In Search of Best Practice

Enduring Ideas in Strategy and Innovation

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Other Books by Michael A. Cusumano

The Business of Software (2004)

Platform Leadership (2002, with Annabelle Gawer)

Strategic Thinking for the Next Economy (2001, edited with Costas Markides)

Competing on Internet Time (1998, with David B. Yoffie)

Thinking Beyond Lean (1998, with Kentaro Nobeoka)

Microsoft Secrets (1995, with Richard W. Selby)

Japan's Software Factories (1991)

The Japanese Automobile Industry (1985)

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Preface

In many ways, this book began when I first stepped into a college classroom in 1972. College presented an opportunity to learn about how the world I saw around me – our Western approaches to politics, economics, and society, as well as science and technology – had come about. I quickly decided to major in history and would eventually take my senior year departmental exams in a field called the "History of Ideas." In my junior year, at the suggestion of my fall-semester advisor, Professor William Jordan, I decided to take advantage of my Spanish language skills and write an undergraduate thesis on why the 18th-century European Enlightenment had failed to take hold in Spain. This thesis experience then convinced me to study in the future not a nation that had declined and needed "apologists," but a society on the rise that needed people to explain what was happening there and why.

So, after graduating in 1976, I applied for and was lucky to receive a fellowship from the Princeton-in-Asia Foundation to go to Japan to teach English for two years and study the Japanese language. I had never been outside the United States before. While in Japan, I did what I had done regarding Europe, and read as much as I could about the country's history, literature, economy, and society. It then occurred to me that a good topic for graduate school would be to compare Japan's own successful "enlightenment" in the 19th century and subsequent modernization to the experience of Western countries – and try to understand why Japan succeeded whereas a country such as Spain had languished. I was again fortunate to gain admission to an interdisciplinary doctoral program in East Asian history and language studies.

In my second year of graduate school, at the prodding of my next advisor, Professor Albert Craig, I decided to specialize in Japanese management and business history. I then spent more than three years based at the University of Tokyo and completed a Ph.D. thesis for Harvard University on the development of the Japanese automobile industry, focusing on technology management and strategy at Nissan and Toyota. I included comparisons to General Motors, Ford, and Chrysler, which already had fallen behind the Japanese, especially Toyota, in productivity and quality, as well as the management of people, suppliers, sales, and probably everything else. At this point, I was much more in the field of operations and technology management or strategy than Japanese business studies. Consequently, after completing my degree, I was fortunate again to spend two years as a postdoctoral fellow in production and operations management at the Harvard Business School. Then I joined the MIT Sloan School of Management in 1986 as a joint member of two groups: Strategy and Technological Innovation and Entrepreneurship, which merged in 2009.

In 1985, I shifted the bulk of my new research to the computer software business and the evolution of software engineering practices. The reason was that I considered software the next great challenge for Japan to master and a technology that, I was sure, would change the world if only we could make software development less of an art and more like science and engineering. I had been exposed to computers briefly in college (where I once used an IBM 370 mainframe to do word processing on punched cards) and in graduate school (where I took some short courses on computing systems and applications in order to use another IBM mainframe to write my Ph.D. thesis). On the research end, I quickly became engaged in detailed studies of Japanese software factories and product development in the Japanese video-recorder industry, and then a range of other topics and companies, from Microsoft and Netscape to Intel and Cisco as well as innovation in services and business models.

In December 2005, another fortunate event occurred: I received an invitation to deliver the 13th Annual Clarendon Lectures in Management Studies at the University of Oxford in 2009 and, as part of the fellowship, write a book based on these lectures. After consulting with members of the awards committee, including Professor Mari Sako from Oxford's Said School of Business and David Musson from Oxford University Press, we decided that an overview of my research would be the best book to write. This advance warning gave me ample time (as it was intended) to think about how to describe what I have learned in my career. I like to think that this book has become much more than an overview because, unlike my prior work, the focus has turned out to be less on history or empirical facts and more on *ideas*. It took a couple of years of pondering, and a lot of thinking about how Toyota and Microsoft, which had occupied a lot of my time, were both similar and different. But the ideas emerged as it became clear to me that, in college, graduate school, and as an academic, and while working as a company consultant, advisor, and director, I really have been concerned with a particular pursuit: understanding and applying best practice.

How well I have succeeded is a different matter, but my challenge for the Clarendon Lectures has been to describe what I have learned. The resulting book is not a comprehensive review of the most valuable things we know about management and organizations, an enormous task because of the many subfields and specializations. The thousands of articles and books written on management theory and practice would require several lifetimes to survey. I have simply organized my lectures and this book around a handful of potentially "enduring" ideas that, at least in retrospect, have been central to my research and that of my co-authors. Perhaps most importantly, I have selected ideas that I believe have direct value to managers <u>and</u> which have been the subject of years of rigorous empirical and theoretical study by academic researchers in a variety of disciplines.

Introduction:
The Search for Best Practice

Anyone who has ever thought about what is the best course of action to take in order to create and build a new product or deliver a new service, or organize a small group of people and then manage operations of any scale, is probably thinking about best practice in management. It is not a simple topic. Companies such as General Motors and nations such as Japan once embodied best practice but have seen their fortunes change dramatically. IBM once dominated the computer industry, and then gave way to Microsoft and Intel. Now we look to newer competitors such as Google, Facebook, and Twitter, and sometimes to older innovators such as Apple, for leadership in information technology. Nations such as China and India now rival or surpass the United States and Japan, at least in certain industries. No one knows which ideas, firms, or countries will dominate business in the years to come. So what are best practices in management? Is there any such thing? Why is it so hard to identify ideas or practices that can stand the tests of time and geography? Tackling these questions, largely from the viewpoint of my research and personal experiences over what is now nearly thirty years, is the subject of this book. The primary audience is any manager interested in leading-edge research on best practice and any researcher, student, consultant, or manager interested in leading-edge practice.

Understanding best practice is especially important now because managers in this century face an extraordinarily difficult challenge: *We have entered an age of simultaneous innovation and commoditization, increasingly enabled by digital technologies.* Customers around the globe continually demand new types of products and services, but too often they want to pay no more than what Google charges for searching the Internet: *nothing.* As a result, many companies struggle to offer their products and services at low and often declining prices. This has become true in conventional manufacturing such as automobiles, where the most efficient producer in the world, Toyota, has at times reduced prices in recent years and announced its first ever operating loss in 2009. But the combination of innovation and commoditization has occurred most noticeably in businesses touched by computers, microprocessors, and other components of information technology. Consumers now realize that the marginal cost of reproducing a software program or a digital text, video, or music file, or sending a telephone call or message over the Internet, is essentially zero. Yet most of the value in today's high technology world as well as information products ranging from books to newspapers comes in the form of easily reproducible software or digital content and services. How can companies adapt to such rapid change and still make enough money to survive and thrive? That is the extraordinary challenge.

The world is dramatically different today than it was even a few years ago. For products and services relying on electrical and then electronic components, this dual trend of commoditization and innovation accelerated in the 1960s and 1970s with the increasing application of transistors and then microprocessors. With the dawn of the PC era from the mid-1970s, the "price-performance" of computers and other programmable devices fell rapidly, even as reliability, functional sophistication, and ease-of-use increased. Many software products, such as for large organizations, held their value until recently. But then the rise of free and open-source software distributed through the Internet caused many of these prices to collapse as well in the late 1990s and early 2000s. The bursting of the Internet bubble in 2000 and then the worldwide recession of 2008-2009 merely has exacerbated long-term trends that threaten the very existence of many firms around the world.

Globalization is a big part of this story. In the 1960s and 1970s, dozens of Japanese firms entered manufacturing and high-technology markets and drove down prices while gradually offering increasingly innovative products. Korean firms became major players in the 1980s and 1990s in sectors ranging from semiconductors to consumer electronics. More recently, Chinese and Indian companies have come to compete effectively in key segments of manufacturing and high technology. The Chinese now can make nearly any product, exemplified by computers from Lenovo as well as telecommunications equipment and software from Huawei. China Mobile is now the world's largest cell-phone services provider. And China is now the third largest automobile-producing nation (behind Japan and the United States) and the world's second largest motor vehicle consuming market (after the United States). China's costs, or more precisely, its prices, have become the world's prices for most manufactured goods and many high-technology products. At the same time, Indian firms have learned how to deliver many high-technology services, initially starting with back-office operations but now extending to custom software development and complex product engineering. India also is becoming a major automobile producer. India's costs, or its prices, have become the world's prices for many sophisticated professional services. It is no surprise that managers in the developed world – the United States, Europe, Japan, and elsewhere – now ponder how to continue innovating in technologies whose prices can fall dramatically, sometimes to zero.

Enduring Ideas

The best way to survive and thrive in such a dynamic, competitive world – particularly when business cycles and economic calamities worsen commoditization tendencies – is to understand what makes companies successful over periods of years or even decades. This means distinguishing between short-term fads in management thinking and concepts that have stood the tests of time, practical experience, and academic scrutiny. To be truly useful, though, these ideas must help managers to anticipate or at least respond to quickly as well as effectively to change – such as when dealing with rapidly declining prices or dramatic as well as subtle shifts in value from hardware to software and services.

My research generally has looked at how firms use strategy, structure, and process to balance efficiency and flexibility in manufacturing as well as product development and other operations. In trying to synthesize what I have learned, I concluded that a handful of ideas – I have chosen six – appear to have been essential to long-term competitive advantage. Other authors have discussed each of these ideas before me; in fact, as noted in the Preface, I have focused mainly on concepts supported by considerable theoretical and empirical research undertaken by a variety of scholars in different disciplines. Nor do I try to cover all or even most areas in management. What I offer is a selective list, formed through the lens of my research, including studies done with students and colleagues, and my personal experiences. My examples come mainly from the automobile, computer software, consumer electronics, and computer hardware industries. I devote a single chapter to each of my ideas. What follows below is a brief synopsis.

1. Capabilities, Not Just Strategy

<u>The Idea:</u>

Best practice should include a focus not simply on formulating strategy or a vision of the future (i.e., deciding what to do) but equally on building distinctive organizational capabilities and operational skills (i.e., how to do things) that rise above common practice (i.e., what most firms do). Distinctive capabilities center on people, processes, and knowledge that reflect a deep understanding of the business and the technology and enable the firm to offer superior products and services as well as exploit foreseen and unforeseen opportunities for innovation.

The main problem addressed here is that too many managers rely on strategy to differentiate their firms without making the more difficult, longer-term investments in operations and other in-house skills that make strategies successful and can be essential to both product and process innovation. I describe more of the thinking behind this idea as well as some of the relevant literature that has influenced me in Chapter 1. Then I illustrate the idea with three examples. To begin, I return to the case studies in my first book, *The Japanese Automobile Industry: Technology and Management at Nissan and Toyota* (1985). The founders of both Nissan and Toyota had a vision to establish a domestic automobile industry in Japan during the 1930s. Toyota decided to learn how to design and produce automobiles on its own rather than follow Nissan's lead, which decided to important American designs and mass-production technology directly. Toyota ended up with strong internal skills for product development and manufacturing, and created a unique approach to production management that revolutionized mass-production practices, which I will discuss in Chapter 2. The second case involves the incremental development of the home video-cassette recorder (VCR) by Japan Victor Corporation (JVC) and Sony compared to other companies in Japan (Toshiba) as well as the United States and Europe (Ampex, RCA, and Philips) – a topic I studied with Richard Rosenbloom for a 1987 article in *California Management Review*. The main issue here is why only a few firms were able to succeed in this quest while other firms invented the basic technology and were able to design home versions of these products in their laboratories but not mass-produce them. Finally, I turn to the early history of Microsoft, relying on my book *Microsoft Secrets* (1995), done with Richard Selby. I review how Bill Gates' vision of an emerging future of personal computers and software to run those new machines led to distinctive capabilities in software product development as well as platform-based competition, which I discuss in more detail in later chapters.

2. Pull, Don't Just Push

The Idea:

Best practice should include a "pull-style" of management that reverses the sequential processes and information flow common in many firms for manufacturing as well as product development and applied research. The goal should be to link each step in a company's operations and development activities (except in basic research) backward from market demand or customer requirements. When implemented consistently, a pull approach can help an organization make continuous adjustments and incremental innovations by responding almost instantly to feedback on changing customer needs, new technology, competitive conditions, or operational difficulties.

The main problem addressed here is that too many firms have no way to incorporate feedback from customers, competitors, suppliers, partners, or even internal operations while they are in the process of developing new products or managing production and service operations. It is too easy for managers and employees to wait until after they have completed a project or a production cycle before thinking about change and improvement. I describe more of the thinking behind this idea as well as some of the relevant literature that has influenced me in Chapter 2. Then I illustrate the idea with two examples. First, I return to my prior research on Toyota but this time I review the basic concepts of its Just-in-Time system, compared to conventional "push-style" mass production, as described in *The Japanese Automobile Industry*. Then I go back to the Microsoft story as described in *Microsoft Secrets* and related articles. I examine the analogy of a pull system in product development – the iterative development process pioneered in PC software firms and now used commonly throughout the global software industry. The comparison here is to more "waterfall-ish" or sequential development processes emphasized in the mainframe computer industry and still common in many hardware and software companies today. There are similar disadvantages to mass production in that waterfall-ish projects are more likely to build products that do not meet customer requirements or rapidly changing market needs. Note here that I am talking primarily about how to manage incremental innovation in product development, and not prepare for truly radical or disruptive technological change.

3. Scope, Not Just Scale

<u>The Idea:</u>

Best practice should include the ability to achieve efficiencies even in activities not suited to conventional economies of scale, such as research, engineering, and product development as well as services. Scope generally requires sharing knowledge and experience, as well as inputs, intermediate components, and other key elements of production or service delivery across seemingly diverse activities, groups, and projects.

The main problem addressed here is that too many managers focus on getting big and deriving relatively obvious efficiencies from scale, without pursuing the more complex but equally powerful efficiencies that can come through scope economies. Scope economies are more complex to achieve, and can place some constraints on the firm if not managed properly. I describe more of the thinking behind this idea as well as some of the relevant literature that has influenced me in Chapter 3. Then I illustrate the idea with two examples. I begin with a discussion of the software factory, used both for custom projects and product development, as it was first proposed in the United States and then refined in Japan and later in India. I rely primarily on research described in my books *Japan's Software Factories* (1991) and *The Business of Software* (2004). Second, I look at product development in the automobile industry, focusing on techniques aimed at reusing key components and "in-house platforms" through coordinated, overlapping projects that balance the benefits of dedicated project teams with shared technology. The latter "multi-project management" style, described by myself and Kentaro Nobeoka in our book, *Thinking Beyond Lean* (1998), was done especially well at Toyota and other Japanese automakers in the 1980s and 1990s in order to expand sales rapidly and improve their product lines while minimizing engineering and manufacturing costs.

4. Flexibility, Not Just Efficiency

<u>The Idea:</u>

Best practice should include as much emphasis on flexibility as on efficiency in manufacturing, product development, and other operations as well as strategy and entrepreneurship, with the objective of creating effective structures and processes that accomplish strategic goals while minimizing tradeoffs with efficiency or focus. Manages should cultivate flexible capabilities and decision making skills in order to anticipate or respond quickly to changes in market demand, customer needs, technology, competitive threats, and foreseen as well as unforeseen opportunities for innovation.

The main problem addressed here is that managers too often neglect the short-term and potentially longer term benefits of investing in flexibility, and adopt practices that inhibit the ability of the firm to respond to new information from the market or ongoing activities. I describe more of the thinking behind this idea as well as some of the relevant literature that has influenced me in Chapter 4. Then I illustrate the idea with three examples. I begin with a brief review of what flexibility means in manufacturing, such as at Toyota, and then focus on a relatively large-sample study of printed-circuit board manufacturing done with Fernando Suarez and Charles Fine. Our goal was to examine the different ways of achieving flexibility and whether or not there were in fact tradeoffs with efficiency, or quality (note: we did not find any, nor has Toyota). For product development, I return once more to the example of "iterative" versus "waterfall-ish" software development and research done initially for *Microsoft Secrets*. I highlight specific techniques that enable "large teams to work like small teams." I also review findings from a study of software projects at Hewlett Packard and Agilent, published in 2003 with Alan MacCormack, Chris Kemerer, and Bill Crandall. Finally, I turn to the importance of flexibility in strategy and entrepreneurship, where it often takes a number of tries to get the business model and the product offerings right. In this last discussion I rely on the Netscape case from my book with David Yoffie, *Competing on Internet Time: Lessons from Netscape and Its Battle with Microsoft* (1998).

5. Platforms, Not Just Products

<u>The Idea:</u>

Best practice (at least, for firms affected by digital technologies as well as "network effects" more broadly) should include the ability to move beyond conventional strategy and capabilities to compete on the basis of platforms, or complements. A platform strategy requires an ecosystem that generates external complementary innovations and helps build "positive feedback" between the complements and the platform.

The main problem addressed here is that, while many markets today have become platform markets, managers still seem to think primarily at the product level or find it difficult to formulate a platform strategy. I describe more of the thinking behind this idea as well as some of the relevant literature that has influenced me in Chapter 5. Then I illustrate the idea with three examples. First, is a discussion of what is an industry-wide "platform," including key concepts such as complementary innovations and network effects, and how a platform strategy differs from a product strategy. I rely on a comparison of Sony and Apple (mainly product-oriented companies) versus JVC and Microsoft (mainly platform-oriented companies). Then I compare the platform-leadership model refined at Intel with other established platform leaders such as Microsoft. Finally, I discuss how relatively new firms can turn a product strategy into a platform strategy as well as help a market "tip" in their direction, with successful examples such as Google in Internet search, Qualcomm in wireless chips, and Linux in server operating systems. I also review numerous failed or ongoing platform battles. The analysis of network effects and “strategic maneuvering” in the VCR story relies on research published with Richard Rosenbloom and Yiorgos Mylonadis in 1992. The Netscape research I did with David Yoffie for *Competing on Internet Time*. The later research on platform leadership and wannabe strategies at Intel, Microsoft, and other companies I did with Annabelle Gawer for our book *Platform Leadership: How Intel, Microsoft, and Cisco Drive Industry Innovation* (2002) and follow-on articles.

6: Services, Not Just Products (or Platforms)

<u>The Idea:</u>

Best practice should include an ability to use service capabilities and innovations to sell, enhance, and even “de-commoditize” product offerings as well as create new sources of revenues and profits, such as an ongoing maintenance stream. The goals should be to “servitize” products to create new value-added opportunities and to “productize” services to deliver them more efficiently and flexibly.

The main problem addressed here is that too many managers in product firms see services as a cost center and a necessary evil, even though many products have become commodities and the major source of differentiation and value for customers may have already shifted to services. I describe more of the thinking behind this idea as well as some of the relevant literature that has influenced me in Chapter 6. Then I illustrate the idea with three examples. I begin with a discussion, based mainly on ideas in *The Business of Software*, of how the software *products* business – consciously or not – has largely become a hybrid products and service business over the past decade. I use examples from SAP and Oracle as well as other diversified systems firms such as IBM, Apple, Sun Microsystems, and Cisco, and newer companies such as Salesforce.com and Google. I also present some data, first published in 2008, on the impact of this change from product to service revenues on a large sample of firms. Next, I review how services can aid the business of a product company, with examples drawn from computer software and hardware, automobiles, and other industries. Finally, I discuss how platform leaders and wannabes might use services to enhance their platform strategies. All of the database and recent theoretical and empirical work on product and service business models I have done jointly with Fernando Suarez and Stephen Kahl. The platform and services work was done with Fernando Suarez.

The first four of my enduring ideas (capabilities, the pull concept, scope economies, and flexibility) all deal with “agility” and primarily aim to help firms anticipate, adapt, or respond quickly to unpredictable or rapidly changing market environments and competitive conditions. The term agility is similar to flexibility but also has connotations of quickness – both of which are important for firms to deal with rapid change or unpredictable markets. (Agility is also a term commonly used in software development as a contrast to the slow, sequential, and inflexible "waterfall-ish" way of doing things. Some members of the software community have even issued a set of precepts they call "The Agile Manifesto."[1]) This first four ideas have been around sufficiently long enough that I believe they have now become standard practices in the best companies in the world, though with different degrees of emphasis, depending on the firm and the market. The latter two ideas (platforms and services) are newer and less widely accepted. In many cases they also depend at least in part on technological innovation associated with information technology, especially computer software, digital content, and the Internet. These ideas may be more controversial, but they primarily aim to expand our views of the product firm beyond conventional modes of thinking about strategy, capabilities, and business models. Before going further, however, it is important to explain the thinking behind my approach to research, which has led me to select these particular ideas and illustrations.

The Research Challenge

Academics and consultants have written extensively about best practices in management and their relevance to competitive advantage, though much of this research raises as many questions as it answers. One major problem is that what seems to work for one company in one time period, industry, or national setting often does not work for other companies in different circumstances or even for the same company in another time period or a different industry. We must therefore face the possibility that, as I will discuss later in this section, "best practice" in a given area of management may not exist in any absolute sense – that is, for all firms, industries, and environmental or institutional conditions. For these and other reasons, to evaluate research and its relevance to best practices, managers and students of management need to be able to judge business research themselves and make their own assessments as to which ideas are potentially enduring and which are simply management fads or easy to imitate.

Another problem in the search for best practice is that many different styles of research exist and these variations can lead both to different insights and different conclusions. Each style of research has its advantages but usually produces an incomplete picture of a given phenomenon. Sometimes the academic lens of one methodological discipline – such as that of the economist, sociologist, operations researcher, or business historian – acts more like a "silo" and obscures a broader view of what is really happening (not unlike partially blind men touching different parts of an elephant). The obvious conclusion here is that no one approach to management research is likely to tell the whole story about what leads to competitive advantage. We need an eclectic combination of methodologies, qualitative and quantitative, and at different levels of analysis and abstraction (products, teams, firms, industries, nations, regions, etc.) to understand how firms perform and why.

Academics also must come to terms with the possibility that "practice" (i.e., firms), and not "theory" (i.e., academia) is the source of many or most of the great ideas in management. Theorists and empirical researchers may well be the first people to explain, document, or model a great idea, and there probably are some areas, such as financial modeling, or combinatorial optimization, where theory has led practice. But my experience studying firms ranging from Toyota, JVC, and Microsoft to Intel, IBM, and Google suggests that academics generally trail leading-edge firms in introducing the practices that end up winning markets – including all of the practices associated with my six enduring ideas.

The Case Study

Most academics in business schools at some time confront the *case study*, at least for teaching purposes. Harvard Business School, founded in 1908, pioneered business school education and research largely by adopting this method for teaching. A business school case is usually an in-depth descriptive look at a particular organization or managerial situation to illustrate one or more pedagogical lessons, though without giving the reader a complete answer. In fact, there often is no one right answer to the problem highlighted in a case study, so the professor guides students through a "Socratic" discussion, with the goal of improving their analytical and judgment capabilities.

I should note that my undergraduate and doctoral training in history were well-suited to appreciating the value of the case method as well as conducting qualitative archival or field research. Indeed, my doctoral thesis, which became my first book while I was a postdoctoral fellow at Harvard Business School in 1985, compared the strategies for technology transfer and product development, production and quality management, and labor relations at Japan's two largest automobile companies – Nissan and Toyota – from their origins in the 1930s through the mid-1980s. I later wrote or co-wrote several other books centered on detailed case studies of operations, product development, and innovation strategy at several Japanese computer companies that operated software factories (Hitachi, Toshiba, NEC, and Fujitsu) as well as at Microsoft, Netscape, and Intel. My co-authors and I suggested lessons that might represent broadly useful "best practices," but it was impossible to tell from the small numbers of examples. It is true that, in my study of Nissan and Toyota, I spent many months compiling numerical data in order to analyze things like changes in physical and value-added productivity over time and to compare productivity at the two leading Japanese firms with General Motors, Ford, and Chrysler. My case research, therefore, was both qualitative and quantitative. But my sample was still small and I did not perform any statistical analyses of the data, which I primarily used for descriptive purposes.

My research on the auto industry illustrates both the strengths and the weaknesses of cases and qualitative research. The ability to probe an interesting situation or a leading-edge organization deeply is the great strength of the case method, especially when extended to book length. A deep analysis of one firm with a proven record over a long period of time should tell us something about what is best practice in a particular domain and context. But the obvious weakness is that we cannot generalize from one or even a few examples. The cases may be too unusual. Or non-obvious factors or random chance may have influenced what we see. Cases may suggest higher-level concepts but often they do not even do this because the cases are so rooted to the particular circumstances of one organization and situation. In short, as noted by Kathleen Eisenhardt and Robert Yin, case studies can have great value to generate ideas, if they are selected carefully. But, ultimately, case studies are only exploratory and illustrative.[2] They do not bring certainty – at least, not *statistical* certainty – about what might or might not represent an enduring concept or a best practice in management.

Large-Sample Research

To overcome the limitations of case studies and very small samples, scholars in many fields, ranging from medicine and physics to economics and management, have turned to *large-sample research.* Here the goal is to study a phenomenon by analyzing a bigger percentage of the relevant "population," hopefully with a sample that is random or at least not obviously biased in a particular direction. The goal is also to analyze the data statistically so that we can attach probabilities or percentages to our propositions and add "controls" to see if what we think is happening really is happening. How large the sample and how sophisticated the statistical methods usually depends on the questions asked and the available data.

After arriving at MIT in 1986, I decided to combine case studies with large-sample statistical analysis for my new research on software factories and engineering practices. In particular, I wanted to know if we could measure "factory-ness" in software development and compare the effectiveness of different approaches in Japanese and US firms. Collecting survey data on a relatively large number of projects seemed the only way to do this. Though I had taken an introductory course in statistics in college, I quickly learned that I needed to use more advanced methods than I had studied. In addition, I struggled with problems such as how to define robust "variables" as well increase the reliability and comparability of the data. Then there was the additional difficulty of separating out what is cause and what is effect, and interpreting results from the "50,000 feet-high view." This also led to a "black-box" problem: With limited information, we often make assumptions about how an organization might have made decisions or behaved, and this can produce wrong conclusions about underlying causes.

The most famous case taught in business schools illustrating the use and misuse of statistics versus the insights available only through a detailed case study centers on Honda and why it succeeded in the motorcycle industry.[3] (I was first exposed to this material when I began teaching the core Strategic Management course at MIT Sloan in 1986.) The Boston Consulting Group (BCG) was hired by the British government to analyze Japanese competition and make recommendations to improve the British motorcycle industry. The consulting firm told a compelling story by analyzing data on Honda's motorcycle production and pricing practices, and offering the theory that "learning curve" economies enabled Honda from the mid-1950s to reduce costs significantly by pursing high-volume production and thereby wipe out the competition. The recommendations from this analysis were clear: standardize components, limit model offerings, build mass-production factories in advance of demand, price below cost initially, and then sell aggressively based on the assumption that costs will drop as volumes increase, learning occurs, and productivity rises.

In many ways, BCG's explanation, detailed in its lengthy report as well as summarized in the Harvard Business School's "Honda A" case, sounds plausible. Reams of pricing data seemed to prove the learning-curve theory, although students quickly catch on that prices are not costs. But this strategy clearly appeared to be the "best" way to enter and dominate an industry. Encouraging firms to go after volume, market share, and learning curves provided the basis for much of BCG's successful consulting work during the 1970s and 1980s.

But then we are shocked to learn in the "Honda B" case that the BCG view was really a *retrospective misinterpretation* of what actually happened. In reality, Honda never intentionally pursued volume manufacturing and learning-curve pricing strategies in the mid-1950s or early 1960s. It produced in volume because customers were drawn to its distinctive products. In fact, it had trouble keeping up with demand. Moreover, Honda's products were so popular not because of any deliberate strategy and rational planning, but because of their technical superiority, which we find out was at least partially due to happenstance. The founder, Soichiro Honda, was a racing enthusiast and simply wanted to build the best engines in the industry and race his motorcycles. He attracted similarly minded, and similarly talented, engineers to the company. When Honda managers finally devised a strategy to export motorcycles to the United States, the effort failed miserably because the company had not designed bikes for the high speeds and distances of American highways. Honda survived by accidently identifying demand for its small 50cc SuperCub, which Japanese employees in Los Angeles were using themselves to get around the city.

What we learn from the detailed case is that Honda succeeded because the founder was inspired to build the best engines on the planet. This vision enabled the company to build *distinctive capabilities* for engine design, which it exploited in the motorcycle business and later took advantage of in the automobile industry. Honda saved money not because of scale but because it's small powerful engines were cheaper to make than bigger engines found in competing products. Honda was also fortunate to pair its technical founder with a marketing specialist, Takeo Fujisawa, who had a keen sense of customer needs and how to capitalize on the company's technical expertise. Detailed strategic planning itself failed at this company. But the organization, its leaders, and its people, were highly flexible. Honda evolved the technical and marketing capabilities that enabled it to exploit opportunities as they arose, in the Japanese, U.S., and other markets, and to adjust the specifics of competitive strategy incrementally, through trial and error.

Then we have some best-selling books that appear more rigorous than they really are because of problems in their samples and the questions asked, or in the lack of statistical controls. Two examples come immediately to mind. One is from 1982, *In Search of Excellence*, by the consultants Tom Peters and Robert Waterman.[4] They were relatively ambitious: Peters and Waterman's team reviewed 62 of McKinsey's best-performing clients, used some financial and subjective criteria to weed out some firms, and then focused on 43 companies that illustrated a small set of principles seemingly fundamental to success. Their list: *a bias toward action, closeness to the customer, a spirit of autonomy and entrepreneurship, a focus on productivity* *through people, hands-on management, a strategic focus on what the company is good at, simple and lean staffing, and a simultaneous combination of tight centralized values with looseness or decentralization at the workplace level.* While it is hard to argue with these principles, soon a number of the highlighted firms ran into major problems or even disappeared (Atari, Data General, DEC, IBM, NCR, Wang, Xerox). What was enduring excellence and what was luck – or bad luck – seems hard to distinguish, in retrospect. We might also wonder exactly why Peters and Waterman choose these companies and not others. How do we really know that the factors they talked about, and not other factors, were responsible for the performance, good and bad, of these firms?

A more recent best-seller by Jim Collins, *From Good to Great* (2001), suffers from this same lack of controls.[5] It is harder to detect because Collins did use a lot of numbers and does appear methodologically rigorous. As with *In Search of Excellence*, there are also many useful ideas and anecdotes about management in Collins' book, which builds on his previous best-seller, *Built to Last*. But, again, because of the structure of the study, it is not possible to determine which concepts represent enduring best practices and which do not. At least in part to remedy this problem, Collins recently published another study on why great firms fail.[6]

For *From Good to Great*, Collins and his team of MBA researchers at Stanford Business School looked at nearly 1500 of the largest companies to find the best performers in terms of stock market value. Then they analyzed in some depth the best 11 of those companies – Abbott Laboratories, Circuit City, Fannie Mae, Gillette, Kimberly-Clark, Kroger, Nucor, Philip Morris, Pitney Bowes, Walgreens, and Wells Fargo. The sample they chose through statistics – only those firms that had a particular level of superior stock market performance before and after a certain starting date made the cut. Choosing a sample this way is fine and the book ends up with a complete population of firms with a particular level of stock market performance. But then Collins and his research team, relying on interviews with managers and reading secondary materials, turned to subjective methods and extracted a small set of principles that seemed to describe why these companies had done so well. Their list: *a particular leadership style of mostly internally promoted CEOs, a focus on talented people, clear understanding of internal strengths, simple fact-based performance goals, a disciplined culture centered on commitments, a reinforcing use of technology, and momentum built from early successes*.

Once more, it is hard to argue with the relevance of these attributes. Collins also cites the opinion of one friendly statistician that his results are very unlikely to be random. But, unfortunately, like Peters and Waterman, Collins made no attempt to measure and test these attributes, such as to correlate one or more of them with the stock performance or any other measures. As a result, we have no way to determine *statistical* significance, or prioritize the factors, or control for other companies that might have exhibited the same or most of the same factors but did not have comparably good or great stock-market performance. It is also a potential problem when a large group of researchers collaborate on subjective analyses; it is very hard for them to be consistent.

Moreover, as with *In Search of Excellence*, several of the companies Collins highlighted did not do so great after publication of the book – again, making us wonder what are and what are not "enduring" best practices. Abbott has struggled with fraud lawsuits and problems with its drug Oxycontin. Circuit City encountered several years of dismal performance and dramatic losses in its stock value (even before the crash of October 2008), and saw its new CEO resign before the company declared bankruptcy and closed down early in 2009. Fannie Mae went bankrupt in the 2008 subprime mortgage crisis and had to submit to a government takeover. Gillette did reasonably well but not well enough to avoid a takeover by Proctor & Gamble in 2005. Even Wallgreens stumbled financially and saw a major decline in its stock price, again, before the crash of October 2008.

Some of Collins' points resemble what we saw in Peters and Waterman (Table 1) – understanding what the firm is really good at (internal strengths) and a focus on (talented) people. But the other points seem completely different in the two books. I must note here that my list of enduring ideas has some similarity but is mostly different from these other two books. The idea of "capabilities" is not far from focusing on what the firm is good at and understanding its strengths. But the concept of capabilities as it has evolved in the strategy field, which is what I am referring to, is much more proactive and evolutionary: Firms *build* capabilities in order to compete more effectively or move into new markets, and these capabilities are *dynamic*, not static. "Pull" definitely relates to closeness to the customer and requires a disciplined culture, but it is both more abstract and specific in applicability. "Scope" can mean lean staffing, but again, the concept is much more abstract and specific – and as powerful as economies of scale. I talk about "flexibility" in the context of entrepreneurship and strategy, but I also talk about this in terms of manufacturing and product development, or the ability to innovate in technology and business models. And then "platforms" and "services" are ideas that bring the firm to yet another level of strategy, capabilities, and operations.

[Table 1 about here]

I must admit, though, that if one reads *In Search of Excellence* carefully, the book is also very insightful about flexibility and how to build innovation capabilities, which is very much what my book is about. In fact, every one of my six ideas can be found somewhere in the many anecdotes or impressive literature discussions in the Peters and Waterman study. We can also find many relevant examples and points of wisdom in *Good to Great*.

One common weakness in the three books is that our lists are all subjective, even though Peters and Waterman as well as Collins went through a specific process to get to their group of firms. My list also differs in that it is primarily a set of themes found in my research. I do not claim to have compiled *THE* list of anything except recurring ideas that I and many other researchers continue to encounter. Another caveat is that two of my ideas apply only to particular kinds of firms: the importance of platform strategies and services to complement or substitute for a product business. In addition, I have chosen my ideas with a particular goal in mind. Each contrasts with a set of more rigid ideas that do *not* seem to represent best practice. Moreover, there is considerable theoretical and empirical research, some done by myself and much more done by other researchers, that supports the importance of these ideas. I will summarize some of this research in the introductions to the various chapters. In short, compiling definitive or objective lists of best practices is very hard to do, and it is not really what I have tried to do in this book. This book is about recurring ideas that seem to have lasting relevance for managers as well as confirmation in numerous academic studies.

Hybrid Methods – Qualitative and Quantitative

Of course, it must be possible to go beyond the best-sellers and use more rigorous methods – that is, use statistical analysis of a large sample, some theory or concepts to develop and test hypotheses, and then drill down through detailed case studies or intensive field work to understand the phenomenon in more depth. Intuitively, this approach is very appealing and, at MIT Sloan, we often encourage our doctoral students to follow this path. I also have used this hybrid approach in most of my work since 1986. But there are drawbacks. The research is generally very time-consuming, and the researchers have to master two very different skill sets.

One academic study I found inspiring followed somewhat of a similar methodology to Collins but primarily was an historical comparison of capitalism at the national level. This is the book *Scale and Scope: The Dynamics of Industrial Capitalism,* written by the late (and Pulitzer prize-winning) business historian, Alfred Chandler, who began his career at MIT and finished it at the Harvard Business School.[7] This 1990 tome was an enormous project – analyzing financial data on the top 200 corporations in the U.S., Britain, and Germany (600 in total) between the late 1880s and the 1940s. Chandler concluded that size brought specific advantages in terms of both scale and scope economies, in operations ranging from production to marketing, distribution, and professional administration. This was especially true in the United States, where firms were bigger. The nature of capitalism also varied in these countries, apparently driven very much by the average size of the dominant firms.

Chandler used simple descriptive numbers rather than statistical analysis to rank firms by assets and market value. But the idea was bold: Measure an entire population, identify quantitatively which were the top organizations, and then describe those firms in more detail qualitatively. Because it focuses on national comparisons, *Scale and Scope* does not go into the firm-level detail that some of Chandler's other writings have done, such as the classic *Strategy and Structure* (1962).[8] This earlier book explained how DuPont, General Motors, Standard Oil (Exxon-Mobil), and Sears moved to a multi-divisional organizational structure – which we now see as an organizational best practice – after adopting business diversification and globalization strategies. In Scale and Scope, the primary idea is that size matters. Firms can take advantage of size both through conventional scale economies, where costs decline with volume production or distribution of a single product, and through scope economies, as in the production, marketing, and distribution of multiple products using the same facilities or channels.

Two other books stand out for effectively combining large-sample analysis with detailed field work. The research I cite here was actually the result of two multi-year programs at MIT and Harvard Business School in the 1990s.

The more scholarly of the two is by Kim Clark and Takahiro Fujimoto, *Product Development Performance: Strategy, Organization, and Management in the World Auto Industry* (1991). This study is based primarily on Fujimoto's 1989 doctoral thesis, supervised by Clark, the former dean of the Harvard Business School.[9] They documented the advantage of Japanese automakers in product development productivity (lead time and engineering hours) as well as total quality through a meticulous study of 29 projects at 22 automakers in the United States, Japan, and Europe. Confidentiality agreements prevented Clark and Fujimoto from identifying specific firms, so they focused on regional comparisons. But, even so, they illuminated some striking differences. Most importantly, the best firms and projects, which were mainly in Japan, utilized what they called "heavyweight" project managers and project teams. The firms also relied extensively on overlapping activities, short concept development phases, tight coordination across engineering and manufacturing functions, and "black-box" designs and components from suppliers. Several other related techniques facilitated fast product development times and very efficient use of engineering resources. The great strength of the book is the detail with which the authors analyzed development processes at the component and project level, the linkages to manufacturing capabilities and competitive strategies, and the rigor with which they collected and analyzed both qualitative and quantitative data.

The more managerial of the two books is by James Womack, Daniel Jones, and Daniel Roos, called *The Machine that Changed the World* (1990). This was a summary report of research done under the auspices of the five-year International Motor Vehicle Program (IMVP), based at MIT.[10] (Full disclosure: I was a faculty member of the program at the time and supervised several of the master's theses. These included the initial assembly-plant productivity survey by John Krafcik, who coined the term "lean production" as a generalization for Toyota's production system, and the product development work by Antony Sheriff and Kentaro Nobeoka.) Again, the IMVP researchers were not allowed to reveal individual company names and focused on regional comparisons. But they clearly documented the striking advantage of Japanese automakers – whether they operated in Japan or in North America – in manufacturing productivity (mainly assembly plant hours) and quality (defects, provided by J.D. Powers), based on the study of 60 assembly plants in Japan, North America, Europe, and developing countries. The assembly plant study was taken over by John Paul MacDuffie, now a professor at the Wharton School of the University of Pennsylvania, after Krafcik (now the CEO of Hyundai Motor America, following a career at Ford) graduated in 1988. *The Machine that Changed the World* also summarized the program's findings on regional differences among world auto makers in supply-chain management, human resource management, sales management, and product development, including a summary of the work by Clark and Fujimoto, who were (and remain, at least in the case of Fujimoto) IMVP research affiliates to this day.

Both studies demonstrate how academics can do research of enormous value to managers without leading practice but by analyzing, measuring, and explaining what the best firms are doing. *The Machine that Changed the World* ended up selling over 600,000 copies in 11 languages. This book, as well as *Product Development Performance*, became required reading for managers in many industries, not just automobiles. Both studies also demonstrate the importance for academic researchers to take the time to gain a deep understanding of the phenomena they are analyzing. The quantitative evidence of Japanese superiority during the 1980s and 1990s in efficiency, flexibility, and quality, both in automobile manufacturing and product development, is hard to refute. Some questions remain regarding the effectiveness of the practices in different settings, such as with different union work rules. But it was probably the careful qualitative descriptions in these two books of *why* the Japanese practices worked so well that were most convincing and valuable to managers.

The Importance of Timing and Context

All the studies I have mentioned so far, including my own research, encounter challenges when we try to generalize. Are Toyota-style "pull" techniques always associated with higher productivity and quality – in any company, industry, country, and time period? Are economies of scope and flexibility always important or only under certain conditions? There are many factors to consider before we can confidently state that one underlying concept or specific practice is better than another. Every practice takes place within a certain time period and context, and we need to understand the impact of that context to determine what is a "real" effect and what is specific to a particular firm or industry or due simply to chance.[11] Any college course or textbook dealing with the use of statistics will emphasize the need to identify the proper "control variables," though how to identify these is not always so obvious. The methods that business school researchers use are also growing increasingly sophisticated and difficult for non-experts to understand. Nonetheless, I offer some examples below of major contextual issues that researchers in the fields of strategy, innovation, and technology management have generally considered important. More importantly, managers should consider these same issues when deciding which ideas or best practices to follow and how to implement them in their particular situations.

Imitation or "The Lemming Effect"

First, we need to acknowledge that, even if a particular practice appears fundamental to competitive advantage, once other firms imitate that practice like lemmings following a leader[12] – which is likely – then, at some point, it will no longer be as effective as when the practice was unique or rare. The advantage here may only go to the first mover or pioneers. Successful imitation is perhaps the major reason why industry leaders lose their edge.[13] If most firms adopt a new best practice, then that becomes standard practice and, at least on the surface, is no longer a best practice. At the same time, however, firms can differ greatly in organizational capabilities related directly to implementation skills, and this difference may reflect a deeper, more subtle set of concepts or techniques related to process, people, and technology management.

For example, most firms in the automobile industry have introduced pull systems to reduce parts inventories and assemble vehicles more on a Just-in-Time basis. But the degrees of inventory levels as well as productivity and quality vary greatly across firms or even within firms depending on product design, worker training, management commitment, and other factors. Therefore, managers and researchers should not simply measure firms as following or not following a particular practice; we need to look deeper into how well organizations implement a specific concept or technique, and why differences exist.

Industry or Technology Lifecycles

Another basic factor to consider when comparing firms is stage of the industry or technology *lifecycle.* In the 1970s, William Abernathy and James Utterback brought the idea of lifecycles into the everyday language of managers as well as scholars concerned with operations and technology strategy. Their studies and follow-on research found that many industries go through periods of "ferment" to maturity and then decline in terms of growth rates, profitability, or number of competitors. The theory argues that, in industries where maturity sets in, manufacturing companies need to shift their focus from innovation in product design to process efficiency (i. e., manufacturing) as designs standardize and companies come to compete on the basis of price. Other scholars such as Stephen Klepper have explored this idea further, with some refinements, but the general observations remain similar. There is now a large literature on this topic, and the research includes both a rich set of industry analyses and large-sample statistical work.[14]

The implications for best practice and enduring ideas are straightforward. Some strategies or management approaches may be superior in the early stages of an industry, when emphasizing applied research or innovative product designs is most important. But, as competitors move in, industry structure changes, and the technology evolves, specific practices that once led to competitive advantage may no longer be effective. We know that not all industries follow lifecycles, but enough do to make this an important factor for managers and researchers to account for to the extent possible.

Unfortunately, it is much easier to identify where one is (or if one is) within a lifecycle *after the fact* rather than before or in real time. This is a major problem when trying to use lifecycle or industry stage as a control variable. Nonetheless, it is important for any firm-level or project-level analysis to try to account for differences that may be related to the lifecycle phenomenon, or at least the maturity of an industry.[15]

Nature of the Technology or Innovation

Another variable is the nature or type of technology, or the associated innovation, apart from the maturity of the industry. Academics generally define technology as the knowledge or capabilities used to turn some form of inputs (e.g., materials, components, or even raw knowledge itself) into some form of outputs (e.g., products and services). But technology, or technological innovation, can take the form of product design, system architecture, materials, manufacturing, or even the distribution process (such as the web). A product or process technology can also be new to the firm, new to a particular market, or new to the world. Technological innovations can be incremental, radically new, or potentially disruptive to existing players in an industry and their ways of making money, as Clayton Christenson and others have demonstrated. All these variations on the nature of a technology and associated innovations can affect whether or not a particular practice works or does not work in a given context.

For example, practices that work extremely well in designing or manufacturing a relatively well-understood technology may fail completely with a new technology. Techniques that work well with a technology such as software may not work well when managing product development using very different kinds of tasks, processes, people, and inputs, such as pharmaceuticals, chemicals, or automobiles. Toyota-style production may not result in the same benefits outside repetitive assembly operations, although I am not so sure of that. Best practices for managing technological innovation may differ when we are talking about a product versus a process versus a service. Or perhaps the nature of the technology and the innovation does not matter so much and a particular practice, such as a pull system, provides equal benefits in almost any setting.

We do know that the challenges facing managers as well as researchers are not simple. Many products contain a mix of new and old technologies, such as in the form of different components or subsystems or subroutines. Hence, it is often difficult to classify the technology used in many products, including automobiles – which contain some technologies invented a hundred years ago and other technologies invented almost yesterday. Case studies are useful to explore these different issues, but they cannot settle many arguments. Larger sample research using type of technology or type of innovation as a control variable is usually the best way to explore the question of how much does technology matter. But researchers still need to figure out how to measure different technologies in ways that make them comparable and control for meaningful differences in technology or levels of innovation.[16]

Industry and Business Differences

A related factor is how much does the *industry* matter when trying to understand differences in company performance. For example, is the fact that a particular firm is a member of the pharmaceutical industry as opposed to the automobile industry more important for predicting its profit and growth rates than other factors? Certainly, industry differences affect other metrics, like average product development times – which can be a decade for a new drug versus a few years for a new car and perhaps a few days for a new software program. Fortunately, several economists and economics-trained strategy researchers have tackled this question in some depth. The studies do not all agree completely, though we can also view them as building upon each other and refining prior results. Point of view also matters here, as it usually does in research.

Economists have been trained mainly to look for macro-effects, such as industry differences, whereas strategy researchers tend to place more importance on firm-level or managerial differences – what the field has come to call differences in "resources" and "capabilities" (more on this later, in Chapter 1).[17] But we need the tools of the economist here because large-sample econometric analysis is the best methodology for this type of question.

In a 1985 article, the economist Richard Schmalensee (and my former dean at MIT Sloan) reported that industry factors were most important in influencing profitability rates at the business-unit level compared to corporate affiliation (membership in a particular diversified firm) or market share effects, and accounted for about 20 percent of the difference in profitability. This was a landmark study, but Schmalensee used only one year of data. Richard Rumelt, a strategy researcher with an economics bent who had earlier done pioneering work on corporate diversification levels and profitability, used a multi-year data set and was able to separate out "stable" from "transient" influences. In his 1991 article, he found that industry-level differences did matter somewhat, but that business-unit effects were far more important, accounting for nearly half the variance in profit rates. In other words, business units within the same industry differed much more when compared to each other than industries differed when compared to other industries.

Another article from a strategy and economics point of view, by Anita McGahan and Michael Porter, was published in 1997. They confirmed results found in both of the earlier studies, finding that industry accounted for about 19 percent of the variation in profitability and business-unit differences about 32 percent. But since the business-unit differences persisted over time, and these were strongly affected by industry differences, they concluded that industry effects, in fact, mattered more than the other factors, albeit with some sector variations.[18]

For my purposes in this book, the flip side of the basic question about industry impact is: To what extent does *management* matter? At least within an industry, we can say that there are significant differences in how different organizations perform, and this surely has something to do with the characteristics of those organizations and their managers and employees, rather than just industry-level factors. But, when looking at an entire population of firms, such as the S&P 500, we need to control first for industry differences, direct and indirect, such as at the business-unit level. It is also impossible to generalize convincingly without multiple years of data. It follows that researchers have to be careful with regard to market segment differences that may show up more in metrics such as product type, within an industry or business. But the problem remains – it is not always so clear that managerial practices matter more than other factors when it comes to understanding firm performance.

Environmental or Institutional Context – Japan as a Case in Point

Another obstacle that has stood in the way of determining best practice relates to the *environmental or institutional context.* We have to account for the fact that firms in one country may perform differently or do things differently than firms in another country, possibly because of differences in culture or socialization but also due to potential differences in local institutions such as government policy, educational systems, or locally available resources.

Here, my experience as a student of Japanese business history and management practices has heavily shaped my thinking. When I first joined a business school in 1984, a very large number of cases throughout the MBA curriculum – dealing with strategy, organizational behavior, operations management, and industrial policy – touted the excellence of Japanese approaches. Japan was "number one," as Harvard professor Ezra Vogel stated so directly in his best-seller book from 1979.[19] (Full disclosure: I was once Professor Vogel's teaching assistant while in graduate school.) Japan in many ways represented overall "best practice" at this point in time, especially when it came to manufacturing and quality control, long-term capabilities development and the management of people, and government policy. A number of best-selling books helped educate the Western public, such as *Kaisha: the Japanese Company*, *The Art of Japanese Management*, and *MITI and the Japanese Miracle*.[20] Learning from Japan declined in popularity very quickly after its economic bubble burst in 1989; fortunately, most "Japan specialists" in business schools had taken on functional specialties, like technological innovation, strategy, and international management. But the realization has stayed with me, from observing Japan as well as studies such as from Peters and Waterman, that best practices and top performers in one context and time period may not endure once the environment changes.

The case of Japan is so striking that it deserves more treatment here.[21] While many features of "Japan, Inc." and the country's political, economic, cultural, and social "systems" provided benefits during periods of rapid growth, they proved to be a drag on change and international competitiveness once growth stagnated.[22] These features as well as a long list of managerial and employment practices once thought to represent the best way to do things also turned out to be inefficient and ineffective in the 1990s (Table 2). For example, as far back as the 1960s and 1970s, foreign observers of Japan began describing specific aspects of "the Japanese management system" that they believed contributed to the low-cost but high-quality goods produced by major Japanese firms, led by companies such as Toyota and Sony. In particular, management experts in the West praised Japan's preference for lifetime employment in large firms, seniority-based wages, company-based unions, and long-term and consensus-oriented decision making, as well as the Just-in-Time or Lean Production techniques, quality control practices pushed down to the lowest level of the organization, and extensive use of low-cost but dedicated supplier networks.[23] Ironically, at least some of these Japanese management practices, especially the focus on quality and a broader view of leadership and decision making, were not really Japanese in origin but initially taught to Japanese managers after World War II by American engineers and consultants. These Americans included both the Civil Communications Section instructors during 1949-1950, who were part of the U.S. Occupation forces and needed to procure locally made telephone equipment, and later quality experts led by Edward Deming and Joseph Juran.[24]

[Table 2 about here]

The Japanese economy grew at "miraculous" levels from the mid-1950s through the early 1970s, and still did very well through the 1980s. After 1989, however, the economy went into a tailspin due primarily to over-heated real estate and stock markets, and weak financial regulation – much like the United States and the world experienced in 2008-2009. This recession exposed the less attractive side of Japan's supposed strengths. As seen in Europe, too, commitments to lifetime employment can greatly reduce the flexibility of firms to adapt to increasing external competition or changes in technology or knowledge requirements, such as by reducing headcount or hiring different kinds of personnel or making acquisitions. Seniority wages do not reward merit and can lead to complacency and conservative decisions. Consensus decision making can be good when strategic options are clear, but it can also lead to "lowest-common-denominator" compromises, as well as "herd-like" thinking, rather than crisp decisions and strategic focus. The lack of pressure from shareholders may be good to support long-term investments, such as the kind Japan needed to develop a domestic automobile industry or to create innovative consumer electronics products like the home VCR. But, as global competition increased, many corporate investments have not brought good returns for Japanese firms and have contributed to their extremely low levels of profitability by international standards.

Just-in-Time practices can be extremely useful in manufacturing, but a preoccupation with manufacturing seems to have encouraged Japanese firms and the government to neglect productivity in services –the bulk of the economy. Japan also has moved too slowly into new information-intensive and science-based industries such as software and biotechnology, which have become new sources of employment and wealth in Western countries. The obsession with "continuous improvement" (called *kaizen*) can lead to higher quality and productivity but also waste time and money when diminishing returns set in or when firms produce much better quality than customers really need. And pressing suppliers to reduce costs or absorb excess labor from large firms can become little more than a "shell game" as demand slows.

It became clear by the mid-1990s that there were limits to Japanese management techniques, such as how far the Japanese could push JIT production, reductions in inventories, cuts in worker headcount, and other efficiency-oriented practices in product development and manufacturing.[25] But another factor that caused Japan's economy to stagnate for a decade was intensifying external competition. Most firms in the United States, Europe, and Asia have learned what there is to learn about Japanese production, engineering, and quality-control practices, as well as human resource management practices. Japan remains a very wealthy country and, as of 2009, the second largest economy in the world, behind only the United States. It still has many firms that are highly competitive in global industries. Nonetheless, the "Japanese management system," which provided a powerful competitive edge in the 1970s and 1980s, no longer did so in the 1990s or 2000s.

"Luck" and Population Ecology

When trying to determine which factors really matter we also need to confront the role of luck – good and bad. Some firms or practices may be successful because of random decisions or weak competition at a particular point in time. A large enough sample and multiple years of data help with this problem. But then we still must confront the issue brought up by population ecologists. I am taking liberties with my paraphrasing, but some scholars of this persuasion seem to argue that such a thing as "best practice" cannot really exist in any absolute sense. All firms and organizations, they point out, have trouble adapting to change. Those organizations that seem to thrive just "happen" to fit the requirements of the market and circumstances of the time one is looking at. Success is more like a Darwinian process of natural selection. The actions of managers have little or no impact precisely because it is so hard to alter the structural characteristics of an organization. Hence, when we think we are looking at best practice, we are simply looking at practices of the remaining population of firms with characteristics matching the market needs of a particular time and place.[26]

I do not fully agree with the view that best practices are entirely artifacts of time and place.[27] But, surely, there is some truth to this argument – otherwise successful firms would remain successful for much longer periods of time. Did Bill Gates and Microsoft just happen to be in the right place at the right time? Yes, they were lucky, but unmatched as well at exploiting the opportunity before them in 1975 and in 1980, when IBM came calling for an operating system. But Microsoft's power has declined with time. What about General Motors? Or General Electric? Or Lehman Brothers, Fannie Mae, Citibank, et al.? At their peak, these and many other firms cultivated practices and strategies that clearly dominated and suited a particular time and set of circumstances. Then they became much less appropriate (or even disastrous) as circumstances changed.

Concluding Thoughts

So what are we to conclude? For sure, we need to think in terms of the old concept of a *contingency framework* to help us understand when and why particular practices might work better than others. Building such frameworks has always seemed logical to me, and there have been a number of useful ones in the organizational theory literature, as I discuss in later chapters. But academics also tend to put in too many "if this, then that" kinds of relationships. As the number of contingencies increase, the frameworks can become too cumbersome to be useful to managers.

We also need to think in terms of *complementarities and tradeoffs* – which managers are paid to manage! Investing in long-term distinctive manufacturing or R&D capabilities may involve short-term tradeoffs in financial performance. Implementing a pull system in manufacturing may save costs and vesting improve visibility into operations but reduce inventories to a dangerously low level. Pursuing scope economies in production, product development, and services may also be costly in the short term and impede some kinds of flexibility. Promoting manufacturing or

R&D flexibility at the expense of short-term efficiencies such as through fixed automation or scale economies has a cost as well as a benefit. Designing your product and strategy to enhance your chances of establishing an industry-wide platform may also involve sharing revenues and profits with ecosystem partners – potentially costly in the short-term though potentially much more profitable in the long-term. Investing in services can also hurt profit margins for a product firm. But if product revenues or prices are collapsing, the real value of the product business may actually be in complementary services that generate revenue as well as the kind of deep customer knowledge gained only through services.

To understand the complexity of these strategic and operational issues, we are left with a dilemma: Case studies provide the detail to uncover the subtle and not so subtle realities of strategy, structure, process, and decision making – and many more elements critical to understanding best practice in a variety of domains. But we cannot generalize, or not very confidently. Large-sample studies, with and without sophisticated statistical analysis, provide the power to argue with greater levels of confidence. But the "50,000 feet-high" view and statistically significant results usually do not provide enough depth to help managers move beyond general principles. Intermediate approaches that start with large data sets and then dive down into the phenomenon can be more informative, but these studies are difficult to do and subject to their own constraints.

Fortunately, I am only arguing for *potentially* enduring ideas through the very personal lens of my own research. Readers can judge for themselves whether or not the ideas are useful or broad enough, and if the cited research is convincing enough. The invitation to deliver the 2009 Clarendon Lectures in Management Studies provided me with this unique opportunity to think about broad themes in my research and in business schools more generally. Looking back, my methods have varied with the questions and the data available. But usually I have combined qualitative and quantitative case studies with attempts at larger-sample empirical analyses to gauge "the state of the forest," so to speak, while focusing on particular "trees." I believe the ideas I propose are enduring because they are relatively high-level abstractions, based on years of thinking, research, data collection, hypothesis testing, discussions with managers, and validation by many other researchers.

Of course, any set of ideas is likely to have some constraints or work particularly well only with a specific combination of factors. But I argue in this book that managers who try to build distinctive capabilities to implement their strategies, rely on pull as much or more than push concepts, exploit economies of scope as well as scale, and pursue flexibility as much as efficiency, will create agile firms able to survive and even thrive in many different circumstances. They should be able to respond well and anticipate the signs of change better than their competitors. And firms that turn their products into industry-wide platforms, supported by global ecosystems of innovation, or understand how to complement existing platforms, and develop services to make their product businesses more resilient, will find themselves better able to endure the dual challenge of innovation and commoditization.

Before proceeding to the main chapters of this book, I would like to clarify the inspiration for my title. At least in part, I owe an obvious debt to the 1982 best-seller by Peters and Waterman, *In Search of Excellence*. More directly, though, in taking this opportunity to reflect on my research, I have drawn inspiration from another book written by another student of history, Theodore White. He is perhaps best known for his 1962 Pulitzer-prize winning analysis of John Kennedy's candidacy, *The Making of the President, 1960*. But White is also the author of a fascinating account of his life and work beginning with his undergraduate years at Harvard and then observations while covering China as a journalist during the 1940s and 1950s. Published in 1978, this book has a title I have never forgotten and have always found inspiring: *In Search of History: A Personal Adventure*.[28]

Table 1 Comparison of Best Practice Ideas

In Search of Excellence	Good to Great	In Search of Best Practice
1. bias toward action	1. internally promoted CEOs	1. capabilities, not just strategy (or vision)
2. closeness to the customer	2. focus on talented people	2. pull, don’t just push
3. autonomy & entrepreneurship	3. understanding of strengths	3. economies of scope, not just scale
4. productivity through people	4.fact-based performance goals	4. flexibility, not just efficiency
5. hands-on management	5. momentum from successes	5. platforms, not just products (when possible)
6. focus on what firm is good at	6. reinforcing use of technology	6. services, not just products (or platforms)
7.simple, lean staffing	7. momentum from early successes	
8. simultaneously loose-tight or centralized- decentralized		

Table 2 Japan: 1980s Strengths to 1990s Weaknesses?

1980s Strengths	1990s Weaknesses
Economic System	
• low wages • high savings • high exports	• rising value of yen • bubbles in stock market and real estate)
Financial System	
• low interest rates • lots of capital for investment, • protected banks • deficit financing	• inefficient use of capital & poor investment returns • bankrupt banks
Political System	
• stable, conservative, • consensus-oriented • sharing of wealth through subsidies	• struggles over shrinking pie, • political "gridlock", • slow/negative growth, unemployment)
Social and Cultural System	
• centralized & standardized primary education • shared values, • hierarchy & authority • group over individual	• weak universities • too much emphasis on rote learning, • not enough individualism & creativity
Management and Employment System	
• lifetime employment in large firms • seniority-based wages • company-based unions • consensus decision making • long-term view • institutional share-holding • Just-in-Time" ("Lean") production • QC & kaizen (continuous improvement) • low-cost dedicated supplier networks	• reduced flexibility • do not reward merit & achievement • inadequate concern with worker welfare • lowest-common denominator decisions • little pressure for efficiency/profits, • problem in global competition for some firms • too much focus on manufacturing; traffic jams • diminishing returns • "shell game" of transferring costs to suppliers

Endnotes, Introduction

[1] See the website http://agilemanifesto.org/ (accessed April 16, 2009).

[2] See the classic references K. M. Eisenhardt (1989), "Building Theories from Case Study Research," *The Academy of Management Review*, 14(4): 532-550; and R. K. Yin (2002), *Case Study Research. Design and Methods* (Thousand Oaks, CA: Sage Publications).

[3] See E. T. Christiansen and R. T. Pascale (1983), "Honda (A)" (Boston: Harvard Business School Case #384049), and "Honda (B)" (# 384050). Also see Richard T. Pascale (1984), "Perspectives on Strategy: The Real Story Behind Honda's Success," *California Management Review*, 26(3): 47-72.

[4] T. J. Peters and R. H. Waterman (1982), *In Search of Excellence: Lessons from America's Best Run Companies* (New York: HarperCollins).

[5] J. Collins (2001), *From Good to Great: Why Some Companies Make the Leap…and Others Don't* (New York: Harper Business).

[6] J. Collins (2009), *How The Mighty Fall: And Why Some Companies Never Give In* (New York: HarperCollins).

[7] A. D. Chandler, Jr. (1990), *Scale and Scope: The Dynamics of Industrial Capitalism* (Cambridge, MA: Harvard University Press).

[8] A. D. Chandler, Jr. (1962), *Strategy and Structure: Chapters in the History of the American Industrial Enterprise* (Cambridge, MA: MIT Press).

[9] K. B. Clark and T. Fujimoto (1991), *Product Development Performance: Strategy, Organization, and Management in the World Auto Industry* (Boston: Harvard Business School Press).

[10] J. P. Womack, D. T. Jones, and D. Roos (1990), *The Machine that Changed the World* (New York: Rawson/Macmillan).

[11] An excellent discussion of this issue combined with an empirical study of pharmaceutical R&D is Ian M. Cockburn, R. M. Henderson, and S. Stern (2000), "Untangling the Origins of Competitive Advantage," *Strategic Management Journal*, 21(10/11): 1123-1145.

[12] My thanks to David Yoffie for this analogy.

[13] A commonly cited strategy article that makes this point is M. A. Peteraf (1993), "The Cornerstones of Competitive advantage: A Resource-based View," *Strategic Management Journal* 14(3): 179-191.

[14] See, for example, the classic J. M. Utterback and W. J. Abernathy (1975), "A Dynamic Model of Process and Product Innovation," *Omega*, 3(6): 639-656, and W. J. Abernathy and J. M. Utterback (1978), "Patterns of Innovation in Industry," *Technology Review*, 80(7): 40-47. Also see J. M. Utterback (1994), *Mastering the Dynamics of Innovation* (Boston: Harvard Business School Press); and S. Klepper (1996), "Entry, Exit, Growth, and Innovation Over the Product Lifecycle," *American Economic Review*, 86: 562-583, and S. Klepper (1997), "Industry Lifecycles," *Industrial and Corporate Change*, 6: 145-181.

[15] For example, see the analysis in F. F. Suarez and J. M. Utterback (1995), "Dominant Designs and the Survival of Firms," *Strategic Management Journal*, 16(6): 415-430.

[16] How to think about the nature of technology and different types of innovation is an enormous subject. For a brief review done because we needed to control for different levels of innovation in automobile projects, see pp. 101-106 in M. A. Cusumano and K. Nobeoka (1998), *Thinking Beyond Lean: How Multi-Project Management is Transforming Product Development at Toyota and Other Companies* (New York: Free Press).

[17] Prior to Peteraf (1993), cited earlier, another important reference for resources is B. Wernerfeldt (1984), "A Resource-based View of the Firm," *Strategic Management Journal*, 5(2): 171-180. I will briefly review some of the capabilities literature in Chapter 1, but a particularly useful reference is D. Teece, G. Pisano, and A. Shuen (1997), "Dynamic Capabilities and Strategic Management." *Strategic Management Journal*, 18(7): 509-533.

[18] See R. Schmalensee (1985), "Do Markets Differ Much?", *American Economic Review*, 75(3): 341-351; R. P. Rumelt (1991), "How Much Does Industry Matter?", *Strategic Management Journal*, 12(3): 167-185; and A. M. McGahan and M. E. Porter (1997), "How Much Does Industry Matter, Really?", *Strategic Management Journal*, 18 (Summer Special Issue): 15-30.

[19] See E. F. Vogel (1979), *Japan as Number One: Lessons for America* (Cambridge, MA: Harvard University Press).

[20] J. Abegglen and G. Stalk (1985), *Kaisha: The Japanese Corporation* (New York, Basic Books); C. Johnson (1982), *MITI and the Japanese Miracle: The Growth of Industrial Policy, 1925-1975* (Palo Alto: Stanford University Press); R. T. Pascale and A. G. Athos (1982), *The Art of Japanese Management: Applications for American Executives* (New York: Warner Books).

[21] See M. A. Cusumano (2005), "The Japan Problem as Paradox: Views from Abroad, in Good Times and Bad," unpublished working paper, "End of Japan? Conference," Honolulu, Hawaii, January 2005. A version of this is published in M. A. Cusumano and D. E. Westney (forthcoming), "Nihon no kyosoryoku ni taisuru obei roncho no henkan" [The change in Western commentary on Japan's competitiveness] in A. Takeishi, Y. Aoshima, and M. Cusumano, eds., *Nihon no kyosorykoku-ron [On Japan's competitiveness*] (Tokyo: Toyo Keizai Shimbunsha).

[22] On this topic, see M. E. Porter, H. Takeuchi, and M. Sakakibara (2000), *Can Japan Compete?* (Cambridge, MA: Perseus).

[23] For more particularly influential books on Japan, see also, as examples from a very long list, R. Dore (1973), *British Factory-Japanese Factory: The Origins of National Diversity in Industrial Relations* (Berkeley: University of California Press); R. Schonberger (1982), *Japanese Manufacturing Techniques: New Hidden Lessons in Simplicity* (New York: Free Press); and Vogel (1979).

[24] See K. Hopper and W. Hopper (2007), *The Puritan Gift: Reclaiming the American Dream Amidst Global Financial Chaos* (London and New York: I.B.Taurus), especially pp. 108-124.

[25] See, for example, M. A. Cusumano (1994), "The Limits of Lean," *MIT Sloan Management Review*, 35(4): 27-32.

[26] The most important early works from this point of view are generally considered M. Hannan and J. Freeman (1977), "The Population Ecology of Organizations," *The American Journal of Sociology*, 82(5): 929-964; and H. Aldrich (1979), *Organizations and Environments* (Palo Alto: Stanford University Press, classic edition 2008). See also M. Hannan and J. Freeman (1989), *Organizational Ecology* (Cambridge, MA: Harvard University Press).

[27] I have elaborated a bit more on this in M. A. Cusumano (2009), "Strategies for Difficult (and Darwinian) Economic Times, *Communications of the ACM*, 52(4): 27-28.

[28] See T. White (1978), *In Search of History: A Personal Adventure* (New York: Harper & Row).