proposal is reminiscent of the call for a comparative database that John Freeman (1986) made when he was the editor of *Administrative Science*

Quarterly, it is closer in spirit to the *Human Area Relations File* (HARF) started in 1949 by a consortium of major universities under the leadership of Yale University. Until today, the HARF has the mission "to provide information that facilitates the cross-cultural study of human behavior, society and culture" (2012). The HARF project made it possible to systematically compare cultural practices, for example, marriage patterns across different areas of the world and determine how similar and different they are. If we had available similar data on many firms and industries, we could look for empirically grounded generalizations and articulate more precisely boundary conditions of such generalizations. To facilitate these comparisons, I will lay out a set of research questions and then articulate a list of variables[3] that would be very useful for scholars in strategy and history to collect. The novelty of this proposal is that it does not require one scholar to collect all variables and all time periods on a particular firm or industry. In fact, constructing a robust comparative framework makes it possible to divide the labor and pool efforts among strategy and organization scholars and their colleagues in business and economic history. The key in making such a project successful is to reduce the costs for people to contribute by publishing a common framework, so that at the same time as people engage in their particular research on a firm or industry they collect the information called for in the common framework. As will become apparent later in the chapter, the comparative framework is complementary to the FIVE Project organized by Connie Helfat[4] and to the efforts of the *Industry Studies Organization*[5].

STRATEGY AS HISTORICAL SOCIAL SCIENCE

One definition of the key task of strategy research that many scholars can agree on is: the field of strategy is concerned with providing explanations for sustained performance differences among firms (Nelson, 1991; Porter, 1996; Rothaermel, 2012; Rumelt, Schendel, & Teece, 1994). If we are trying to explain sustained rather than short-term or temporary advantages, the key question is: *How long does a competitive advantage have to exist before it would qualify as "sustained"?* Independent of the precise answer to this question,[6] the longer the time period one samples before pronouncing a sustained competitive advantage has existed, the more other scholars would agree with the finding.

After having taken course work as a doctoral student and read widely in strategy and related fields during the period from 1991 to 1994, I arrived at

the working hypothesis that our real challenge was not that we lack theory of what causes competitive advantage on a very general abstract level. Comparing the most prominent perspectives at the time (main-line economics, organizational ecology, early-Porter (Porter, 1980, 1985), resource-based theory, etc.),[7] I found that across the main theories available in strategy virtually all external factors and internal factors that one could dream of were covered when conceived on an abstract level. Our challenge hence was and is not that we lack *theory* per se. Rather our challenge was and is (1) to identify the boundary conditions of existing theories, (2) to combine existing theoretical perspectives without ending up with 1,001 variables to analyze, and (3) to operationalize general categories of variables in a theory, so they capture concrete competitive dynamics in a particular industrial setting. Examining competitive advantage over long periods of time raises the fourth challenge that environments can shift quite dramatically, making it more likely that new factors play a role in determining how competitive a firm is. This means that a priori it is almost impossible to identify what are good ways to operationalize on a semi-abstract level the dimensions of a variable.

In the early 1990s and still today, most of strategy research is of the hypothesis testing nature. But this style of research is rather ill equipped to address the four aforementioned challenges. Michael Porter articulated this problem clearly in his 1991 SMJ article. He writes: "I concluded in my most recent research that detailed longitudinal case studies, covering long periods of time, were necessary to study [competitive success] … This style of research nudges strategy research, and indeed industrial economics, into the world of the historian" (p. 116). Encouraged by Richard Nelson, who also believed that detailed empirical work on how industries and firms developed over time is going to bring about rich knowledge dividends (Mowery & Nelson, 1999), I set out to marry the sampling strategies developed by organizational ecologists (Hannan & Freeman, 1989) with methodologies developed by historians (Chandler, 1990; Hughes, 1983) and comparative sociologists (Skocpol, 1984; Tilly, 1984).

In the early 1990s virtually all full population studies that tracked the entry and exit of firms in the course of the development of an industry were carried out on the U.S. industries (Hannan & Carroll, 1992; Romanelli, 1989; Tushman & Anderson, 1986; Utterback & Suárez, 1993). Researchers had documented for a series of different U.S. industries that the number of producers initially started to rise and at some point the number declined, often quite substantially. This phenomenon was later dubbed *industry shakeouts* (Klepper & Simons, 1996). I had the suspicion that at least the

timing and maybe even the patterns of industry evolution would be shaped by a country's institutional infrastructure. For this reason, one of the key issues that I wanted to examine in my dissertation was whether country-level institutions had an effect on patterns of industry evolution. Second, I wanted to get enough detail about the development of individual firms so that I could observe how industry-level forces have an impact on the life course of individual firms. Third, I wanted to trace performance not just in terms of birth and deaths but also in terms of level of profitability and market share.

To make developments comparable I canvassed a number of different industries: steel, chemicals, machine tools, car batteries, car brakes, water filters, and initially settled on the chemical industry for two main reasons. It had a history dating back to the 19th century and, unlike machine tools, the leading firms across different countries went public in the 1880s, leaving ample public records. I started to collaborate with Ashish Arora, Ralph Landau, and Nathan Rosenberg (1998), who at the time were organizing a comparative study of the development of the chemical industry in Britain, Germany, Japan, and the United States. Initially, I was asked to write the chapter on corporate strategies, but when Alfred Chandler joined the team, my task became to write a comparative history of the industry in Germany and Britain with Ralph Landau (Murmann & Landau, 1998). Because he had developed important petrochemical processes, Ralph Landau knew the technological history of the industry after World War II very well. But to be able to write a comparative history that would meet the quality standards of professional historians, I followed the standard process of historians to read as much as possible of the entire literature on the subject. As I was learning a great deal about the chemical industry, it became clear that the products in the industry were so diverse that I was effectively dealing with multiple industries.

For this reason, I decided to focus my dissertation research on one branch of the industry, synthetic dyes, which was special for a number of reasons. The industry displayed huge differences in performance both at the country level and at the firm level, which would make it much easier to detect the causes of those differences. Britain and France led the industry for the first 8 years until about 1865, but then Germany came to dominate the industry and had at least 75% world market share for three decades (Table 1, Row 5 provides detail on the development of country shares). Similarly, three German firms—Bayer, Hoechst, and BASF—overtook their British and French rivals by the early 1870s, and then steadily increased their output and market share. Each possessed 20% world

Table 1. Indicators for the Evolution of National Populations of Synthetic Dye Firms.

	Great Britain	Germany	France	Switzerland	U.S.
1. Total firm entries	53	118	68	32	28
Total firm exits	43	94	57	26	18
Firm failure rate	81%	80%	83%	81%	64%
2. Firm entries + exits	1861–1877: 50	1861–1877: 74	1861–1877: 44	1861–1877: 25	1865–1877: 5
	1878–1893: 24	1878–1893: 72	1878–1893: 24	1878–1893: 8	1878–1893: 13
	1894–1914: 15	1894–1914: 59	1894–1914: 15	1894–1914: 2	1894–1914: 28
3. Firm turnover[a]	1861–1877: 7.14	1861–1877: 12.33	1861–1877: 7.11	1861–1877: 8.33	1865–1877: 2.50
	1878–1893: 1.71	1878–1893: 2.88	1878–1893: 2.18	1878–1893: 1.14	1878–1893: 4.33
	1894–1914: 1.00	1894–1914: 1.74	1894–1914: 2.36	1894–1914: 2.62	1894–1914: 4.67
4. Share of all firms in the world	1860: 28%	1860: 24%	1860: 36%	1860: 12%	1860: 0%
	1877: 22%	1877: 38%	1877: 15%	1877: 11%	1877: 5%
	1893: 17%	1893: 39%	1893: 12%	1893: 11%	1893: 7%
	1914: 14%	1914: 31%	1914: 15%	1914: 8%	1914: 13%
5. Share global market[b]	1862: 50.0%	1862: 3.0%	1862: 40.0%	1862: 2.5%	1862: 0.0%
	1873: 18.0%	1873: 50.0%	1873: 17.0%	1873: 13.0%	1873: 0.2%
	1893: 12.0% (est.)	1893: 70.0%	1893: 11.8% (est.)	1893: 10% (est.)	1893: 1.8% (est.)
	1913: 6.5%	1913: 74.1%	1913: 5.4%	1913: 7.0%	1913: 3.3%

Source: Adapted from Murmann (in press).

[a]Turnover is calculated by adding up the firm entries and exits in the period and dividing it by the number of firms in the year before the period.

[b]The 1862 figures are from Leprieur and Papon (1979, p. 207). The authors report that Germany and Switzerland together held 5% of the market. I estimate that Germany's share amounted to 3% and the Swiss share to 2%. The 1873 figures were put together by Ernst Homburg from Hofmann (1873, p. 108), Wurtz (1876, p. 235), and Kopp, 1874, p. 153). The 1912 figures are from Thissen (1922). Except in the case of Germany, I did not have figures for the year 1893. I estimated the countries' market shares by assuming that market shares declined between 1877 and 1914 in a linear fashion.

market share in 1913. The biggest British firms at the time (Levinstein and Read Holliday) had 2.0% of the world market each and the biggest American firm (Schoellkopf) had 1.7% of the world market. Second, the synthetic dye industry started at roughly the same time in several countries—Britain (1857), France (1858), Germany (1858), Switzerland (1859), and the United States (1864). This would give the comparisons across the different national dye industries more face validity because in a contemporaneous comparison many factors are held constant that would probably be variable in those comparisons made across national industries that started at much different historical moments. Third, it was what historians dubbed the first science-based industry and constituted the high-tech branch of the chemical industry in the second half of the 19th century. Corporate R&D laboratories as a routine aspect of firms were pioneered in this industry. This meant the industry stayed in constant technological flux during its first six decades, requiring firms to do novel things and adapt to stay in the game.

It is convenient to describe the research design I selected using the typology Abbott (2004) laid out in his wonderful short book entitled *Methods of Discovery: Heuristics for the Social Sciences*. Abbott identifies three different ways of classifying research methods: by the type of data gathering (ethnography, surveys, record-based analysis, history—here he means old records and documents); by the type of data analysis (direct interpretation, quantitative analysis, formal modeling); and finally by how one poses the research question (case-study analysis, small-*N* analysis and large-*N* analysis). He then explains that one can mix and match these different strategies leading to 36 ($4 \times 3 \times 3$) possible subtypes. Abbott notes that while all 36 types have been tried, five types have been most widely used. They are: 1, ethnography; 2, historical narration; 3, small-*N* comparison; 4, standard causal analysis (by which he means large *N*, statistical models); and 5, formalization. In terms of this scheme, I married historical narration with small-*N* case comparison. The observed differences in performance outcomes in the synthetic dye industry from 1857 to 1914 both at the country and firm levels were so large that I did not need econometric tools to detect effects but could rely on small-*N* comparisons instead. Small-*N* comparisons try to overcome the disadvantages of single-case studies and large-*N* studies. With single-case studies, there is always a question of whether the findings generalize at all. Large-*N* studies, on the other hand, as Abbott explains (2004, p. 22) have the problem that they oversimplify and change the meaning of variables by removing them from their context. Furthermore, small-*N* comparisons do not assume that one

already has been able to identify how to break a complex phenomenon into constituent parts, which is presumed in large-*N* studies with multiple independent variables. Ragin (1987) articulates in detail how it is possible to compare wholes (e.g. countries, national industries, firms) in small-*N* comparisons.

As mentioned before, to identify the causal processes that led to dramatic variations in performance across countries and firms in the period from 1857 to 1914, my study design was also going to marry the sampling strategies of organizational ecologists with the methods of small-*N* comparative sociologists and historians. Together with Ernst Homburg, a historian of technology, I put together a quantitative and qualitative database of all firms that left any trace somewhere in the historical records. (The database design is described in Appendix II of my 2003 book.) As a doctoral student more than a decade before, Homburg had participated in a project on the development of the dye industry (van den Belt, Gremmen, Homburg, & Hornix, 1984; van den Belt, Homburg, & Hornix, 1981) and had become the expert on the data sources for the industry and its relationship to science. Working together with a leading historian of the synthetic dye industry was invaluable for the quality of my work, preventing me from offering explanations that a person with knowledge of the details of the industry would not make. (I will give an example of this later.)

With simple descriptive data we could show that the patterns of firm entries, exits, and the number of producers over time showed substantial variation across the five major producer countries during the first 55 years of the industry's existence (see Fig. 1 taken from Murmann & Homburg, 2001). While France displayed a seemingly classic shakeout after 1862, the number of producers continued to rise in Germany until 1898 and then started to drop. If one has data only on the number of firms in the industry and no contextual knowledge, one could construct many equally plausible interpretations for the shakeout, for example, increasing returns to scale, pushing smaller players out of the industry. But in the case of the synthetic dye industry in France, the shakeout was caused in large part by a patent court ruling, which gave one firm, La Fuchsine, a monopoly. This firm used the police to shut down rival producers who sometimes set up shop across the border in Switzerland, where French law did not apply. The historical literature on the synthetic dye industry provided the details on the court case (van den Belt, 1992), which prevented me from misconstruing it as a shakeout primarily caused by increasing returns to scale. Here we already see how important the historians' emphasis is on getting facts right to safeguard against obviously false interpretations.

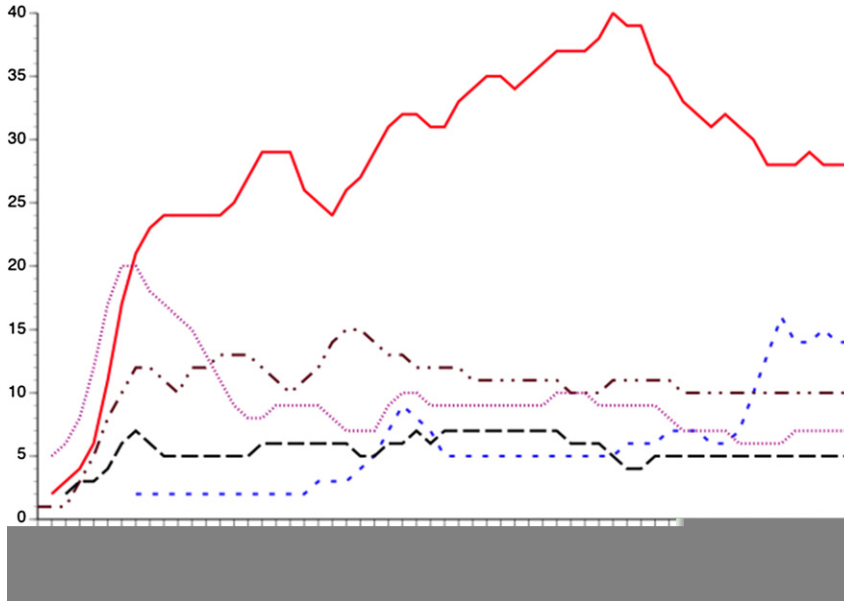

Fig. 1. Number of Synthetic Dye Firms by Country, 1857–1914. *Source*: Murmann and Homburg (2001). *Note*: To make the graphs easier to process for the eye, upon the request of the editor we reported 3-year moving averages in Murmann and Homburg (2001).

One other important advantage of using more than one case is that one can more confidently make causal inferences (Mahoney, 2000). If I had only studied the German synthetic dye industry and not compared it to Britain and the United States (Murmann, 2003) and later to Switzerland and France (Murmann, in press; Murmann & Homburg, 2001), it would have been more difficult to have confidence in my causal inferences.[8] For example, at the national level I cited the differences in university systems (which influenced the number of chemists available to start firms and later to staff R&D laboratories of existing firms) and patent laws as key causal factors behind Germany's success in the synthetic dye industry before 1914. Being able to show that Switzerland, the other country that became relatively more successful, also developed a stronger capability in training organic chemists, and that the other countries that became less successful, France, Great Britain, and the United States trailed Germany and Switzerland in that capability made the inferences about Germany more compelling. As far as

the patent systems are concerned, being able to show with detailed data that Switzerland rejected granting patents for synthetic dyes until 1903–46 years after the start of the industry—so that Swiss firms could freely copy German dye inventions, lent strength to the interpretation that Germany benefited from not having an effective patent for the first 20 years of the industry until 1877, while Britain, France, and the United States offered patent protection.

One of the key predictions an evolutionary theory of industry evolution is that firm failures are absolutely necessary for developing some successful firms. Analyzing the underpinnings of creative thought, Campbell (1960, p. 395) pointed out: "The […] variation-and-selection-retention model unequivocally implies that ceteris paribus, the greater the heterogeneity and volume of trials the greater the chance of a productive innovation. Doubling the number of efforts very nearly doubles the chance of a hit, particularly when trials are a small part of the total domain and the repetitiousness among trials is low. […] unconventionality and no doubt numerosity [are] a necessary, if not sufficient condition of creativity." A similar prediction can be made regarding industrial development: More start-ups, ceteris paribus, increase the odds that some firms will be successful. While historians in different countries had written in detail about the synthetic dye industry, there had never been a systematic effort to collect data on all the firms that participated in the industry before 1914 to test such a prediction for the early history of the industry. One of the reasons why I collected with Ernst Homburg data on all the firms in the world before 1914 is precisely to be able to test whether this theoretical prediction held true in this industry and could explain why Germany overtook Britain and France and dominated the industry for decades. As you can see from Row 1 in Table 1, Germany's industry dominance was indeed built on having both a larger number of start-ups in all periods except for the first 4 years of the industry and a larger number of failures. Notice that I am turning the small-*N* case comparison into a much larger *N* comparison by focusing on all the firms within a country, although we know relatively little about all the firms except for what products they offered and when they entered and exited the industry.

To be able to verify the causal factors that I claimed operate at the country level and to account for country-level performance differences in this industry, it would have to be the case that causal processes impacted differentially on individual firms operating in different national contexts. For this reason, I conducted six detailed case studies of a winning firm and a losing firm in three countries: Germany, Great Britain, and the United States (see Murmann, 2003, Chapter 3). In the case of Germany, I was able

to select two firms (Bayer and Jäger) that started in the same town (Elberfeld, now a part of Wuppertal). The firms also had the same background, namely the trading of natural dyes. This matched comparison held constant environmental factors, allowing me to isolate with greater confidence that managerial actions mattered for performance. (For a similar reason the contribution by Kipping & Westerhuis, 2012, in this volume, carefully selects two Dutch banks in the same country in order to be able to isolate the causal factors that led one firm to adopt the M-form earlier than the other firm.)

In analyzing what factors discriminated between the successful and unsuccessful firms in the three countries (even though the successful firm in Germany was a lot more successful for a lot longer than its British or American counterparts), three factors stood out: aside from making investments in Chandlerian organizational capabilities (in-house R&D, marketing, management, and manufacturing capabilities), those firms that went international earlier were more successful in the long run. But most importantly in the present context, the successful firms (Bayer, Levinstein [in Britain], and Schoellkopf [in U.S.], compared to the unsuccessful ones (Jäger, Brooke Simpson and Spiller [Britain], and American Aniline) secured better access to the centers of the organic-chemical knowledge network by recruiting students who had obtained their doctoral degrees in Germany. I thereby found support for the national-level argument that the German industry outperformed Britain and the United States because Germany became the leading country in organic chemistry. What differentiated the British and American successful firms from their less successful national counterparts was that they tried to overcome the problem of having an inferior national organic chemistry capability by hiring chemists from Germany, in the case of Levinstein in Britain, or by sending the son of the founder to Germany to study organic chemistry and acquire critical knowledge for running a synthetic dye business. Thirty percent of my 2003 book was devoted to tracing the entire life histories of the six firms until 1914, if they existed that long, because this provided the opportunity to detect national-level causal processes in the details of the firm histories. If I had not been able to confirm at the firm level that access to the German centers of organic chemistry mattered for performance, the country level explanations would have looked very suspect.

The strength of the historical method is precisely that it looks for evidence in all sizes and shapes to put together the most accurate account of what happened and why. I find it useful to draw an analogy between

social science research and detective work. A good detective will not rely alone on DNA evidence that most advanced scientific methods from biology make available. She/he will always seek to see the crime scene, to interview not only the suspects, but also other people close to the crime scene or familiar with the suspects. In the case of historians, people are typically dead, so one needs to rely on written accounts. Some of the most useful empirical descriptions of what happened in the synthetic dye industry before 1914 were written by industry participants. Heinrich Caro, the first research director of BASF, wrote one of the best historical accounts of how Germany overtook France and Britain in the dye industry (Caro, 1892). Similarly, Carl Duisberg, one of Bayer's first research chemists, who later became the firm's CEO, left a sizable body of speeches and writings including an autobiography (1933) that provided me invaluable insights into how Bayer managed to outcompete every other firm except for BASF and Hoechst in this industry before 1914. Clearly, one needs to be cautious about the potential bias of any single participating observer. Just like a good detective, one is always skeptical of the accounts of single individuals and treats them as interesting hypotheses to be confirmed by other sources of information. While any one industry or academic participant may display a biased view of events, reading the accounts of many participants allows one to piece together an account where the individual biases largely cancel each other out.

What is true of historical research in my view is true of strategy research as well. In the end what makes an empirical argument compelling is a patchwork of different pieces of evidence that can include econometric analyses. But relying only on publicly available datasets and econometric analyses to determine causal relationships, as is often done in strategy research, is a fraught with danger. If one does not know anything about the context (as is the case if one only codes a trade directory), it is very easy to mistake correlations for causation because no alarm bells go off, as is the case with researchers who know something about the context.

In my view, strategy research would benefit if a larger number of studies would marry social science with historical methodologies as I articulated in the preceding pages. If we had more comparable research, we could start building up empirical generalizations from well-researched case studies of industry. In the next section I will put forward a proposal on how we could speed up the development of these industry case studies.

OUTLINING A COMPARATIVE INDUSTRY STUDY PROJECT

Evolutionary economists (Dosi, 2000; Nelson & Winter, 1982; Saviotti & Metcalfe, 1991) have articulated powerful theories about how industries change. During the last 20 years economists, sociologists, and business school scholars have begun to study empirically how particular industries develop over long periods of time. For example, Steven Klepper (2008) has studied the entry and exit patterns of firms in a number of different industries. He explicitly tried to make systematic comparisons across industries, asking why some industries experience a shakeout while other industries do not. Many other scholars have studied one particular national industry, focusing on particular aspects of the industry's evolution. Because scholars frequently do not analyze the same aspects (variables), it is often difficult to compare systematically across industries and figure out what causes lie behind the similarities and differences in patterns of industry evolution. The comparative effort by Mowery and Nelson (1999), which brought together authors of seven industry studies, showed that it would be fruitful to engage different industry studies with one another.

To make it easier in future studies to compare across industries and come up with causal explanations, it seems expedient to formulate an analytical framework, that is, a common list of characteristics that future studies could trace. The few existing cross-national comparisons (Chesbrough, 1999; Murmann, 2003; Murmann & Homburg, 2001) have highlighted that institutional differences can lead to very different patterns of industry evolution. Hence, a framework is called for that combines concepts from traditional industrial organization economics, evolutionary economics, innovation studies, and institutional theory broadly defined. Because industries within different national environments are also typically connected through flows of trade and investment, it is also vital to add a cross-national, integrative component that is able to detect cross-border interdependencies. Since the performance of an industry in one country often interacts with the performance of the same industry in another country, it is useful to find out, for example, whether these interactions create systematic differences in the timing of shakeouts.

There is yet another compelling reason for formulating such a framework. Business and economic historians are the custodians of a large empirical literature on how industries and organizations within industries have changed over time. When these scholars write up their own studies, they often do not provide all the evidence that an evolutionary economist or

like-minded analyst would like to know for their analytical purposes. Articulating a common set of features to compare, contrast, and provide integrated explanations across industries would have the additional benefit of being able to recruit business and economic historians to share information they may have in their files but never published when they wrote about a particular firm or industry.

PRELIMINARY SKETCHES OF FRAMEWORK

The framework should be able to address two questions at the same time: Firstly, why do patterns of development in the organization of an industry differ? Secondly, why do patterns of development differ for the same industry in different countries? Klepper, for example, has shown that a shakeout in the number of producers started in the automobile industry already after 9 years (2007). But he also showed that there was no shakeout in the laser industry after 33 years (Klepper & Sleeper, 2005). Why? In the synthetic dye industry before 1914 Homburg and I (Murmann & Homburg, 2001) showed significant differences in national patterns of evolution. Do these differences exist in all industries or are they limited to a few industries? What explains these differences?

My initial work on the synthetic dye industry stopped in 1914 because World War I completely changed the industry dynamics. When the war broke out, the German government blocked German firms from supplying key foreign markets. As a result, some of the major consumer countries of dyes such as the United States no longer had enough dyes to color clothing, prompting the government to try to stimulate local production. Governments also discovered during the war that synthetic dye plants could be readily converted to making explosives, allowing Germany to ramp up its explosives production much faster than other countries that lacked large synthetic dye facilities. As a result, governments in Britain, France, the United States, and Japan took active steps to build significant national dye industries and afford these industries' protection after the war was over, so German firms would not wipe out local industries given their superior capabilities built up over 50 years.

The case of the United States after 1914 is instructive. Fig. 1 shows that the United States, unlike the other four major producer countries, experienced a rise in the number of industry participants just before World War I. The disappearance of German imports and the government's action to create a local industry had dramatic effects. In 1914, the

Murmann–Homburg database shows 14 firms producing dyes in the United States. According to Welsh (1944, p. 185) by 1918 the number had risen to 90. Then it started to fall to 49 by 1929 and to 29 firms by 1939, with five firms accounting for approximately 90–95% of total output (p. 186).[9] At the same time, the entire Germany dye industry was merged into one firm. This was possible in Germany because at the time the country allowed cartels that were illegal in the Unites States. What these numbers show is that World War I was an external event that reset the clock of industry evolution in the United States but not in Germany. This leads me to this general question: Under what circumstances show national industries the classical singular shakeout pattern versus when does the same industry[10] experience again a rise of producers after a shakeout?

After World War II, India and, after 1978, China became producers on the global synthetic dye market, initially fueled by the rise of their domestic textile industry. Industrial dye production started in India in 1956. Under strict state regulation, 20 firms started production during the first 10 years (Mandal, 2006). After 1974, the industry was deregulated, allowing many new firms to enter the industry on a small scale. India had 400 dye factories in 1985 (Mandal, 2006, p. 17) and 600 in 2005 (Mandal, 2006, p. 24). Similarly, in China an estimated 500 to 800 dye firms entered the market after liberalization of the economy (Jiang & Murmann, in press). Even in Germany the largest number of firms ever operating in the industry never reached more than 40. So what explains that the number of firms participating in the industry was so much higher in India and China than in any other producer country in the long history of the industry? At present I don't know enough to have well-founded answers. I simply present these numbers here to paint a vivid picture of what kinds of questions a comparative industry study framework will need to address.

Even more than previous work, the comparative industry study framework should focus on how producers are linked to users, how new users are brought into the market, and how the speed of market growth in their home environment affects the long-term global positions of firms that started in different countries.

Given the space limitation of the present chapter, I cannot go into articulating all the important questions that should be addressed. I will simply confine myself to presenting an outline. Here is the beginning of a list of candidate variables aside from the traditional industrial organization economics variables (size of market, rates of market growths, number of producers, export, import and international investment patterns, market shares in different countries, profit rates, cost structures, entry and exit rates

of producers, capital intensity, etc.) that could form the core of the comparative framework: How big are the economies of scale or scope, and are they increasing or decreasing over time? What is the unit value and value per unit weight of the product? How numerous are the customers of the industry? What are the salient characteristics of users and how do they evolve over time? What is the frequency of product and process innovation in the course of the development of the industry? What is the frequency of innovations in organizations and in the institutional and regulatory frameworks shaping the evolution of different industries in different economies? How many of the skills needed by firms in the industry are created inside the firms and how many are created by external educational institutions? How are skills transferred from one producer to the next? What factors determine export versus international investment decisions? Does government have many policies tailored to the particular industry? How have policies in different countries shaped the evolution of the industry on a world scale? How did the regulatory pattern in the industry develop? What are the strategies and structures of firms that become the leading producers?

THOUGHTS ON THE PROCESS

In formulating a comparative framework it is necessary to balance the desire to develop a comprehensive framework (i.e., identifying all the causal mechanisms) with not having an endlessly long list of variables that would make these comparisons unwieldy. Thus, it should focus on some core evolutionary/managerial/policy questions. It also seems important to articulate what features of industry evolution are readily tracked quantitatively and what aspects call for more qualitative descriptions. I have created a beta version of the framework in Table 2. I invite other scholars to work me with on refining it. We should then subject it to a trial-run on a few industries, ideally where much of the material is accessible relatively quickly through secondary sources, and then refine it further.

Ideally, all industries should be traced from their beginning. For some industries this is easily feasible because the data is readily available after a bit of digging. For other industries only particular periods can be readily documented. Individual scholars may only possess part of the data on a particular industry. But collectively different scholars may be able to bring together all the data called for by the framework. For this reason, I have created a collaborative platform (called EEpedia on economic-evolution.net), which contains the beta version of the framework and where different

Table 2. Comparative Industry Study (CIS) Framework.

	Country	World	Firm
Quantitative variables			
Demand	Size of market	Size of market	Sales
	Rates of market growth	Rates of market growth	Sales growth
	Imports	Imports	
	Number of consumers	Number of consumers	Number of customers
Supply	Number of producers	Number of producers	Variety of products offered
	Entry/Exit rates of producers	Entry/Exit rates of producers	Date of production start
	Concentration ratio	Concentration ratio	Market share Percentage of sales in particular industry
	Exports	Exports	Exports
	Cost structure	Cost structure	Cost structure
	Capital intensity	Capital intensity	Capital intensity
	Frequency of product and process innovations	Frequency of product and process innovations	Frequency of product and process innovations
	Capacity investment rates and distribution	FDI and portfolio control	Capacity investment rates
Finance	Profit rates	Profit rates	Profit rates
	Size of foreign direct investment	Size of foreign direct investment	Size of foreign direct investment
	Share of FDI of all investments	Share of FDI of all investments	Share of FDI of all investments
	Source of funds	Source of funds	Source of funds Investment in R&D
Qualitative variables			
Users	What are the salient characteristics of users and how do they evolve?	How diverse are the needs of users across countries?	What user segment is served? How does this change?
	How do producers find out about users' needs?		How does the firm find out users' needs?
Products	What is the type of product or service (final consumer good, intermediate good, primary good, standalone product, subassembly, component in system)?		How do products reach the users; does the firm market and sell directly, or are other organizations involved; are there changes?
Production	How are production skills formed (internal, other firms, or external organizations)?	Is global production concentrated in few countries?	What prior experience did the firm have?
			What factors determine export vs. international investment decisions?

Table 2. (*Continued*)

	Country	World	Firm
Policies/ regulations	Does government have many policies/ regulations tailored to the industry; do they have a demonstrable effect on country competitiveness in the industry?	What trade regimes exist and how do they change?	What is the strategy of the firm? What kinds of policies (routines) does the firm develop for its operation?
	How do policies/ regulations change over time?		What is the relationship among policies (routines)?
Supporting institutions	What is the role of trade associations and how do they change over time? Are there any other institutions that are crucial for the industry?	Are there any supranational nonfirm actors (e.g., UN, WTO)?	Does the firm have specific alliances with other actors?

scholars can deposit their findings. The hope is that with the help of the website and e-mail, the data collection on a particular industry can be done to some extent by virtual teams from all over the world.

DISCUSSION

When management scholars in the 1970s and 1980s endeavored to institutionalize strategy research as a separate field in business schools, they felt an imperative to imitate the then-reigning ideal of a social science in management to gain legitimacy for the subfield. Built on the positivist model of a natural science, this conception of science seeks to discover Newtonian-type universal laws (e.g., $F = m \times a$) independent of time and place by reducing social phenomena to abstract numbers and running econometric analysis of representative datasets to find the coefficients that relate a few explanatory variables to the outcome to be explained. This chapter argues that while econometric analyses are powerful tools in the mature final stages of a research problem, they typically have very limited power during the long period of research when one is trying to build an understanding of a social phenomenon. During this stage it is generally more productive to

build deeper conceptual understanding by carrying out detailed empirical case studies about the causal processes driving a phenomenon (for an excellent example, see Danneels, 2011).

If one wants to look for a role model in natural sciences, it is the historical branches of natural science such as biology and geology that can serve as much better inspiration than Newtonian physics. Understanding how firms gain and lose competitive advantage in a larger industrial context that is changing quite dramatically within short periods of time requires contextual knowledge. The craft of historians has always been to place action in context, and this chapter argues that the field of strategy would be better off if more scholars would use historical methods because that would allow us to build generalizations from the ground up instead of trying to find Newtonian laws that either don't exist in strategy or are trivial (e.g., all profitable firms take in more money than they spend).

To give an illustration of how one can productively marry history with social science research in strategy, I described the methods I used in Murmann (2003). Since it is so easy to be misinterpreted, I would like to emphasize: I am not advocating that all strategy research should turn historical. My argument is more nuanced. I simply contend that the relative frequency in which different research methods are presently used in strategy research is leading to suboptimal outcomes. We need more careful empirical research based on case studies and small *N*-comparisons to articulate more clearly how firms gain and lose competitive advantage. When we have articulated with the help of these case studies more precisely the causal mechanisms and when we know more about boundary conditions where they apply, then it is fruitful to design large *N* studies that test more precisely stated theoretical statements.

The chapter advocated a comparative industry studies project that is in part inspired by the power of the Wikipedia model (Giles, 2005). Business historians reading these lines may fear that my proposal for a comparative industries studies project devalues the writing of traditional business histories of individual firms. That is not my aim and it would be unfortunate if I am understood that way. I simply wanted to point out that business history can have additional impact if we find a mechanism to more readily compare findings across different authors.

The comparative framework sketched in the preceding pages is only a start. We may find that important aspects of industry dynamics have been missed and we need to add them to the comparative framework. I am not a fan of preaching about research instead of doing it. In the next few years I will attempt to organize the study on an industry in open source way

articulated in this chapter and validate that the concept can work in the realm of strategy research. Perhaps starting out with an industry that was recently born (e.g., solar industry) is the best way to involve many people, including industry participants who are eager to see the history of their industry documented. I have established a collaborative platform called EEpedia on Economic-Evolution.Net[11] to facilitate interaction among scholars. Would it not be fantastic if in 50 years we had something like the HARF in anthropology, allowing every university student in business to study industry dynamics by comparing the dynamics across particular industries of interest?

NOTES

1. A good example about fighting about facts occurred in the literature on transaction cost economics. See the debate about why General Motors merged vertically with Fisher Body in 1926 (Casadesus-Masanell & Spulber, 2000).

2. Precisely for this reason historians writing about a particular company often compare the firm under study with other firms in the industry and describe how typical or atypical the behavior of the firm was in the industry at a particular point in time.

3. I mean the term "variable" here to include qualitative features.

4. More information on the FIVE Project can be found here: http://five.dartmouth.edu/

5. More information on the Industry Studies Organization can be found here: http://www.industrystudies.org/

6. I invited the e-mail list members of the Business Policy and Strategy Division of the Academy of Management to complete a short survey to find out what scholars think about this issue. I am reporting here the responses of self-identified professors (112 out of the 154 respondents): 85% of professors believe that the time required to qualify for a sustained competitive advantage is 10 years or less; 54% think it is between 2 and 5 years; and 33% picked 5 years as the time required. The precise wording of the survey and its full results are available here: http://jpm.li/5

7. You can find the tables I prepared in comparing the major theories here: http://jpm.li/6

8. It is important to emphasize here that single cases can be quite easily turned into multiple cases by shifting the unit of analysis downwards. A single-country case can be – and often is – turned into multiple cases by studying the same phenomena in different regions within the country. A single firm study can also be decomposed into multiple cases by looking at different divisions or projects. Sophisticated practitioners of case studies frequently make the move of showing that explanations at the country level are empirically confirmed at smaller geographic units within countries. Hence one needs to be cautious to accuse case study practitioners that they only have an N of one because when you look into the details of the study a much larger N may become apparent.

9. At present I don't know how many of the five firms cannot be traced back to firms that existed in 1914. But I know for sure that DuPont was a new entrant in the industry, acquiring a significant position.

10. What counts as the same industry is theoretically not a straightforward question. Hannan, Pólos, and Carroll (2007) spent the past decade trying to bring some theoretical rigor to this question. Empirically, it is often quite easy to identify industries because participants typically will form a trade association and governments categorize firms into an industry for regulatory and policy purposes.

11. More information at http://economic-evolution.net

ACKNOWLEDGEMENTS

This chapter was largely written while I was the R. Graham Whaling Visiting Professor of Management at the Wharton School, University of Pennsylvania. I would like to thank the participants of the conference for the volume at the University of Chicago and the three editors for useful comments previous versions of the chapter.

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