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Introduction

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Business history ought to be an interdisciplinary subject. Because businesses are first and foremost economic units that make such decisions as how much of a good to produce, how to make it, and what to charge for it, the "dismal science" of economics inevitably has a role to play in understanding their behavior. At the same time, however, businesses are organizations of people whose choices are affected by the social and cultural environment in which they live and work. Hence understanding how businesses operated in the past—and why they succeeded or failed—is also an interpretive activity that requires the tools and sensitivity of historical scholarship.

Unfortunately, there is little communication today between economists and historians or even between economic historians (who are largely economists by training) and business historians (who typically come out of history departments). The former have organized themselves into the Economic History Association; the latter into the Business History Conference. Only a small number of people attend both sets of meetings. Moreover, the two groups of scholars largely subscribe to and publish in different journals. Economic histo-

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rians read the *Journal of Economic History* and *Explorations in Economic History*, and business historians the *Business History Review* and the conference's annual publication, *Business and Economic History*. There are relatively few articles in either set of journals that appeal to both groups of scholars.

Although this division can be traced back to the period before World War II, recent developments in economic theory now make it possible to bring the two groups of scholars together in writing a new kind of business history. In this introduction we first detail the history of relations between economic and business historians from the early twentieth century on. We then outline some aspects of the new theory that, we believe, will further interdisciplinary research in business history and make the work of business historians more interesting to the larger community of economists and historians. This book is the third in a series. The essays collected in this volume, as in the previous two (Temin 1991; Lamoreaux and Raff 1995), were originally presented at a conference organized to bring business and economic historians together. They are concrete examples of the kind of theoretically informed business history that we are promoting. Hence we conclude our introduction by situating this volume relative to the previous two and highlighting the contributions made by the individual essays toward a more complete understanding of how businesses—and other organizations—have functioned in the past.

Economic History and Business History

The field of economic history had its formal beginning in the United States in 1892 when Harvard created a chair in the subject and appointed British scholar William J. Ashley to fill it. A number of other universities followed Harvard's example and established similar chairs during the last decade of the nineteenth century. After the First World War, a new generation of economic historians assumed positions of leadership, and the field began a period of rapid growth. One of the most important of the new scholars was Edwin F. Gay, who had replaced Ashley at Harvard and who went on to lend his prestige and energies to a major program of institution building. Among the fruits of his efforts were the National Bureau of Economic Research, the Commission on Recent Economic Changes, the Commission on Recent Social Trends, and the Social Science Research Council. Underlying all of these organizations was Gay's belief that research in economic history, particularly the careful amassing of long-term quantitative data sets, would provide a vital foundation for both historical understanding and policy making. For similar reasons, Gay believed that it was important for archives to collect business papers, and he helped to found an association devoted to this end, the Business History Society. One of his students, N. S. B. Gras, the first Strauss Professor of Business History at the Harvard Business School, became a leader of the new subfield of business history; and from 1928 to 1931, Gay and Gras co-edited the *Journal of*

Economic and Business History, which aimed to bring together work in both subject areas (Sass 1986, 15, 29–43; Cole 1968, 558–59).

This collaborative effort soon foundered, however, over the very different conceptions the two men had about the direction that scholarship in business history should take. Gay and other economic historians at the time believed that business history should contribute to the synthetic view of economic history they were seeking to construct—that it was precisely because businesses were subjected to the discipline of the market that their records could provide insight into larger economic processes. Gras, on the other hand, had little use for the type of theorizing that characterized the more established field. He was a rigidly inductive thinker who believed that business behavior should be studied for its own sake, and that new generalizations would ultimately emerge from case studies amassed by scholars doing highly focused research on particular enterprises. He and Gay disagreed vehemently about the amount of such work the journal should publish, and the two men (and their respective fields) became increasingly estranged. Gay resigned his editorial position in 1931, and the journal folded the next year (Sass 1986, 42–45; Heaton 1952, 194).

Although Gras had a number of followers, many scholars interested in the study of business history soon grew frustrated with the particularism of his approach; and for a time, it seemed as if there would be a reconciliation of business and economic history. Economists like Arthur H. Cole, whose work fell within both subdisciplines, led a new wave of organization building that culminated in the 1940s with the founding of the Economic History Association, the Council for Research in Economic History, and the Center for Research in Entrepreneurial History at Harvard (Heaton 1941, 1965; Sass 1986, 54–59). As the history of the center illustrates, however, whatever reconciliation occurred was short-lived.

Scholars at the center were interested in reinjecting theory into the study of business history. Their starting point was Joseph Schumpeter's concept of entrepreneurship as a creative act that shifted the economy's production possibility frontier outward in a discontinuous fashion (Schumpeter 1934). Entrepreneurship was an important subject to study, they argued, because this kind of creativity was the key to sustained improvements in social well-being. However, Schumpeter himself was unable to explain why some societies at some times produce disproportionate numbers of entrepreneurs. Neoclassical price theory also appeared to lack answers to such questions, and so scholars at the center turned instead to sociological (particularly Parsonian) models of human behavior in order to understand why some cultures seem to offer particularly fertile ground for entrepreneurial innovation. The work of the most important historians associated with the center—David Landes, Thomas Cochran, and Alfred D. Chandler, Jr., are good examples—consistently employed concepts and addressed debates at the heart of this sociological literature, even when

they did not make extensive use of its rather arcane vocabulary and categories of analysis.[1]

Parsons's approach to the study of society was essentially an equilibrium one, and there was nothing inherently incompatible between the broad syntheses of business history developed by scholars at the center and the work of economic historians trained in economics. In recent years, indeed, economists as prominent as Oliver Williamson and David Teece have found much to admire in Chandler's model of the evolution of business organizations (Williamson 1981; Teece 1993).[2] But circumstances at the time made the differences seem more important than they actually were. During the early 1950s, concern about the causes of underdevelopment in large parts of the world led to heated debates about the role of entrepreneurship in industrialization. On the positive side, of course, were scholars at the center. The negative side was championed by Alexander Gerschenkron, also of Harvard, who in this debate stressed the role of natural-resource endowments, income levels, and the size of the domestic market (Abramovitz and David 1996, 50–57). The spirit of the negative view was essentially that of neoclassical economics, and Gerschenkron's students, along with those of the equally prominent economist Simon Kuznets, formed the vanguard of what came to be known as the New Economic History (or sometimes cliometrics), a brand of scholarship committed to the systematic application of neoclassical economic theory and formal hypothesis testing to the study of the past.[3]

The young scholars who led the cliometrics movement disparaged the importance of heroic individuals and so, ipso facto, the entire topic of entrepreneurial history. Douglass North, for example, famously downgraded the role of the entrepreneur in his *Economic Growth of the United States.* Entrepreneurs, he argued, did little more than respond to opportunities to maximize profits; their role was essentially passive. If Eli Whitney had not invented the cotton gin, someone else would have: "The growing dilemma of the South was that the demand for its traditional export staples was no longer increasing and its heavy capital investment was in slaves. . . . [I]nvention of the cotton gin can be viewed as a response to the dilemma rather than as an independent accidental development" (North 1961, 8, 52). Technological innovation was induced by changes in relative prices—that is, by market-driven opportunities for profit. There was no reason to devote time or resources to studying the entrepreneurial function in American business.

1. This search for theory often took the form of written scholarly debate in the pages of the center's in-house journal, *Explorations in Entrepreneurial History.* For a study of the process by which participants turned to Parsonian sociology, see Sass (1986, 107–223). For an analysis of the utility of this body of theory from someone associated for a time with the center, see Galambos (1969).

2. In addition, the faculty of the Graduate School of Business of the University of Chicago, well known for their orthodox economic views, awarded Chandler the Melamed Award for his book *Scale and Scope.*

3. For a more extended treatment of these opposing views, see Lamoreaux (1998).

As the cliometricians grew in strength and came increasingly to dominate the Economic History Association, business historians gradually abandoned that organization in favor of a recently formed alternative. The Business History Conference had its origin in a series of meetings, beginning at Northwestern in 1954, that brought together economic and business historians who were rebelling, again, against the atheoretical type of scholarship promoted by Gras. The group met twice in 1954, once in 1956, once in 1958, and then yearly thereafter; and in 1971 it transformed itself into a full-fledged professional association with dues, officers, a board of trustees, and a journal (albeit one that published only a single issue a year). Although many of its original members were economists and the association was initially formed to bring economists and historians together, from the 1970s on the conference increasingly served to provide historians fleeing the cliometric revolution with a new organizational base.[4]

To the present day, the Business History Conference is dominated by trained historians, whereas the Economic History Association is controlled by trained economists. Despite large areas of common interest, the professional reference groups, not to mention the norms about what constitutes interesting questions, pertinent evidence, and persuasive argument, sometimes seem alarmingly different. Moreover, in the absence of a compelling new interdisciplinary effort, this divergence seems likely to endure. The purpose of the conferences we have organized is to make such an effort and to bring business and economic historians together once again. It is our contention that recent developments in economic theory provide a historic opportunity for greater communication, and we think that the essays that resulted from these conferences show the new interdisciplinary approach to be uncommonly promising.

Beyond Traditional Neoclassical Economics

When today's business historians react negatively to the idea of using economic theory in their work, they typically have in mind a type of standard neoclassical modeling that no longer plays much of a role in advanced research. This older tradition dominated the discipline during the 1960s and 1970s, at the very time when the split between economic and business historians became embodied in rival organizations, and it is still taught in the economics courses that many budding historians take (and dislike) as undergraduates. Although the misconception is therefore understandable, the result has

4. Alfred D. Chandler, Jr., opposed the move to transform the Business History Conference into a formal organization because he did not want to abandon the Economic History Association to the cliometricians. His point of view did not prevail, however (videotape of "Heritage Session," consisting of informal remarks by Harold F. Williamson, Sr., Donald Kemmerer, Alfred D. Chandler, Jr., and Wayne Broehl [reading comments from Thomas Cochran], 34th Annual Meeting of the Business History Conference, Atlanta, 1988). We would like to thank William Hausman for providing us with a copy of this tape. We are also basing this account on the recollections of Louis Cain, communicated to Naomi Lamoreaux in an email message of 18 January 1996.

been to leave historians largely uninformed about recent work in economics, in particular the extent to which theorists have abandoned the neoclassical assumption that all economic actors make decisions on the basis of perfect information. Economists have now begun instead to reconceptualize the world as a place where information is scarce, imperfect, and costly, where bounds to the rationality of human beings affect their economic decision making, where institutional structures evolve in response to problems of imperfect information, and where economic processes can have multiple outcomes depending on both participants' past experiences and their perceptions of each other's actions. This new thinking has made economic theory much more useful for the writing of business history and vice versa.

Although traditional neoclassical economics purported to offer a "theory of the firm," in fact it was not centrally about firms. The neoclassical approach put the market at the core of the analysis and focused attention primarily on the determination of equilibrium prices and transacted quantities. Firms as such figured in the analysis only in such detail as was necessary to make the models of markets work. This treatment was no different from that which the theory accorded to other economic actors, but because firms typically are complex organizations composed of people who often have conflicting interests and goals, the effect was particularly unworldly. Neoclassical theory endowed firms with perfect knowledge of the marginal benefits and costs attached to all possible actions and with the ability to act both instantaneously and effectively. Whereas consumers were portrayed as maximizing utility, a concept that at least paid lip service to the idea that human beings may have different preferences, firms were depicted as maximizing the more objective concept of profits. Indeed, neoclassical theory treated firms as little more than equation-solving entities that, given market prices, determined output by equalizing marginal revenue and marginal cost.

Neoclassical economics' single-minded focus on markets, however, was not without its advantages. One important strength was its ability to tie the behavior of individuals and firms directly to competitive processes so as to highlight the disciplinary impact of the market. Another was the way in which it linked the actions of economic actors to a rigorously defined concept of social welfare. Competition was efficient in the sense that it led to a Pareto-optimal equilibrium—that is, to a situation where it was impossible to make any one person better off without making at least one other person worse off.[5]

More recent work has aimed at correcting some of the deficiencies of neoclassical theory's restrictive focus on market competition without losing all of

5. This statement is known as the fundamental theorem of welfare economics and can be found in any elementary microeconomics text. It is of more than abstract importance, as it has provided, for example, the intellectual basis for the recent deregulation movement. Critics of this view typically do not dispute the notion that competitive equilibrium is efficient in a static sense, but instead point out that Pareto efficiency is often achieved at the cost of a high degree of social inequality because there is nothing in these market processes that can correct an unequal initial distribution of income and wealth.

its advantages. The new research has progressed in a number of directions, and we summarized some of these (particularly principal-agent and transaction-cost analysis) in our previous volumes.[6] In the present essay, we focus on developments that explore the ways in which firms (and other organizations) learn: the role past experience plays in deciding what should be done in a world in which information is imperfect; how different firms, even within the same industry, can learn different things; and how the results of these varied learning processes can come to be embodied in individual firms' practices and structures, so that there is a great deal of persistent heterogeneity, even within a given industry.

The least formal and (to historians) most accessible version of this theory is the so-called evolutionary economics developed by Richard Nelson and Sidney Winter (1982). Although Nelson and Winter do not scrap the role of market discipline in their model, they place human actions, rather than relatively abstract market mechanisms, squarely at the center of their analysis. In their view, decision makers within firms want to maximize profits, but they do so by making choices in a trial-and-error and decidedly not omniscient fashion. One way of thinking about these choices is to imagine a decision maker considering what the value should be of some decision variable that he or she controls. How should the decision maker proceed? An optimizing but not omniscient actor might begin with the status quo and perform an incremental analysis. Would a small change in the value (in some particular direction) of a variable he or she controls increase profits? If the answer appears to be yes, the next step would be to repeat the exercise based on the new value. If the answer appears to be no, the decision maker would try an equally incremental change in the opposite direction. If the answer in both directions is no, the decision maker would stick with the status quo.

From the neoclassical perspective, this model of understanding decision making might appear to be little more than an explication of how firms get to equilibrium prices and quantities. But the difference in emphasis that is implicit in this focus on actions (as opposed to outcomes) has profound implications for what the student of firms ought to study. The reason is that the action-based approach immediately draws attention away from markets and market mechanisms in and of themselves and instead focuses it on such important questions as what decision makers actually know and how what they know affects what they do. The critical assumption that the evolutionary economists make—the assumption that distinguishes them most dramatically from their neoclassical predecessors—is that decision makers within firms in fact know very little about the opportunities and costs of unfamiliar or new procedures and activities. They have to feel their way along.

Because the level of uncertainty is so high, choices that yield positive out-

6. See the introductions to Temin (1991) and Lamoreaux and Raff (1995), and especially Raff and Temin, "Business History and Recent Economic Theory," in the former, 7–40.

comes often become embedded in the routines and organizational culture of enterprises. A business does something in a particular way and achieves good results. It then attempts to build on its success by exploiting this experience. It finds that expanding the same or similar practices to other areas in an incremental way is a profitable thing to do, in part because it has already mastered these practices. As a result, past ways of doing things gradually come to play a greater and greater role in the operations of the enterprise. This process of institutionalizing previous achievements can make it difficult for firms to respond to challenges that require them to operate in novel ways, but it can also be an important source of competitive advantage. Rivals may find a firm's success difficult to imitate because the routines in question depend to a very considerable extent on firm-specific knowledge and experiences. Successful imitation can involve replicating not only a product or production process but also the entire organizational environment that generated and sustained the innovation. It is often extremely difficult for other firms even to learn the outward details of what is required, let alone understand what underlies them.

The implications of this approach are thus very different from those of standard neoclassical theory. Under conventional assumptions, the neoclassical "theory of the firm" yields one global optimum—that is, one equilibrium level of prices and output toward which firms' incremental trial-and-error decision-making processes will inevitably push them. Competitive pressures force all enterprises to move toward the least-cost method of production—toward a technologically defined best practice. Otherwise they fail. On the other hand, the more action- and knowledge-oriented approach associated with evolutionary economics leaves open the possibility that firms' myopic decision-making processes may leave them in positions that are local, rather than global, optima. Where a firm ends up depends on where it started—on status quo ante decisions and patterns already established. From this perspective, a firm's costs are a function not only of the technology it employs but also of the organizational routines that put that technology to work. Competitive pressures still operate to keep firms on their toes, but successful firms, even within the same industry, can differ strikingly in ways that are remarkably persistent over time. Understanding the sources of such variation—and hence the nature of industrial competition itself—therefore requires both a general understanding of how economic actors are likely to behave under conditions of imperfect information and the ability to track the ways in which enterprises build up firm-specific routines and capabilities over time. In other words, it requires us to marry the time-tested methods of historical inquiry with insights from new theoretical developments in economics.

More formal versions of the new theory take as their starting point George Stigler's formulation of knowledge as an economic good (1964). Like all economic goods, knowledge has a cost, and so people and firms are only willing to acquire it if they expect its marginal benefit to exceed its marginal cost. Because it is common for the former to fall short of the latter, individuals and

firms often make decisions without the full range of potentially available relevant information. Whereas the analysis described above simply assumes that almost all relevant information is costly for all actors, these more formal alternatives allow for the possibility that groups may possess varying amounts and qualities of information because the costs they face in obtaining knowledge differ.

There has been a tremendous growth in recent years in the number and types of models that aim to elucidate the implications for organizational behavior and competitive outcomes of such differences in the cost and possession of information. Economics journals are full of papers embodying new assumptions that generate novel solutions. But the richness of this literature is also its Achilles' heel, because it gives the appearance that anything can happen in the absence of good—that is, cheap—information. Of course, this impression is false—there are many constraints operating to limit the choices that actors make. At the simplest level, the desire to pay one's bills or make a profit can limit the number of actions that seem wise; similarly, competitive forces can constrain the alternatives that are reasonable to take. Nonetheless, eliminating more than the most obviously unrealistic models requires concerted scholarly effort. Economists have approached this problem by refining their theoretical apparatus, but they have also begun to analyze real-world examples—actual company histories—for insight into the nature of the information that matters both for firm and market behavior (Gibbons 1997).

Business history is an obvious place for economists to turn for assistance. But in order for business historians to provide theorists with this kind of intellectual discipline, they have to communicate the import of their findings in a way that economists can appreciate. As the essays in this volume show, there is no requirement that historians adopt formal theoretical approaches in their work or even that they weave their narratives around abstract economic models. All that is necessary is that they share with economists a few fundamental assumptions about how human beings behave. Too often, historians have reacted to the limitations of the neoclassical approach by attacking the notion of economic rationality itself—by challenging the idea that at its heart economic behavior is fundamentally a matter of weighing the expected outcomes of alternative decisions in a systematic fashion. But this is throwing out the baby with the bath water. One does not have to assume that economic actors are all-knowing to believe that they make the most advantageous choices they can on the basis of the limited information they possess. Thanks to recent theoretical developments, economists are now employing a more commonsensical notion of rationality—one that business historians should be able to embrace without doing violence to any of their deeply held beliefs about the importance of context, ideas, or culture. As a result, historians can offer economists intellectual discipline simply by focusing their efforts on what they are well-trained to do: elucidating what economic actors actually know at any given point in time, how they use their knowledge to make informed choices, and how they learn

from their past decisions. In short, there is now the opportunity for renewed dialogue between those who analyze business decisions at an abstract level (economists) and those who analyze them in their individual, concrete, and idiosyncratic variety (historians)—dialogue that will not only help economists improve their modeling but will also provide stimulating new questions for historians to explore in their research.

Contents of the Current Volume

The two previous volumes in this series essentially asked the question, "What goes on inside firms?" They advanced the answer that business leaders have been centrally preoccupied with the management of information flows and asymmetries (situations where one party in a relationship has more or better information than another). The individual essays dealt with subjects as widely disparate as small watchmakers versus large automobile firms, or local mortgage banks versus investment behemoths like J. P. Morgan and Company, but they can be grouped into three broad categories. The first group traced efforts by business leaders to improve their knowledge of subordinates' activities, the second analyzed the ways in which alternative organizational arrangements could reduce informational asymmetries, and the third showed how legal and regulatory constraints could affect business's ability to cope with information problems. The collective impact of the essays was to demonstrate that many characteristic features of modern business organizations can be understood in terms of the scarcity and value of information.

Although some of the essays in the previous volumes dealt with learning processes, the present volume moves this theme to center stage by asking explicitly how firms, industries, and even nations can learn to overcome uncertainty. It shows that organizations—like people—learn different things in different ways and that this variation has implications for competitive outcomes. The essays in this volume thus mark a transition from focusing on problems that are common to a whole class of firms or industries to explaining why firms, groups, and nations can differ in important and persistent ways.

The essays in the first part of the volume explore some of the techniques that firms employ to create competitively valuable informational asymmetries and at the same time prevent unfavorable ones from arising. The starting point is the observation that the amount of information firms possess depends upon the extent to which they invest in collecting it. In "Inventors, Firms, and the Market for Technology in the Late Nineteenth and Early Twentieth Centuries," Naomi Lamoreaux and Kenneth Sokoloff argue that an active market for patented inventions developed over the course of the nineteenth century. In the "high-tech" parts of the economy in particular, firms seeking to stay on the technological cutting edge began to devote resources to building staffs of employees whose main function was acquire to knowledge about inventions developed outside the firm and decide whether the company should purchase rights to use

them. In an environment where many inventions were protected by patents and where this kind of property right was vigorously enforced, investment in collecting information about new developments throughout the economy was vital to success. A wrong decision about the value of a patent could mean that a competitor gained control of vital technology.

Because investments in capabilities of this sort were expensive, some firms turned to organizational substitutes. Many small firms, for example, used patent solicitors to keep abreast of inventions related to their interests. Although initially the main purpose of these legal representatives was to shepherd applications through the Patent Office and to defend patentees in interference and infringement proceedings, over time they developed specialized technical expertise that firms could tap to keep up with developments in the rest of the economy. Another organizational substitute was the patent pool. As Steven Usselman describes in "Patents, Engineering Professionals, and the Pipelines of Innovation: The Internalization of Technical Discovery by Nineteenth-Century American Railroads," pools were employed by large firms in industries (like railroads) where there was already a significant amount of interfirm cooperation. They enabled firms to collude in the acquisition of new technology. Because pools insured that all patents would be cross-licensed to participating firms, they eliminated much of the risk that a competitor would monopolize an essential patent and kept the cost of inventions low.

Such substitutes for direct investment in information collection could work quite well so long as most of the important new technological developments occurred in the external environment. Once internal R&D grew in significance, however, firms that did not invest in building up research capabilities often found themselves at a competitive disadvantage. Moreover, once technological change moved inside firms, its character shifted in important ways. As Usselman suggests, it became focused less on the acquisition of patents and more on the overall goal of increasing efficiency through systemization and standardization. Improvements of this sort typically involved a great deal of firm-specific knowledge. Unlike patents, therefore, they could not be traded on the market.

Although the purpose of patent pools was to reduce asymmetries associated with the acquisition of new technologies, firms could also use similarly structured organizations to alleviate competitive pressures resulting from other kinds of information problems. For example, with cartels illegal in the United States after the turn of the century, it was in firms' interests to develop alternative ways of coping with price competition. One device that grew in popularity during the 1920s was the open-price association, the main purpose of which was the collection and dissemination of information about prices and output. The idea was that, if firms had better information about these magnitudes, price cutting would be easier to detect, and so the incentive to increase market share by undercutting competitors' prices would be greatly reduced. However, as David Genesove and Wallace Mullin found in their study of one such associa-

tion, the Sugar Institute, firms were initially reluctant to make this kind of information available to competitors. The institute had to learn how to guarantee credibly that data on individual producers would be handled carefully and confidentially so that competitors would not gain any advantage from the exchange. Even so, firms were reluctant to share certain kinds of information, for example, detailed reports of their sales. At the root of the problem were informational asymmetries within the organization itself. Firms that were large relative to the market knew more about what their competitors were doing than firms that were small, and they were not willing to give up that advantage. This fundamental inequality in access to information proved impossible to overcome.

The second group of essays in this collection focuses on the ways in which firms built up special capabilities over time, capabilities that could be sources both of competitive advantage and of resistance to new kinds of learning. In his reconsideration of learning by doing in the production of bombers during World War II, Kazuhiro Mishina describes how Boeing's managers succeeded in effecting the enormous productivity increases needed to satisfy insistent demands from the military for additional planes. Over a period of only four years, the direct labor time it took to make a B-17 bomber at Boeing declined from seventy-one worker-years to a mere eight. Mishina shows that the prompt to productivity growth was not cumulative experience per se, but rather the dramatic increases in the flow of production that Boeing had to accommodate. Boeing's managers learned, in other words, because they were experiencing extraordinary new pressures on capacity.

Not all firms were able to respond so successfully to these pressures, however. Boeing's achievement, Mishina shows, resulted from its flexible use of space and its group assembly techniques. By contrast, Ford attempted to apply its automobile technology to aircraft production, transferring its mass-production assembly-line techniques to the manufacture of airplanes. These methods meant that space in Ford's plants was rigidly partitioned by assembly lines and dedicated to particular uses, a structure that made it difficult for Ford to increase its output rapidly in response to the skyrocketing military demand for planes.

Ford's more rigid system proved disastrous, and the firm emerged from the Great Depression and World War II nearly bankrupt. To rescue the failing enterprise, the new CEO, Henry Ford II, lured a whole team of executives away from General Motors. The GM people immediately set to work reinventing Ford along the lines of GM—that is, they set about replacing Ford's highly centralized organizational structure with the decentralized multidivisional form used at GM. As David Hounshell's chapter shows, the GM people seemed to be succeeding in restructuring Ford. In a key vote in late 1949, for example, the company's top executives approved a plan to build two new engine plants that would provide the firm with badly needed production capacity and at the same time further the process of decentralization. Less than a month later, how-

ever, the decision was reversed. Although there are no records that reveal what actually happened at that subsequent meeting, Hounshell goes to great lengths to elucidate the various possible influences shaping the decision.

Part of the explanation for the shift appears to be the difficulty of the project itself. Ford's centralized organizational structure was embodied in physical capital in the form of the huge River Rouge plant in Detroit. As the executives confronted the costs involved in dismantling the Rouge in order to transform Ford's organizational structure along GM lines, they seem to have backed away from the project. Rather than copy GM, Ford instead developed a new business strategy that made effective use of its own sunk investments in plant and in particular ways of doing things, a strategy that proved profitable in the next period.

When firms failed to exploit the capacities they had built up over the years, the results could be momentous. The case of Sears offers an instructive lesson. As the chapter by Daniel Raff and Peter Temin argues, Sears faced two important turning points during its history—the first in the mid-1920s, and the second in the late 1970s. During the first episode, Julius Rosenwald hired General Robert Wood to add retail stores to Sears's catalogue business. This expansion made good use of the expertise and goodwill that Sears had already accumulated and enabled the firm to hold onto its clientele as families became more urban, work moved from agriculture into industry, and people increasingly traveled by car. In the second episode, Sears's executives debated the firm's future path. One group wanted to follow what it thought to be General Wood's example and add new dimensions to Sears's retail activities; another group wanted to revitalize the company's stores. The first group won, and Sears expanded into financial services. But the hoped-for synergy between the sale of goods, on the one hand, and financial instruments, on the other, did not materialize. More important, the executives' misperception of the firm's special capabilities cost the firm many years in its contest with stores like Wal-Mart and The Gap, stores that increased their market share by using new information technology to lower prices and improve responsiveness to consumer demand.

The final section of the volume extends the notion of learning from the level of firms to that of nations. The idea is that nations, like firms, can make investments in specific ways of organizing economic activity—what we call institutions—that give them economic advantages in a world where information is imperfect and costly. Alfred D. Chandler, Jr., has argued that the large-scale enterprises that emerged in the United States during the early twentieth century (in large measure because cartels were illegal here) were responsible for the extraordinary performance of the U.S. economy (Chandler 1977, 1990). But Leslie Hannah's essay shows that this view will not withstand empirical scrutiny. Hannah tracks the performance of the largest firms in the United States, Great Britain, and Germany over the course of the twentieth century, and finds that large firms in general and U.S. firms in particular have not done especially well. Chandler assumed that managerial control of vertically integrated enter-

prises provided a coordination mechanism superior to any that could operate through the market, but this assumption has also been called increasingly into question.[7]

If large-scale enterprises do not account for the extraordinary success of the U.S. economy during the early twentieth century, what does? Hannah argues that the explanation for long-run national differences in economic performance must reside either in the nonindustrial sectors of the economy or in the achievements of small firms. Rising to the challenge that this kind of question poses, Gavin Wright has attempted to elucidate the particular "social capabilities" that allowed the United States to move into a position of world economic leadership by 1890.[8] In "Can a Nation Learn? American Technology as a Network Phenomenon," Wright focuses on the networks of people that made possible the transfer of technological knowledge throughout the nation. During the early nineteenth century this type of communication was facilitated by the high geographic mobility of labor, particularly the movement of skilled mechanics with a great deal of technological know-how. Over the course of the century, however, these networks became more formal as technological change increasingly became the work of engineers with college training and budding professional identities. The engineers organized themselves into national societies devoted to the promotion of their fields and spread their brand of specialized knowledge to like-minded people in other parts of the country.

Perhaps the most important of these networks centered on the mining industry. One common explanation for the U.S. economy's extraordinary performance by the early twentieth century was its abundant raw material resources, but Wright shows that the nation's share of resource production during this period greatly exceeded what we now know to be its share of reserves. What accounted for the superior U.S. performance, he argues, was not resources per se, but the capacity to exploit them that the nation had acquired through its network of mining engineers. The important lesson to take away from this example, then, is that nations like firms have business histories. That is, we can understand their success by studying the special organizational and institutional arrangements they developed to exploit informational asymmetries.

Conclusions

The three volumes we have published, and especially this last collection of essays, demonstrate through myriad examples that information is crucial to economic success. More than any other factor, the ability to collect and use

7. For example, Michael Enright (1995) has shown that small vertically disintegrated but geographically concentrated firms can develop coordination mechanisms that are superior to managerial hierarchies in their flexibility to respond to changes in consumer demand. Similar arguments about the advantages of clusters of small vertically disintegrated firms over large managerially directed enterprises have been made by scholars as diverse as Michael Piore and Charles Sabel (1984) and Philip Scranton (1983, 1989).

8. See also David and Wright (1992); Abramovitz and David (1996).

information effectively determines whether firms, industry groups, and even nations will succeed or fail. There are several more subtle points to underscore, however. First, although it is obviously important for firms and other organizations to learn about the external environment (for example, about market opportunities and technological developments), it is equally important that they be introspective—that is, that they devote resources to learning about the capabilities that their own ongoing operations are creating and to understanding the opportunities that are evolving as a result. Second, the outcome of these learning processes, especially the introspective ones, may well be quite firm-specific. As a result, economic organizations ranging from firms to nations may develop special capabilities that distinguish them in striking ways from the other organizations with which they compete. Finally, although the essays in these volumes have dealt with many very specific subjects and indeed have highlighted the idiosyncratic aspects of the individual firms and organizations studied, the essays nonetheless have an underlying coherence. All of the authors take the imperfect state of information as their starting point, and all aim to illuminate the ways in which this condition effects the playing out of economic life. This common preoccupation then leads to a deeper source of coherence—the structural unity behind all these various topics. The information problems that firms face in their internal operations are not so different from those that they face in dealing with the external environment or from those faced by firms and other economic actors when they interact with one another. Further, the solutions adopted in response to these information problems typically have many features in common, even though they often result in the creation of capabilities that are specific to the organization. In sharp contrast, therefore, to the case studies that characterized the Grasian tradition of business history, the essays in these volumes contribute to a coherent view of American economic development and organizational change.[9]

Fruitful exploration of these underlying ideas depends first and foremost upon genuine interaction between economists and business historians. These two groups of scholars do different things. Business historians are primarily interested in understanding changes over time in the behavior and structure of particular economic organizations. Economists are primarily concerned with building general models of economic relationships and with exploring the implications of the models they build. Despite their very different interests, however, the two groups of scholars have much to gain from intellectual exchange. As we have already suggested, business historians can turn to economic theory for intriguing new questions and for the light a coherent perspective sheds on an otherwise untidy past. On the other hand, business history can offer economists useful correctives and provocative examples that will inspire them to give their models heightened realism and greater practical significance. Business decisions, after all, are not abstract optimization exercises. They are embedded

9. For such a summary view, see Lamoreaux, Raff, and Temin (1997).

in contexts. Moreover, because past choices condition future ones, the study of these contexts has an important historical dimension—capabilities built up slowly over time affect firms' evaluation (and ability to take advantage) of new possibilities. Hence it is really only by marrying historical methods with insights from the new economic theory—by making business history truly interdisciplinary—that scholars can obtain a full understanding of the nature and determinants of competition and the sources of economic success.

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