New Economic Approaches to the Study of Business History

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Business history is by its very nature 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, their behavior is nothing if not the subject of economic theory. 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 inevitably an interpretive activity that requires the tools and sensitivity of historical scholarship as well.

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 historians 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.

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Although the split between business history and economic history is particularly acute at the present time, it can be traced back to the period before World War II. In this essay we first detail the history of relations between the two groups of scholars since the early twentieth century. We then outline a series of theoretical developments that, we believe, now makes it possible to bring business and economic historians together in the writing of a new kind of interdisciplinary business history. In the remainder of the essay, we use examples from the recent literature, and especially from papers given at a series of conferences we have organized over the past few years, to illustrate the possibilities of this new style of scholarship.[2]

Business vs. Economic 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 lead and established similar chairs during the last decade of the nineteenth century. After the First World War, a new generation of scholars assumed positions of leadership, and the field began a period of rapid growth. One of the most important of the new leaders was Edwin F. Gay, who had replaced Ashley at Harvard and who now went on to lend his prestige and energies to a new 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 sub-field of business history; and from 1928 to 1931, Gay and Gras co-edited the *Journal of Economic and Business History*, the aim of which was to bring together work in both subject areas [Sass, 1986, pp. 15, 29-43; Cole, 1968, pp. 558-9].

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,

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[2] The papers from the first two conferences have been published respectively in Temin [1991] and Lamoreaux and Raff [1995]. Papers from a third conference will appear in a third volume [Lamoreaux, Raff, and Temin, forthcoming].

had little use for the type of theorizing that characterized the more established field. He was an inductive thinker who believed that business behavior should be studied for its own sake and that new generalizations would emerge from the case studies amassed by scholars doing highly focused research on the internal operations of 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, pp. 42-5; Heaton, 1952, p. 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 sub-disciplines, led a new wave of organization building that culminated in the founding in the 1940s of the Economic History Association, the Council for Research in Economic History, and the Center for Research in Entrepreneurial History at Harvard [Heaton, 1941; Heaton, 1965; Sass, 1986, pp. 54-9]. As the history of the Center illustrates, however, whatever reconciliation that occurred was short-lived.

Scholars at the Center were interested in reinjecting theory into the study of business history. But many of them came to question the utility of conventional neoclassical economics for that purpose. The starting point of most researchers at the Center was Joseph Schumpeter's concept of entrepreneurship as a creative act that in discontinuous fashion altered – by shifting outward – the economy's production possibility frontier [Schumpeter, 1934]. Entrepreneurship was an important subject to study, they argued, because this kind of creativity was the key to greater social well-being. Schumpeter himself was unable to explain why some societies at some times produce disproportionate numbers of entrepreneurs, and neoclassical price theory also appeared to lack answers to such questions. 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 some of the most important historians associated with the center – good examples are David Landes, Thomas Cochran, and Alfred D. Chandler, Jr. – 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.[3]

Parson'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 these scholars and the work of

[3] 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 an excellent analysis of the process by which participants turned to Parsonian sociology, see Sass [1986, pp. 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].

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]. 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 the scholars at the Center. The negative side was championed by Alexander Gerschenkron, also of Harvard, who stressed in this debate the role of natural resource endowments, income levels, and the size of the domestic market [Abramovitz and David, 1996, pp. 50-7]. 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 [Lamoreaux, forthcoming].

The young scholars who led the Cliometrics movement disparaged the importance of heroic individuals and so, ipso facto, the entire topic of entrepreneurial history. They subscribed instead to the view that technological innovation was induced by changes in relative prices – that is, by market-driven opportunities for profit. Douglass North, for example, famously downgraded the role of the entrepreneur in his *Economic Growth of the United States*, seeing no reason to devote time or resources to studying the entrepreneurial function in American business. 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, pp. 8, 52].

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 new association, the Business History Conference. 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, once 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 only published a single issue per year). Although many of its original members were economists, starting in the 1970s the Conference increasingly provided an intellectual home

for historians fleeing the Cliometric Revolution.[4] To the present day, the Business History Conference is dominated by trained historians, whereas the Economic History Association is controlled by trained economists.

In recent years, this gulf has if anything grown wider as a result of a shift in historical fashions in favor of cultural, as opposed to social or economic, history. This shift has effectively redefined historical studies, in the words of a recent commentator, "as the investigation of the contextually situated production and transmission of meaning" and has inspired historians to take a "linguistic turn" – that is, to turn to the hermeneutics of poststructural literary theory rather than to the abstract models of the social sciences for inspiration and guidance [Toews, 1987, p. 882]. Within business history, the new emphasis has reinforced a growing wave of dissatisfaction with the Chandlerian synthesis that has dominated the field of business history for the past twenty years. This dissatisfaction stemmed in part from the growing conviction that Chandler had overemphasized the efficiency gains to be derived from the managerial coordination of economic activity within large-scale enterprises [Piore and Sabel, 1984; Scranton, 1983; Scranton, 1989]. But it also manifested itself as a rebellion against Chandler's narrow focus on the business organization itself and thus has spurred scholars to work on a variety of previously unexamined issues – for example, the culture of corporations or the way in which gender constructs become embodied in business practice [Dellheim, 1987; Kwolek-Folland, 1994]. Although this broadening of research interests is unquestionably a positive development, its downside has been a tendency for business historians to revert to the writing of largely unrelated case studies in a fashion reminiscent of Gras.

These developments have also moved the field even further away from economic theory than before – a result that is particularly unfortunate, because it is occurring at a time when economics has more to offer business historians than ever before. Abandoning the convenient but unrealistic assumptions of traditional neoclassical theory – in particular the assumption that all economic actors make decisions on the basis of perfect information – economists have begun to reconceptualize the world as a place where information is scarce, imperfect, and costly, where the "bounded rationality" of human beings affects their economic decision making, where institutions develop in response to problems of imperfect information, and where economic processes can have

[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, Georgia, 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 e-mail message of Jan. 18, 1996.]

variant outcomes depending on participants' past experiences and their perceptions of each other's actions.[5]

The questions at the heart of this new work – how do economic actors know what (they think) they know about their world, and how does what (they think) they know affect their behavior – are remarkably similar to those that inform the work of business historians interested in adding a cultural dimension to their work. But, of course, the theorists who are participating in this intellectual movement are interested, as is their wont, in developing formal economic models that capture the new assumptions about information and in exploring the general implications of these models. Their work is mathematical in character and, to the uninitiated observer, often appears to bear little or no connection to actual circumstances, whether current or historical. The aims of these scholars are thus very different from those of historians, who are more interested in understanding specific historical phenomena. Indeed, the intellectual agendas of the two disciplines appear to be so dissimilar that it might seem doubtful whether, on their own, practitioners could ever come to appreciate, let alone learn anything, from each other's work.

The purpose of the series of conferences we have organized has been to try to bridge the gap – to show how these recent theoretical developments in economics might be used to write a new kind of business history that simultaneously speaks to the concerns of the new generation of business historians and piques the interest of economic theorists. In the remainder of this essay, we pull examples from papers presented at our meetings and from other recent work in order to introduce the new theory to the uninitiated and to highlight its potential contribution to business history.

Asymmetric Information Within the Firm

Traditional neoclassical theory assumed that economic actors were rational beings who made optimizing decisions on the basic of perfect information and foreknowledge. This highly stylized view of human behavior was a useful simplification that enabled economists to deal with certain kinds of otherwise intractable problems, especially concerning markets, in an effective way [Friedman, 1953]. As scholars have increasingly come to realize, however, it was inappropriate for other applications – for example, understanding the behavior of individual firms. Traditional neoclassical theory treated the firm as a black box – as an equation-solving entity that determined prices and output by setting marginal revenue equal to marginal cost. It was completely incapable of dealing with firms as complex organizations composed of people with differing experiences and goals. Nor was it capable of explaining how firms would respond strategically to the uncertain environment in which they operated.

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[5]For further references and introduction to the economics of imperfect information, see Raff and Temin [1991].

The large-scale managerial organizations that emerged at the beginning of the twentieth century posed particular problems for the neoclassical theory of the firm. The owners of these enterprises were the stockholders, who might, as in traditional theory, be conceptualized as seeking to maximize profits (though even here questions such as the relevant time horizon stubbornly surface). But stockholders typically did not run the enterprises they owned. This responsibility they delegated to managers who were likely to have a very different set of priorities. For example, managers might be concerned with maximizing their own compensation, guaranteeing themselves long-term job security, exploiting perks in order to demonstrate conspicuously their high status, or establishing reputations that would enable them to secure more important positions in other firms. In order to guarantee, therefore, that firms operated in accordance with neoclassical theory, stockholders had to be able to impose their own priorities on managers. But this condition was unlikely to hold. Stockholders' power was often weakened by the organizational structure of the firm and by the dispersion of shares among a large and varied population of individuals. In addition, stockholders typically had very poor information about what managers were actually doing and therefore little ability to check their behavior on a regular basis.

Analogous problems for neoclassical theory appear at all levels of the large-scale enterprise. The interests of lower managers, for example, may be different from those of managers at the top of the organization, and the interests of workers may diverge from the concerns of their supervisors. These difficulties are known in the recent theoretical literature as principal-agent problems. The principal's responsibility is to specify goals for the agent and, in order to influence the latter's action, set the rules that determine how much the agent will earn. Given these rules, the agent chooses an action from a number of alternatives, which action may or may not be the outcome desired by the principal.

In the normal case – the one that is also the most interesting for historians – the information available to the two parties to the relationship is not the same. In particular, the principal is not able to observe directly the agent's action. Moreover, because there may be other factors that also affect outcomes, the principal is not able to infer the agent's action with any degree of certainty from the results obtained. Such a situation is characterized by what is called "asymmetric information," because the agent knows more about his or her own actions than the principal does. To give some examples, the stockholders of a firm are the principals when they set compensation for the firm's CEO, their agent. The managers of the firm are in turn principals when they determine working conditions and payment rules for workers, their agents. The government acts as a principal when it designs and operates a patent system to induce potential innovators, its agents, to discover new products and processes. In each case, the principal(s) cannot observe the effort put forth by the agent(s) – only its result. And the result may be the effect of chance or other factors as well as the agent's effort.

Economists have fruitfully distinguished two broad classes of asymmetric information problems. In the first (called hidden action or, sometimes, moral hazard), the agent takes an unobservable action after contracting with the principal. For example, after negotiating employment contracts with their bosses, workers have to decide how much effort they are actually going to expend on their jobs. They may decide to work less than their employers expect if they judge the compensation insufficient or if they think their shirking will go undetected. The second class of problems (called hidden information or, in some cases, adverse selection) occurs when the agent has better information about some relevant characteristic than the principal. For example, workers have more knowledge of their own abilities than do potential employers. Therefore, an employer who sets wages too low will end up with an applicant pool disproportionately composed of the poorer sort of worker. Similarly, if the government fails to create secure property rights in invention, serious inventors may be scared away and only charlatans advance new ideas.

The task faced by the principal, therefore, is to design a set of rules that will attract good agents to the activity and elicit their maximum effort. Operating under conditions of asymmetric information, the principal has to forecast the agents' reactions to the rules he or she designs. In the end, moreover, the principal may not even know whether the rules formulated were the best ones possible because outcomes do not provide certain guidance. The principal cannot know, for example, if a successful result owed to the agent's strong efforts or was simply a matter of good luck [Arrow, 1985].

One setting in which these issues arise is the design of employee compensation systems. Because high-quality information about subordinates is often expensive to gather, superiors typically seek ways of economizing on monitoring costs. One technique for reducing costs that has been discussed extensively in the literature is to structure compensation systems in such a way as to align subordinates' interests with those of their superiors. For example, managers might be remunerated at least in part in shares of stock in order to increase the identity of interests between themselves and the firm's shareholders. Similarly, firms may institute incentive schemes that reward individual workers according to their productivity. As Daniel Raff has shown in "The Puzzling Profusion of Compensation Schemes in the Interwar Automobile Industry," the technology a firm employs affects the costs it faces in monitoring its workers and thus the attractiveness of alternative compensation schemes [Raff, 1995]. For example, plans that rewarded individual achievement made sense where artisanal modes of production prevailed – that is, where individual mechanics made substantial fractions of cars. Firms that used more integrated methods, however, were better off using group compensation schemes, because it did the company no good to encourage an individual to be more productive than the other workers on the line. Finally, firms like Ford, which used both mass-production (interchangeable parts) and assembly line methods were better off paying flat wages. This particular technology made it easier and cheaper to monitor workers' efforts because it simplified the tasks that individuals had to accomplish and

allowed machines to set the pace of work. As a result, it was no longer necessary to use incentive schemes as a substitute for information gathering. All that was necessary was to pay workers a wage that made them want to keep their jobs, and fire those that did not keep up. Raff shows that as mass production methods spread through the industry, firms abandoned incentive plans in favor of straight-wage compensation schemes.

Raff's article shed light on the principal-agent literature by showing how the specific character of automobile technology affected the nature of the rules that managers (principals) set for workers (agents). It was also a contribution to the historical literature because it explained why automobile workers during the interwar period were paid in such very different ways, and perhaps more important, gave evidence of the slow diffusion of Ford's mass production methods through the industry. However, Raff's study only serves as an introduction to the possibilities for historical understanding that the new economic theory opens up. Once one begins to think about firms in the context of imperfect information, a whole host of new questions comes to mind. For example, although compensation schemes could be used to economize on the need for information about workers' efforts, managers still had to keep records of other aspects of their business dealings and had to be able to call up that information as needed. As JoAnne Yates demonstrated in "Investing in Information," the development of efficient information storage and retrieval techniques was not a trivial problem for large firms in the late nineteenth century [Yates, 1991; Yates, 1989].

Yates details a series of innovations in the handling of information that revolutionized managers' ability to keep track of their business dealings. Among these were such familiar (to us) devices as the file cabinet, the typewriter, and carbon paper. Previously, businesses kept records of their outgoing correspondence by using presses to make impressions of letters while the ink was still wet – difficult-to-read copies that were then bound in chronological order into large books. Incoming correspondence was folded and placed in boxes, again typically in chronological order. Such a system of record keeping made it very difficult for firms to access information from the past. Managers were often unable to locate correspondence dealing with particular transactions, and even when the information was found, the search was time consuming and labor intensive. The innovations Yates details solved this problem by enabling firms to copy outgoing correspondence in an efficient and readable way, and to store both incoming and outgoing letters together in clearly labeled files. Moreover, other innovations allowed firms to make use of the new retrieval system for internal management. Mimeograph machines meant that managers could duplicate and send memos to subordinates, who would now be able to store the directives in appropriate files. In addition, printed forms enabled managers to collect information about their internal operations, data which could then be analyzed and stored for comparison with succeeding periods.

But what information should firms gather to improve their performance? As Margaret Levenstein has shown in her study of the Dow

Chemical Company, the information that firms collected depended on their business strategy [Levenstein, 1991; Levenstein, forthcoming]. Dow (like most firms during the late nineteenth century) initially employed standard mercantile accounts to keep track of transactions with suppliers and customers and to monitor employees for honesty. As the firm began to develop new products and processes, however, this type of information system proved increasingly inadequate, and Dow experimented with ways of measuring production costs and technical efficiency. The more products the firm produced, however, the more difficult were the accounting problems. What the firm really needed was price-cost differentials and net income figures for individual products, but these, it turns out, were not easy measures to develop because of the many problems involved in allocating the burdens of fixed costs and of shared inputs among a firm's various outputs.

The problem with accounting rules developed for such purposes is that they are artificial constructs which, if not used extraordinarily carefully, can have perverse effects on subordinates' behavior. Whether they be managers or workers, subordinates will naturally concentrate on improving along whatever magnitude is being used to evaluate their performance, even if the actions they take are contrary to the good of the enterprise as a whole. For example, H. Thomas Johnson and Robert S. Kaplan have shown that, as a result of the growing power of finance departments within large firms in the mid-twentieth century, it became common practice for firms to "roll back" their financial accounts and use them to measure operational performance at the plant level – a practice that, they argue, had such detrimental consequences for American industry that it may explain much about its declining competitiveness in recent years [Johnson, 1991; Johnson and Kaplan, 1987].

According to Johnson and Kaplan, in the first half of the century businesses used a variety of different types of information – some financial but much not – to manage the enterprise internally. Beginning in the 1950s, however, businesses began to use the financial accounting techniques for internal management purposes that were initially devised to demonstrate the creditworthiness of the company to the external world – that is, they began to use these accounts for a purpose not originally intended. In order to generate public financial statements, firms had to distinguish between goods sold (the cost of which is an expense deducted on the income statement) and goods still on hand (an asset listed on the balance sheet as inventory) and be able to attach appropriate values to each. This was a fairly straightforward problem for direct costs like raw materials. But it was a much more complicated problem for indirect costs (overhead), and the most common procedure was to prorate them over the direct labor hours expended on each product. Such a procedure was fine for financial reporting purposes, but when this type of accounting system was used internally to monitor and reward managers, it created incentives that had highly pernicious consequences. For example, under this system the tracking schemes used within the firm for labor and machinery typically reported direct costs per unit of output, but for indirect costs, reporting schemes typically tracked the percentage of overhead covered or

earned by units produced. The goal of these reporting schemes was to have all recorded direct labor or machine hours go toward production of units of output and thereby absorb direct and overhead costs. When this happened a manager was regarded as efficient. Under this system, therefore, managers were rewarded for scheduling workers and machines on long production runs – that is, there was an incentive not to devote time to categories of indirect or nonchargeable time such as changeovers or setups. The problem, however, was that under this type of accounting procedure, it did not matter whether the output was really salable. Stopping a line to correct a production defect counted against the efficiency of the department, so managers had an incentive to keep lines moving regardless of quality. Indeed, under these systems, hours spent on allowable rework were often considered to be efficiently covered, so workers might spend hours piling up products with defects that they would later spend hours reworking. Clearly, this behavior was not efficient. On the other hand, it was elicited by the accounting system that was in place.

The spread of this kind of accounting system during the postwar era was to a large extent a function of changing power relations within large firms as a result of the diversification movement and the adoption of decentralized, multidivisional organizational structures. The new "M-form" of organization increased the distance between managers in the operating divisions and those at the top of the organization. The latter now often had very little manufacturing experience and were generally unfamiliar with the process-specific physical accounting measures that had previously been used to assess the performance of individual plants. Trained in rate of return accounting at business schools and determined to find some way of comparing the performance of producers making very different kinds of goods in far-flung parts of the organization, they imposed the new system on the operating divisions. Managers in the operating units would have preferred other measurement systems, but they had lost the power to make their opinions heard.

As this discussion of accounting suggests, managers located in separate parts of a company may have very different ideas about how the enterprise should be run. Bernard Carlson has generalized this idea by arguing that firms should be thought of as collections of interest groups, each with its own "business technological mindset." The balance of power among these groups then determines how the firm will behave. Carlson developed this idea in his study of the Thomson-Houston Electric Company [Carlson, 1995; Carlson, 1991]. He showed that there were three powerful groups within the firm: a marketing and finance group organized around Charles A. Coffin at the firm's headquarters; a manufacturing group led by Edwin Wilbur Rice, Jr. at the Lynn plant; and an invention group headed by Elihu Thomson that worked out of the firm's model room. As each of these groups pursued their own functions, they developed distinct mindsets that affected how each thought the firm should operate. Their ideas about proper strategy often came into conflict with each other, and these clashes had to be worked out before action could be taken. Because none of the groups had perfect information or foreknowledge, the strategy the firm pursued cannot be regarded as the outcome of any strict

logic of the market, however successful it ultimately proved to be. Rather the strategy resulted from the political give-and-take over time of these three major groups, each pushing for what it thought was in the best interests of the firm.

Inspired by Carlson's concept of a business technological mindset, we return to the subject of compensation systems, the topic with which we began this discussion. Raff argued that the profusion of compensation schemes in the interwar automobile industry resulted from the different technologies that firms in operation at that time employed. However, we may also wish to ask why, during this particular period, firms not changing their basic technologies would change their incentive plans at all. Daniel Nelson provides an answer by arguing that this experimentation was part of the ethic of industrial engineering, a movement that swept through the manufacturing sector during the early twentieth century as part of the coming to consciousness of a new generation of professional managers [Nelson, 1995]. Although, in its initial conception, the industrial engineering movement encompassed a much broader set of ideas than compensation schemes, as consultants competed with each other for clients, they found themselves under enormous pressure to deliver quick results. Incentive schemes promised to yield increases in productivity in relatively short periods of time, and so became the focus of their efforts.

These last few examples in particular underscore the consequences that follow from abandoning the neoclassical assumption of perfect information. Understanding a firm's behavior is no longer simply a matter of calculating marginal revenue and marginal cost. Now the importance of the particular historical context has to be recognized. To understand how decisions are made, one has to take into account the technology employed by the firm, the way in which power is distributed within the organization, the knowledge structures at the disposal of different groups within the enterprise, the goals and aspirations of these various economic actors, and the way in which their concerns link up with broad intellectual movements in the larger society.

But abandoning the assumption of perfect information does not mean abandoning economic theory. The overview we have just presented would not have been possible without the discipline of questions derived from theory. What problems does imperfect information pose for the relationship between principals and agents? How can principals restructure the relationship to economize on information? How do informational imperfections shape the decisions and behavior of actors within a firm? And, finally, how does the structure of the enterprise affect the way in which people located in different parts of the enterprise are able to make use of the information they possess? Unlike the pursuit of questions derived from the standard neoclassical model, however, our ability to answer these questions is enhanced by the use of techniques from outside the discipline of economics. It helps to know what managers were thinking about in order to understand just what they were doing.

Asymmetric Information Between Firms

Thus far we have focused on the ways in which asymmetric or imperfect information affected the internal operations of firms. But firms also faced information problems when they confronted their external environment. As we will see, firms dealt with the uncertainty that surrounded them in a number of ways: they exploited whatever informational advantages they possessed, altered their business strategies to pursue activities that entailed less uncertainty, created organizations that either economized on information or reduced the risks of making wrong decisions, and built special capabilities that enabled them (and not their competitors) to exploit informational asymmetries. As we will also see, the best way to understand these behaviors is once again to combine inquiry disciplined by the new economic theory with the techniques of historical scholarship.

Nowhere is the problem of imperfect information clearer or more insistent than in the financial sector, where lending institutions have to scrutinize the creditworthiness of those who come to them for funds. In theoretical terms, financial institutions function as principals when they provide resources to support the activities of their agents (borrowers). As was the case with managers and employees, banks and other financial intermediaries cannot know everything they would want to know about the individuals and firms they finance. Therefore, as principals they want to design ways of lending that avoid moral hazard and adverse selection. If firms, for example, were not held accountable for the funds they received, their managers might simply run off with the money. Or if banks charged such high interest rates that normal firms could not afford to borrow, only poor risks willing to take great chances would apply for loans. The first case, where borrowers act badly is an example of moral hazard; the second, where only bad risks borrow, is an example of adverse selection.

In relatively undeveloped economies, information about borrowers is scarce and of uncertain quality. In such economies bankers have a great deal more information about the creditworthiness of borrowers who are close to them than they do about strangers. Naomi Lamoreaux argues that the practice of banks lending a large proportion of their funds to insiders, common in early nineteenth-century New England, made good economic sense under these circumstances [Lamoreaux, 1991; Lamoreaux, 1994]. The main danger of such insider lending was that excessive loans to those closely connected with the banks' officers would endanger the financial health of the institution, but early New England bankers had strong incentives not to lend too much money to themselves or to their friends and relatives. Their positions in the bank gave them privileged access to loans only so long as their bank remained in sound condition. Moreover, in an economy where information was scarce, reputation counted for a lot. If a bank's officers were to cause their institution to fail, not only would they lose their main source of funds, but the damage that such a failure would cause to their reputations would make it difficult for them to continue in business. Given these pressures for restraint, Lamoreaux argues, insider

lending actually served a useful function in this information-poor economy. Because it was common knowledge that each bank's portfolio consisted in large measure of loans to bank insiders, and because the entrepreneurial activities of these insiders were readily observable, people who had funds to invest had a great deal more information about bank portfolios than they had about other possible investments. They could also benefit from the diversity of the activities in which these groups of insiders typically engaged. As a result, funds flowed freely into the New England banking sector in the early nineteenth century and from there into economic development more generally.

As the economy matured, the quality and quantity of information that was available to bankers improved, but the problems associated with lending to strangers did not completely disappear. As Lamoreaux argues, when banks began to lend more of their funds to arms-length borrowers, they increasingly restricted their business to short-term commercial lending. This type of lending reduced risks by keeping borrowers on a short leash and by confining a bank's business to the types of loans about which information was most likely to be available, but the cost was that banks effectively abandoned their support of economic development. Moreover, as Kenneth Snowden has shown, other types of financial institutions faced even more serious information problems during this period. For example, in the case of mortgage lending the problems were so severe that it was difficult to mobilize funds from outside the immediate area and, as a result, interest rate differentials between capital rich and capital poor areas were huge [Snowden, 1995]. Only someone on the spot could have the necessary knowledge of local real estate values and which borrowers were likely to be good credit risks. Only someone on the spot could make sure that property used as security for a debt did not depreciate in the hands of the borrower. Finally, only someone on the spot could take over and manage the property in the event of a default. Because interest rate differentials made the returns from interregional lending so attractive in the late nineteenth and early twentieth century, lenders experimented with a variety of organizational arrangements (for example, the use of mortgage brokers and local agents) to reduce the informational asymmetries. But these arrangements introduced new agency and information problems of their own, problems which produced the boom-bust cycles that are so familiar to students of the period. Only when the federal government stepped in to provide mortgage guarantees after the Great Depression were the informational problems overcome, allowing the mortgage market to become truly national for the first time.

Similar information problems plagued investment in industrial firms. For this reason, during the nineteenth century individuals with savings rarely bought equity in manufacturing enterprises, except for a small number of well-known local firms whose stock was traded on the regional exchanges. Charles Calomiris has argued that in nations like Germany, where banks pursued a set of policies known under the rubric of universal banking, these information problems were largely resolved [Calomiris, 1995]. Banks took equity positions in the firms to which they lent funds and also placed directors on the firms' boards. In this manner, they were able to gain more information about the

firms' internal operations than was feasible for individual lenders. Banks thus served as "delegated monitors." Because they had both superior information about industrial firms and also diversified portfolios, savers who were not willing to invest directly in industry could put their money in banks which in turn would invest in industry for them.

As both Lamoreaux and Calomiris have argued, by the late nineteenth century most commercial banks in the United States were unwilling to perform this function. But there were some private bankers willing to take it on. The best known was J.P. Morgan. During the late nineteenth century Morgan had confined his activities on the New York Stock Exchange largely to the finance of railroads, but during the Great Merger Movement of 1895 to 1904, he began to underwrite the creation of giant industrial consolidations. Typically, he would invest in the enterprise as well as underwrite it, and then put one or more of his associates on the board so as to be better able to monitor (and influence) the firm's activities. Bradford De Long has attempted to measure the economic benefits of such a participation by Morgan [De Long, 1991]. He found that firms with Morgan partners on their boards earned higher profits and greater returns for their stockholders than other large firms at the same time. The inference drawn is that J.P Morgan & Company's service as a delegated monitor reduced information asymmetries and helped insure that the managers of these consolidations would act in the interests of their stockholders.

Although the problem of acquiring information about the external environment is particularly clear for the banking sector, firms in other parts of the economy faced similar difficulties. For example, in order to be able to dispose of their goods, manufacturing firms needed accurate information about the pricing and output decisions of their rivals. This need became especially great once firms grew large relative to the market and could actually set prices, instead of having to behave like the price-takers of the standard neoclassical model. Moreover, as firms' cost structures became increasingly dominated by fixed capital expenses, the temptation rose to try to increase their market share by undercutting rivals' prices. In a situation of perfect information, firms would quickly learn not to give way to temptation. Their price cuts would be immediately detected by competitors and copied; market shares would remain as they were; and everyone would end up selling their goods for less than before. In a situation where information was imperfect, however, it was possible for a firm to keep a price cut secret from its rivals long enough substantially to increase its sales. During the late nineteenth and early twentieth centuries, for example, there were many new industries in which firms had not yet had time to establish their positions and take the mettle of their competitors. In addition, the dramatic ups and downs of the business cycle made it difficult for firms to tell whether a drop in sales was caused by a fall in demand generally or by the price cutting of a rival. Under such circumstances, the temptation to undercut rivals' prices seems to have been very strong, and firms turned to various kinds of collusive organizations to try to halt the downward-spiraling price competition [Lamoreaux, 1985].

Under similar circumstances European firms organized cartels to control output and fix prices at remunerative levels. In the United States firms also turned to cartels, but with much less success, and ultimately turned instead to horizontal mergers. In order to explain why cartels worked for Europeans and not Americans, scholars have focused on legal differences. For example, Tony Freyer traced the evolution of legal traditions in the United States and Britain to show why the organizational choices made by large firms were different in the two nations. Whereas in Britain cartels evolved from being legal but unenforceable contracts to enforceable ones, in the United States they were not only unenforceable by the end of the nineteenth century but illegal as well [Freyer, 1992, 1995].

Mergers did not usually succeed in eliminating price competition, however. The high prices the consolidations charged in the years immediately following their formation stimulated a host of entry by new firms. The consolidations gradually lost market share, and their industries typically reverted to an oligopolistic market structure. With cartels illegal in the United States, firms had to develop alternative ways of coping with price competition. One device that grew in popularity during the 1920s was the open price association, whose main purpose 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 association, the Sugar Institute, firms were initially reluctant to make this kind of information available to competitors and 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 [Genesove and Mullin, forthcoming]. 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 information asymmetries within the Association 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.

As this discussion of the Sugar Institute has suggested, firms can differ in their command of information about the external environment. In the sugar case, the differences arose primarily as a result of variations in size: large firms had more information about market activity than small. But in other cases, the information firms obtain may be a function of the extent to which they invest in collecting it. Naomi Lamoreaux and Kenneth Sokoloff argue, for instance, 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 invested in staffs of employees whose main function was to acquire knowledge about inventions developed outside the firm and to assess whether the company should purchase them [Lamoreaux and Sokoloff, forthcoming]. David Mowery has also pointed out the importance of such capabilities, arguing that even when large firms built up

their own internal R&D departments, monitoring technological developments in the external environment remained an important aspect of their activities. According to Mowery, tighter antitrust enforcement by mid-century made it difficult for large firms to continue to purchase externally generated inventions, and so they dismantled some of their investments in such capabilities, to the detriment, Mowery argues, of the economy's overall pace of technological development [Mowery, 1995].

Because investments in capabilities of this sort were so expensive, some firms turned to organizational substitutes. For example, many small firms used patent solicitors to keep abreast of inventions relative to their interests. Although the main purpose of these legal representatives was initially to shepherd applications through the Patent Office and to defend patentees in interference and infringement proceedings, they developed over time specialized technical expertise that firms could tap to keep up with developments in the rest of the economy. Steven Usselman describes another substitute for investing in the capability to assess externally generated technologies: patent pools [Usselman, forthcoming]. These organizations were employed by large firms in industries where there was already a large amount of interfirm cooperation. Essentially what these pools did was enable firms to collude in acquiring new technology. By jointly purchasing and cross licensing new inventions, they eliminated the risk that a competitor would monopolize a vital technology. They also kept the cost of acquiring inventions low.

Such substitutes worked fairly well so long as most important technological developments occurred in the external environment. Once internal R&D grew in significance, however, firms that did not invest in building up these capabilities often found themselves at a competitive disadvantage. Moreover, once technological change moved inside firms, its character changed in important ways. As Usselman has suggested, it became less focused 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.

Dynamic Consequences

One strand of the recent theoretical literature has extended this point to argue that particular ways of doing things become imbedded in the routines and organizational culture of an enterprise and can themselves become an important source of competitive advantage [Nelson and Winter, 1982]. They become established in the first place because they work. An enterprise innovates in a particular way, achieves good results, and then attempts to build on its success by expanding the same or similar practices to other areas. Because the routines that result typically depend on considerable firm-specific knowledge and attributes, competitors find them difficult to copy. Successful imitation involves replicating not only a product or production process but also the organizational resources 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. This strand of the literature thus links the concept of asymmetric information with historians' methods of understanding in an explicit way. Firms are successful over the long run because, by building on past choices and experiences, they are able to exploit their own private and highly specialized knowledge.

Although firm-specific organizational routines can be a source of competitive advantage, they can also make it difficult for firms to respond to competitive challenges or move into new technologies. The point can be seen dramatically by comparing Ford's experience in aircraft manufacture with Boeing's during World War II [Mishina, forthcoming]. Ford attempted to apply its automobile technology to aircraft production, transferring its mass-production assembly-line techniques to the manufacture of airplanes. But 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. As Kazuhiro Mishina shows, Boeing's more flexible use of group assembly techniques, which harked back to an earlier stage of automobile production, permitted much greater output growth over the war period. As Boeing's managers faced insistent demands from the military for additional planes, they were able to rearrange their plants so as to make more efficient use of the available space and permit a greater throughput. The results were dramatic: in the space of four years, the direct labor time it took to make a B-17 bomber declined from 71 worker years to a mere eight.

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 General Motors – that is, they set about replacing Ford's highly centralized organizational structure with the decentralized multidivisional form used at GM. As David Hounshell has shown, the GM people seemed to be succeeding in restructuring Ford [Hounshell, forthcoming]. 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, however, the decision was reversed. Although there are no records that reveal what actually happened at that subsequent meeting, 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 so as to transform Ford's organizational structure along GM lines, they seem to have backed away. Rather than copy GM, what Ford did instead was develop 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 fail to exploit the capacities they had built up over the years, the results can be momentous. The case of Sears offers an instructive lesson. As Daniel Raff and Peter Temin have argued, Sears faced two important turning points during its history – the first in the mid-1920s, and the second in the late 1970s [Raff and Temin, forthcoming]. 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 good will 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. 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 which increased their market share by using new information technology to lower prices and improve responsiveness to consumer demand.

Like firms, nations can make investments in specific ways of organizing economic activity – what we call institutions – that give them economic advantages over other nations. 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, as Freyer tells us, because cartels were illegal here) were responsible for the extraordinary performance of the U.S. economy [Chandler, 1977; Chandler, 1990]. But Leslie Hannah has shown that this view will not withstand empirical scrutiny. He 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 [Hannah, forthcoming]. Chandler assumed that managerial control of vertically integrated enterprises provided a coordination mechanism superior to any that could operate through the market, but this assumption has also been increasingly called into question. For example, Michael Enright has shown that small vertically disintegrated but geographically concentrated firms can develop coordination mechanisms that can be 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 and Philip Scranton [Piore and Sabel, 1984; Scranton, 1983; Scranton, 1989].

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 non-industrial 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 [Wright, forthcoming; David and Wright, 1992; Abramovitz and David, 1996]. 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. As these engineers organized themselves into national societies devoted to the promotion of their fields, they 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 has shown 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 information asymmetries.

Conclusion

The studies we have summarized here explore a wide variety of topics in business history, ranging from firms' efforts to improve their internal operations, to the ways in which organizations can mediate relations among individual enterprises, to the wealth of nations. Many of these studies, such as Levenstein's work on accounting at Dow, Genesove and Mullin's analysis of data collection by the Sugar Institute, and Raff and Temin's narrative of Sears's response to changing markets, are narrowly focused case studies. Yet, in sharp contrast to the case studies that characterized the Grasian tradition of business history, these works contribute to a coherent view of American economic development and organizational change.

The sources of this coherence are fundamentally different, however, from those that underpinned the Chandlerian synthesis. The latter had at its heart a deterministic view of technological change, a unidirectional model of organizational evolution, and a focus that excluded a broad range of topics from consideration. The coherence that underpins the diverse set of studies summarized here is of a very different type, for it derives less from a common set of answers than from a common set of questions. All of the authors take the imperfect nature of information as their starting point, and all can be seen as illuminating the ways in which this condition plays out in economic life. This common preoccupation then leads to a second source of coherence – the

recognition that there is a 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, and often result in the creation of capabilities, whether at the firm, organization, or economy-wide level, that have long-term salience.

Fruitful exploration of this underlying coherence 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 an exchange of ideas. As we have already suggested, business historians can turn to economic theory both for useful ideas 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.

It is important to be absolutely clear about the kind of interdisciplinary dialogue we are advocating here. We are not calling for a return to the hierarchical conception of scholarship that Gay attempted to impose on Gras during the 1930s – we do not see business historians as research assistants for economists who engage in a higher level of thinking. Although we hope that a byproduct of this dialogue will be better modeling by economists, our main concern is that the work of individual business historians redound to the credit of the field of business history as a whole. The real benefit of recent theoretical developments in economics is that they enable business historians to recognize the essential unity that underlies a great number of the problems with which they are concerned. As a result, studies on one topic can resonate with studies on others, strengthening them all and, in turn, the field as a whole.

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