Economic Theory and Business History

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Because work in business history has always consisted largely of studies of individual entrepreneurs, firms, and industries, scholars have continually had to struggle to prevent the field from disintegrating into antiquarianism. From the beginning economic theory has offered a solution—a way of distinguishing important trends from trivial changes and of building general understanding by comparing businesses and business people within and across industries and economies. However, practitioners have long resisted making serious use of economics in their work. Until recently they claimed that neoclassical theory's restrictive assumptions about human behavior and the workings of firms rendered it useless for probing the motives of entrepreneurs or the activities of complex enterprises. This excuse disappeared in the 1970s and 1980s, when economists began to develop new theories based on much more realistic ideas about human behavior and organizations. Some business historians immediately recognized the value of the new theory for their work, but most remained indifferent, skeptical, or even hostile.

The most common objection that business historians have posed to employing economic theory, even in its most recent forms, is that it founded on a method of analysis that is essentially static analysis and hence cannot account for the development of new business capabilities over time or, more generally, for innovation. This criticism is at best an exaggeration, but for the purposes of this essay we cheerfully acknowledge that there are important tasks for which economic theory, however construed, is not particularly well suited. As with any tool, however, strengths and weaknesses often come bundled together. Indeed, we would argue that aspects of economic theory that critics find terribly limiting can help scholars avoid a major pitfall of business history—its tendency toward Whiggishness. Business historians for the most part study successful enterprises. As a result, they are always in danger of jumping to the conclusion that the enterprises they have been researching are superior in some robust way—that they are the outcomes of historical processes that have produced higher forms of business organization. Judicious deployment of economic theory can help guard against this danger.

In the first section of this essay we explore the use of economic theory in business history during the era when Alfred Chandler's account of the rise of large firms dominated research in the field and neoclassical theory was in its ascendancy in economics. We then discuss the development of new bodies of economic theory and their growing use in business history as the appeal of the Chandlerian paradigm began to wane. We show that the receptivity of business historians in different parts of the world to the new theory was inversely correlated with the extent to which they had embraced Chandlers' ideas in the preceding period. In the U.S., and to a lesser extent Germany and Japan, many scholars continued to resist using economics in their work. In Britain, by contrast, where Chandler's arguments had long met with considerable skepticism, a number of business historians enthusiastically exploited the new theoretical developments. Even there, however, scholars tended to use economic theory descriptively rather than analytically. In the last section of the essay we argue that by making formal use of economic models to pose and test refutable hypotheses, business historians can both improve the analytical quality of their narratives and avoid Whiggism.

It is useful to begin with a few definitions. We restrict our attention in this paper to those bodies of theory taught in mainstream economics departments, including standard neoclassical theory, the economics of asymmetric information, transaction cost economics, and game theory. Left out are bodies of theory that are clearly economic in their content but that do not have the same standing in the economics profession. These range from Marxist theory, on the one hand, to the evolutionary economics of Richard Nelson and Sidney Winter, on the other (see Lazonick, this volume). When we discuss work in business history, we refer to the most part to scholars who identify themselves as members of this field. We take pains to note, however, that especially in the United States, a considerable amount of scholarship whose subject matter is recognizably business history has been written by economists and economic historians who do not identify themselves as members of the field. We argue that the work of these outsiders offers useful models that illustrate the benefits of making more rigorous use of economic theory in business history.

Business History and Economic Theory in the Era of Chandler

Almost from the moment that business history emerged as a distinct area of scholarship in the late 1920s, its relationship with economics has been fraught with tension.[1] The two founders of the field in the U.S., Edwin F. Gay and N. S. B. Gras, had a severe falling out by the early 1930s over their very different visions of the direction research should take. Gay, whose background was in economic history, believed that the goal of business history should be to develop a synthetic understanding of the

[1] For a more extended discussion of these early years, see Lamoreaux, Raff, and Temin 1997.

development of the economy. Precisely because businesses were subject to the discipline of the market, their experiences could provide insight into larger economic processes. Though Gras had been one of Gay's students, he had little use for such abstract theorizing. An inductive thinker, he believed that business behavior should be studied for its own sake and that new generalizations could only be developed after scholars had amassed a large body of case studies.

Although Gras had a significant number of followers, many business historians worried that the growing volume of studies of individual firms and industries was not adding up to significant scholarship. From time to time, as a result, there were attempts to reunite the fields of business and economic history. One of the most significant such efforts, a series of meetings that began at Northwestern University in the 1950s and involved members of both fields, led to the founding of the Business History Conference in 1971.[2] The rapprochement was only partially successful, however. Both fields were then going through major changes, and practitioners were being pulled in different directions.

In business history, scholars got a hint of the deliverance they had been waiting for when Alfred D. Chandler, Jr., published *Strategy and Structure* in 1962. The core of the study consisted of four long chapters devoted respectively to the histories of Du Pont, General Motors, Standard Oil of New Jersey, and Sears Roebuck and Company. Each chapter recounted a similar sequence of events: how a change in business strategy during the 1920s led the firm to diversify into new economic activities, causing serious organizational problems that managers were only able to solve by adopting a

[2] One of the original members of the group, Harold F. Williamson, worked throughout his career to bring business and economic history closer together. See Williamson 1966. See also Lamoreaux, Raff, and Temin 1997; and Hausman 2003.

multidivisional, decentralized managerial structure (the M-form). Chandler used the four case studies to lay out some basic principles of organizational innovation and then generalized his findings to the economy as a whole by tracking patterns in the M-form's subsequent spread. Pointing a way out of the Grasian morass of individual firm and industry studies, Chandler's work was enormously influential and inspired a wave of scholarship in the U.S. and elsewhere (see Galambos 1970; Kocha 1971; Nakagawa, ed. 1975; Hannah, ed. 1976).

Chandler's primary theoretical training had been in sociology rather than in economics (McCraw 1988). To extent that he read economics, his main influences were scholars such as Edith Penrose who worked outside the neoclassical mainstream. Penrose had noticed that more and more economic activity seemed to be occurring inside firms rather than in the market. In *The Theory of the Growth of the Firm* (1959), she explained this trend by positing that firms were more effective sites of learning than markets. When firms took on new economic activities, Penrose observed, their managers had to figure out how to coordinate them efficiently. These lessons could then be relatively easily and valuably applied to new activities, enabling firms to expand the scope of their businesses. Chandler's *Strategy and Structure* can be seen as an extended illustration of Penrose's ideas. For example, when Du Pont first diversified from explosives into paints, it had to learn how to manage these two very different businesses at the same time. Once it hit upon the solution of the M-form of organization, it was able to use its new structure to diversify profitably into a broader range of activities.

At about the same time as Chandler's *Strategy and Structure* was reshaping the practice of business history, a group of economists, variously dubbed the New Economic

Historians or Cliometricians, were taking the field of economic history by storm. These scholars advocated a more formally scientific type of scholarship that would make explicit use of economic models to formulate refutable hypotheses (North 1965; Fogel 1965 and 1967). They were strongly neoclassical in their methods, which tended toward comparative-static tests of partial equilibrium models. They were also strongly neoclassical in their belief that changes in technology and in the organization of production were for the most part endogenous responses to market signals. As one participant in this movement later acknowledged, there was not much room in this view of economic history for "good and bad entrepreneurs, leaders and followers"—that is, for what many business historians studied (Parker 1971; see also North 1961).

Robert Fogel's study of American railroads epitomized the New Economic Historians' approach. Challenging the Whiggish claim that railroads were "indispensable" to American economic development, he argued that transportation innovations were a response to economic need and that, if had been necessary, the canal system could have expanded to provide transportation services almost as efficiently as the railroad. He then proceeded to use historical data on freight shipments to evaluate this road not taken against the road that was. Employing comparative-static methods, Fogel calculated that in the absence of the railroad the Gross National Product (GNP) of the United States in 1890 would not have been much different—at most 5 percent lower—than it actually was (Fogel 1964).

In Chandler's view, Fogel's entire calculation was misguided (Yates 2004). The railroad's most important contribution to economic growth was the greater speed and regularity of transportation it provided compared to canals. As he explained in *The*

Visible Hand (1977), this improvement made possible the integration of mass distribution and mass production in large-scale, managerially directed enterprises, which in turn permitted the flow of resources and goods to be coordinated much more efficiently than was possible in the market. Underpinning this essentially Penrosian notion was Parsonian role theory. Chandler believed that managers' new positions of importance encouraged them to adopt professional identities and, unlike owners who were primarily interested in maximizing current profits, pursue policies that enhanced their companies' wealth creating capabilities over the long term.

Chandler's analysis of the rise and critical importance of the large managerially directed enterprise dominated research in business history in the United States at least through the 1980s (see Cassis, this volume). Given Chandler's orientation toward sociology and his aversion to the New Economic History, it is perhaps not surprising that few practitioners during this period made any use of economic theory in their work. Table 1 analyzes articles published in the *Business History Review* for a sample of years beginning in 1960. Virtually none of the articles surveyed before the 1990s exploited formal economic models or tested hypotheses quantitatively, and very few used economic concepts to frame the analysis. Even in the anomalous year 1975, when one issue included three pieces written by New Economic Historians, less than a third of the articles in the journal cited any economics literature at all. In most years, such references were few and far between.

Business and Economic History, the Business History Conference's annual proceedings journal, was somewhat more receptive to economic theory (see Table 2). A small number of economists had maintained their membership in the organization, presenting papers at its annual meetings that subsequently found their way into the journal's pages. Nonetheless, only a small proportion of the articles that it published were organized around economic concepts, and fewer still made any use of formal models. Even in this journal, the vast majority of articles included no references at all to the economics literature.

Chandler's synthesis helped business historians who were working in other countries move from isolated case studies to engagement with questions of broader concern. Although there has been disagreement over the extent to which his ideas applied to the experience of particular nations, his insistence on the importance for economic growth of the creation of large-scale, managerially directed firms provided scholars in many countries with an agenda around which they could organize international discussions and also their own research (see, for examples, Yamamura 1970; Daems and van der Wee, eds. 1974; Nakagawa, ed. 1975; Chandler and Daems, eds. 1980; Hannah 1983; Nakagawa and Yui, eds. 1983; Kobayashi and Morikawa, eds. 1986; Coleman 1987). Chandler's work was particularly influential in Germany and Japan, perhaps because scholarly attention in those countries had already focused on successful, large-scale enterprises (Kudô 2003; Schröter 2003). In neither country did business historians have much ongoing connection with economics, and the spread of the Chandlerian paradigm did little or nothing to change the situation.

In Germany, for example, business historians had mainly came out of political and social history, and they worked to make Chandler's paradigm less abstract—for example, by opening up the "black box" of management and examining the social origins, training, and methods of operation of the professionals who staffed company hierarchies, as well as the role played by large German banks in financing and directing major enterprises (see Kocka 1971, 1978, and 1981; Nakagawa and Yui, eds. 1983). They were also preoccupied—understandably given Germany's twentieth-century history—with the relationship between business and the state (see, for example, Horn and Kocka, eds. 1979). Richard Tilly, an economic historian who has written extensively on German banking, attempted to stimulate his German colleagues to write a more quantitative, theoretically informed type of business history, but he had little success (see, for example, Tilly 1982).

In 1981 the German Society for Business History established the *German Yearbook on Business History* to translate selected articles published in German periodicals into English in order to gain international recognition for the work (Pohl 1987). None of the articles that appeared in these volumes before the *Yearbook* ceased publication in 1995 use economic models, and only one (authored not surprisingly by Tilly [1993]) employed quantitative tests. One article (Lindenlaub 1984) surveyed different theoretical approaches to research in business history, including some using economic theory, but very few others cited any economics literature at all.

In Japan the situation was very similar. Keiichiro Nakagawa, the scholar behind the formation of the Business History Society of Japan in 1965, envisioned that the new association would foster dialogue between business and economic historians. In practice, however, participants adopted the methods and approaches of those U.S. business historians who had distanced themselves from economic history. Indeed, Chandler was considered one of "godfathers" of the Japanese branch of the discipline. The first issue of the *Japan Business History Review* in 1966, devoted to a discussion of methodology, focused mainly on the ideas of Chandler and Gras. Although the volume discussed Schumpeter's notions of entrepreneurship, as well as several other approaches, there was no exploration of mainstream theoretical developments in economics (Yamamura 1970). Nor has this orientation shifted over time. The overwhelming majority of the articles published in the *Japanese Yearbook on Business History* (modeled on the German *Yearbook* [Pohl 1987]) contain no references at all to the economics literature. Similarly, very few of the foreign scholars invited to the international conferences that Japanese business historians have hosted annually since 1974 have had any training in economics. Even when the topic of the conference was something about which economic theory clearly had a lot to say (for example, cartels), the organizers made no effort to secure the participation of economists (see, for example, Kudô and Hara, eds., 1992).

In Britain, however, business history began to diverge methodologically from that in the U.S., Germany, and Japan during this period of Chandlerian dominance. Chandler's approach was less appealing there because the giant managerial enterprises that he thought were the carriers of progress were not as important in Britain as in these other countries. Chandler viewed their relative absence as a cause of the U.K.'s poor performance (Chandler 1980 and 1990), an idea that William Lazonick (1983) among others developed further (see the essays in Elbaum and Lazonick 1986). Although this argument attracted some adherents among British business historians (see, for example, McKinlay 1992), most found it unpersuasive (Church 1990; Harvey and G. Jones 1990; Supple 1991; Kirby 1992). In response, they amassed evidence pointing to the vibrancy of smaller, less integrated businesses, including family run enterprises (G. Jones and Rose 1993; Church 1993; Jeremy, ed., 1993; Lloyd-Jones and Lewis 1994).[3]

Perhaps because they were less enamored of Chandler's ideas, a number of British business historians responded with interest to some of the research being produced by New Economic Historians in the United States. Arguing that one should not embrace so sweeping an explanation as entrepreneurial failure unless alternative hypotheses could be falsified, New Economic Historians had reexamined some of the standard examples of failure—for example, British textile firms' reluctance to adopt ring spinning—and were in fact unable to reject the hypothesis that British entrepreneurs had responded rationally to the economic conditions they faced (see Temin 1966; Sandberg 1969; McCloskey and Sandberg 1971; McCloskey 1973; Saxonhouse and Wright 1984). British scholars responded to this work by revisiting the histories of a number of declining industries, including textiles, iron and steel, and automobiles, in some cases developing a new appreciation of the choices British entrepreneurs made (see, for example, Harvey 1979; Foreman-Peck 1981; Fine 1990; Bowden 1991). As Table 3 indicates, British scholars' greater interest in and willingness to make use of the insights of the New Economic History is reflected in the higher proportion of articles published in *Business History*, the flagship journal of the British branch of the profession, that cited the economics literature compared to publications in the U.S., Germany, and Japan. Though none of the articles we surveyed for the period before 1990 went so far as to use

[3] Some scholars who initially were more positively inclined to Chandler's ideas later changed their minds. For example, Hannah organized a conference a conference in London in 1975 to explore the possibility of applying Chandler's insights to British business history. The papers were published in Hannah, ed. 1976. But contrast Hannah 1976 and 1983 with Hannah 1991 and 1999.

formal models, several did make an effort to test the merits of alternative hypotheses using quantitative data.

Business History and Economic Theory in the Post-Chandler Era

Traditional neoclassical theory assumed that economic actors were rational beings who made optimizing decisions on the basis of perfect information and foreknowledge.[4] This highly styled view of human behavior was a useful simplification that enabled economists to analyze certain otherwise intractable problems, especially concerning markets (Friedman 1953). But it was not so well suited for understanding the behavior of individual enterprises. Traditional theory treated the firm as a black box—as an equation-solving entity devoted to setting marginal revenue equal to marginal cost. It was not particularly helpful for analyzing how firms would respond strategically (that is, to small-numbers competition) in their product markets and was completely incapable of dealing with firms as complex organizations composed of people with differing experiences and goals. For these reasons its appeal to business historians was understandably limited.

Over the last three decades, however, several new bodies of analysis have emerged that are potentially much more useful to business historians. Agency theory, for example, provides a set of tools appropriate for analyzing organizations whose members have different and often conflicting interests. Take the case of the large-scale managerial enterprises that figure so importantly in Chandler’s histories. The owners of these enterprises (the principals) were the stockholders, who might, as in Chandler’s account

[4] The following discussion draws on Lamoreaux, Raff, and Temin 1995, 1997, and 1999.

and also in traditional theory, be conceptualized as seeking to maximize profits. But the managers (their agents) who actually ran the firms were likely to have very different priorities. Perhaps, as Chandler claims, they were interested in the long-run health of the enterprise, but they could also have other goals, such as maximizing their own compensation and perks, guaranteeing themselves job security, or establishing reputations that would enable them to secure more important positions in other firms. In order to ensure that firms operated in their interests, stockholders had to be able to impose their own priorities on managers. But even if they had the power to do so, which was not necessarily the case, they typically had only imperfect information about their agents' activities. Moreover, such information as they had was asymmetric—that is, stockholders knew a lot less about what managers were doing than did the managers themselves.

Because agency theory offers a well-articulated analysis of how principals can use incentives to solve these kinds of problems, it can help business historians evaluate the effect of specific rules and historical contexts on hierarchically structured relationships. We can see the rise of what we might call post-Chandler business history in the application of these new tools. For example, Daniel Raff (1995) has used the theory to show how the kinds of compensation schemes that were best suited to induce productive effort from workers in automobile factories changed with the kind of technology the firm employed. Plans that rewarded individual achievement made sense where artisanal modes of production prevailed. Firms that used assembly-line 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 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 centralized control of the pace of work. 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. As Raff has shown, other firms abandoned incentive plans in favor of straight-wage compensation schemes when they adopted Ford-style technology.

The top-down models of agency theory are less useful where economic actors interact in ways that affect each others' outcomes. Such situations can often be more fruitfully analyzed using the tools of game theory. Defining a game involves specifying the number of players, the order of play, the choices each player can make at each stage of the game, how each choice affects the other players' choices and payoffs, the information that each has, whether the players can cooperate or not, and the number of periods the game will continue,. Perhaps because the game has to be laid out in such detail, business historians have had trouble seeing the value of the exercise. But given adequate definition, game theory offers practitioners a set of tools that can be quite valuable for analyzing the choices of real-world decision makers. Particularly useful is the principal of backward induction, in which players anticipate what will happen at later stages of the game and make their initial choices accordingly. For example, Naomi Lamoreaux and Jean-Laurent Rosenthal use backward induction in a simple three-period game to analyze the decision to organize a business as a partnership or a corporation. Their model yields predictions about the choice of organizational form that are a better fit to the historical evidence than the conventional view that corporations are superior for most types of business enterprises.[5]

Backward induction also plays a role in a third major body of theory— the economics of transaction costs pioneered by Oliver Williamson.[6] Williamson's analysis departed from orthodox economic theory in placing the individual transaction rather than a market equilibrium of supply and demand at the center of the analysis and in rejecting the standard neoclassical assumption of perfect information. In his initial approach (1975), Williamson emphasized the idea that when economic actors enter into transactions, they know more about their own capabilities and circumstances than about those of the parties with whom they are dealing. As a result, they can often take advantage of one another—extract more benefit from the transaction than they could if all parties had the same information. Anticipating such exploitation, economic actors may refuse to enter into such activities unless they are able to find ways of reducing the potential for such ill-gotten gains.

In subsequent work, Williamson (1979, 1981, and 1985) articulated the types of circumstances in which transaction costs tended to be high and also suggested how they could be alleviated. For example, he argued that contracting was likely to be particularly difficult in situations characterized by "asset specificity"—that is, situations in which investments had to be made in assets that were highly specific to the transaction and could not be reployed at low cost. The best way to deal with this problem, Williamson

[5] See Lamoreaux and Rosenthal 2006b. For a simpler, more heuristic version of the model, see Lamoreaux and Rosenthal 2006a. Many games have multiple solutions. This theoretical complication may be a boon to business historians, providing an opportunity to explain divergent outcomes to similar problems.
[6] Williamson built on Ronald Coase's classic (1937) but long ignored exploration of the question why some activities were coordinated by the market and others were coordinated within firms. Coase had hypothesized that under certain circumstances internalizing transactions within firms was less expensive than organizing and enforcing them in the market.

suggested, was to move the activity inside the firm. Putting both parties to the transaction under common ownership removed the incentive for them to exploit each other. Moreover, any differences that subsequently arose between the parties could be resolved by fiat rather than by costly adjudication.

Williamson himself was one of the first scholars to apply his economic theory of transaction costs to business history. In an important article in 1981, he recast Chandler's account of the rise of big business. As he saw it, the large vertically integrated firm was able, by expanding its boundaries and substituting managerial (hierarchical) coordination for market exchange, to resolve the serious transaction problems caused by the asset specificity of its investments. Similarly, the integration of mass distribution and mass production was a consequence of what he called the externality principle, where the failure of one distributor to invest the resources needed to maintain the quality of a good could undermine the reputation of the product more generally.

A small number of other scholars recognized early on the value of transaction-cost theory for the study of business history. Two of the first were Mark Casson and Geoffrey Jones, a British economist and historian respectively, who were both interested in the study of multinational enterprises. Casson, himself a theorist, organized a conference in 1982 that brought together business historians, economists, and scholars in business schools to discuss how to write studies of international business that would be both theoretically and historically informed (Casson, ed. 1983). Jones and Peter Hertner organized a conference the next year with a similar agenda (Hertner and Jones, ed. 1986). Both Casson and Jones used the new economic theory in their own scholarship, and they continued to work hard to encourage other business historians to do likewise, organizing conferences and publishing collections of essays (Casson 1982; Casson 1984; Buckley and Casson, eds. 1985; Casson 1986, and 1987; Jones, ed. 1986 and 1990; Jones and Morgan, eds. 1994; Jones 2000 and 2005).

Others soon joined Casson and Jones in their call for more theoretically informed work in business history. Australian academics Diane Hutchinson and Stephen Nicholas urged practitioners to use insights from the economics of transaction costs to extract new generalizations from the histories of individual businesses and to improve their understanding of why business organizations evolved in different ways in different countries (Hutchinson and Nicholas 1987 and 1988; Nicholas 1986). The British scholar Clive Lee (1990a and 1990b) published a two-part article in *Business History* in which he similarly argued that recent theoretical developments in the economics of information and organizational design could help scholars extract more powerful generalizations from their case studies. In the United States Naomi Lamoreaux, Daniel Raff, and Peter Temin organized a series of conferences under the auspices of the National Bureau of Economic Research that brought together business historians, economic historians, and economic theorists with the aim of encouraging both more intellectual exchange among these groups and more use of economic theory in business history (see Temin 1991; Lamoreaux and Raff 1995; and Lamoreaux, Raff, and Temin 1999).

The response of business historians to these calls has varied around the world. Perhaps not surprisingly, given Casson and Jones's leadership, the British branch of the profession has been most receptive. As Table 3 shows, the proportion of articles published in *Business History* that included citations to the economics literature increased beginning in the mid-1970s and then soared by the 1990s. The same pattern appears, albeit to a lesser extent, in the share of articles that used economic concepts to frame the analysis. No one would mistake *Business History* for an economics journal—explicit economic models and hypothesis testing were still the exception rather than the rule—but the shift in the content of the journal is nonetheless striking.

The change in the type of articles published by *Business History* had much to do with the installation of a new editorial team, one of whose members was Jones, but the rapidity of the transformation suggests that research in the field had already moved substantially in the direction of using more economic theory. This change was encouraged by the location of many British business historians in economic history or even economics departments. Since 1990, however, many economic history departments have been merged into history departments, which may alter future research styles.

This is not to say that the majority of business historians in Britain were part of this movement or that there was no opposition to the trend. The annual surveys of the literature that the journal began to publish in the early 1990s suggest that most scholars continued to work on individual industries and firms in the same way as they had before. Some of the surveys, moreover, were very critical of work by economists. Alan McKinlay (1992, 4), for example, wrote dismissively of "the static sterility of the 'new' economic history." But other writers of the surveys lauded the turn toward economics. Indeed, Martin Chick (1993, 6) went so far as to opine, "Were there an Oscar-style award for 'Journal of the Year' my nomination for 1991 would be *Explorations in Economic History*," which was devoted to publishing research by New Economic Historians.

British scholars have used economic theory to explore topics ranging from ancient Phoenicia (Moore and Lewis 2000) to modern finance (Ross 1996), but their work has focused especially in two areas: multinational enterprises and business networks. British business historians participated vigorously, for example, in a debate stimulated by the economist Jean-François Hennart (1994a) who used data on so-called free-standing companies to critique the reigning view (which Casson had done much to promote) that multinational enterprises were devices for the international transfer of firm-specific capabilities. Casson issued a rejoinder, as did T. A. B. Corley, and Hennart responded (Casson 1994; Corley 1994; Hennart 1994b). The debate quickly attracted new participants, ultimately giving rise to an edited volume (Wilkins and Schröter, eds. 1998) and additional books and articles (for example, Greenhill 1995; C. Jones 1997; G. Jones 2000). Although disagreement persists, what is important for our purposes is the extent to which the debate has focused around the question of what is the most appropriate economic theory for understanding this form of business enterprise.

Initially, the focus on business networks was part of the effort to rescue family enterprises from the Chandlerian scrapheap (see in particular the special issue of *Business History* on "Family Capitalism" in October 1993), but it quickly grew into a broader attempt to understand the dynamics of important regional economies (for example, Casson and Rose, ed. 1998), the mechanisms by which capital has been funneled into business enterprises (for example, Cox, Biao, and Metcalfe 2003), and the ways in which economic activity is coordinated over long distances (for example, Munro and Slaven 2001). As in the debate over the free-standing company, much of the discussion has revolved around the utility of particular types of theory, including evolutionary and sociological approaches as well as various branches of economic theory (Casson 1997; Westall 1998; Toms and Wilson 2003; Parsons and Rose 2004). Entrepreneurship also has been revived as an issue using the new tools of economics and providing explicit tests of hypotheses (Godley 2001; Nicholas 1999 and 2004).

Scholars in Australia also played a leading role in showing how economic theory could be applied to business history. Stephen Nicholas followed up his call for this kind of work with studies of multinationals (1983 and 1986), British overseas marketing (1984), and early trading companies (Carlos and Nicholas 1990, 1993, and 1996). Simon Ville joined the debate over early trading companies (Jones and Ville 1996). He also brought the new theory to bear on the agricultural sector, analyzing networks of trade and finance in the pastoral sector (Ville 1996; Ville 2000; Ville and Fleming 2000). Perhaps the most sophisticated applications have been Gordon Boyce's analyses of financial and information networks in the shipping industry (Boyce 1992, 1995, and 2003; Boyce and Lepper 2002). Boyce (2001a) expanded his analysis to the study of cooperative ventures in international business more generally, and he and Ville (2002) wrote a business history textbook that aimed to demonstrate the power of what they called "the contracting paradigm."

During the 1990s, as giant managerial enterprises (especially in the U.S.) gave way to more specialized, less vertically integrated competitors, the Chandlerian paradigm lost much of its luster and the new style of economically informed business history attracted increasing attention around the world. A few business historians from Germany and Japan joined this movement (see, for example, Burhop 2004 and Okazaki 1995), but the new style of business history has developed furthest in places that did not already have a business-history establishment founded on the Chandlerian paradigm. Since 1990, for example, *Business History* has published increasing numbers of articles by Dutch and

Scandinavian scholars who have made use of insights from the new economics of information. Notable examples include Hugo van Driel's study (2003) of the international coffee trade, Anita Goransson's analysis (1993) of the impact of gendered property rights on the separation of ownership and control in Swedish business, and Hans Sjögren and Sven Jungerhem's discussion (1996) of the venture capital market for small firms in Sweden. Similar publications by Greek, Italian, Spanish, Portuguese, and Eastern European scholars have also found their way into the journal (see, for examples, Minoglou 2002; Cuevas 2002; Pérez and Puig 2004; and Stanciu 2000.) The several proceedings volumes of papers given at conferences of the newly formed European Business History Association reveal a similar geography. The volumes vary enormously in the extent to which they include papers that reference the economics literature, with Bonin (2002) on the low end and Olsson (1997) on the high end. In the middle (Amatori, Colli, and Crepas 1999; Kuijlaars, Prudon, and Visser 2000; Amdam, Hagberg, and Sogner 2001) about 20 to 25 percent of the articles cite at least some economics. Virtually all of these are by British, Dutch or Scandinavian business historians or by scholars from Mediterranean countries.

Scholars in the U.S. who made use of the new economic theory to tackle problems in business history tended mainly to be trained in economics, to specialize in economic history or one of the other subfields of economics, such as industrial organization, and to publish in the *Journal of Economic History*, the *Rand Journal of Economics*, the *Journal of Law and Economics*, or other similar venues. Many of them belonged to the Business History Conference, but they typically taught in economics departments. To give just a few examples, Ann Carlos and her various collaborators have used all of the bodies of theory discussed above to analyze early trading companies (see, for example, Carlos 1992; Carlos and Hoffman 1986; Carlos and Nicholas 1990, 1993, and 1996; Carlos and Lewis 1993 and 1999). Charles Calomiris (see Calomiris 1990 and 1995; Calomiris and Gorton 1991), Naomi Lamoreaux (1994), Kenneth Snowden (1995), and Noel Maurer (2002) have employed agency theory to explore the workings of credit markets and the banking system more generally. Avner Greif used game theory with great subtlety to explore the institutions that facilitated trade in the early modern period (the work is synthesized in Greif 2005). Chiaki Moriguchi (2003 and 2005) developed a game-theoretic explanation of what happened when welfare capitalism encountered the shock of the Great Depression, and Knick Harley (1982) and David Genesove and Wallace Mullin (1998, 1999, and 2001) have used ideas from game theory to explore strategic behavior in the railroad and sugar refining industries respectively. The case of General Motors' acquisition of the Fisher Body has been examined and reexamined from the perspective of transaction-cost economics (see Klein, Crawford and Alchian 1978; Coase 2000; Klein 2000; Freeland 2000).[7] Concepts from this literature have also been applied to topics as diverse the study of the Cuban sugar industry (Dye 1994 and 1998), the integration of regional and interregional capital markets in the U.S. (Odell 1989), and the growth of sharecropping in early modern French agriculture (Hoffman 1984).

Very few scholars in the U.S. who identified themselves primarily as business historians exploited the new theoretical developments in economics. Spurred by the work of Michael Piore, Charles Sabel, Jonthan Zeitlin, and Philip Scranton (see Piore and Sabel 1984; Sabel and Zeitlin 1985 and 1997; Scranton 1983, 1989, and 1997), business

[7] For an empirical challenge to the transaction cost view of the acquisition, see Helper, MacDuffie, and Sabel 2000.

historians have willingly abandoned their focus on the large-scale enterprise. But they have not embraced economics as a tool in their explorations. As Table 2 shows, the decline of the Chandlerian paradigm resulted in a slight increase in the number of articles in *Business and Economic History* that made use of formal economic models and/or tested hypotheses quantitatively, but there was no similar rise in the *Business History Review*. As late as the 1990s, moreover, less than 20 percent of the articles sampled in the two journals included even one citation to the economics literature. The founding by the BHC of a new quarterly, *Enterprise and Society*, did not change the situation. A review of the journal's contents over its first half decade of publication reveals only a few articles that involved economic analysis in a central way (Bakker 2001, Beamer and Lewis 2003, and Boyce 2001b), most of them by scholars from other countries. Rather, there seems to have been a shift toward a type of cultural history that is relatively uninformed by any theory, let alone economics (see, for examples, the symposia on "Beauty and Business" [2000] and on "Gender and Business History" [2001]).[8]

Toward More and Better Economics in Business History

To the extent that business historians have made use of the new economic theory in their work, they have for the most part employed it descriptively. As the tables show, they have rarely set up formal models or engaged in explicit hypothesis testing. Rather, they have tended to use economics in the same way they have used other types of theory—as a source of ideas and a framework around which to organize their research.

[8] The main editor during the early years of the journal was an economist (William Hausman), so the trend was more likely a result of the kind of work then being done by U.S. business historians than the preferences of the editor. We do not mean to imply that no business historians write theoretically informed cultural history—just that much current writing is atheoretical. For a counter example, see Lipartito and Sicilia (2004).

They have not been swept up in any social science project of generalization, but have subordinated the theory to what business historians typically do, that is, try to understand the behavior and evolution of specific enterprises, industries, or business practices.

Nonetheless, opposition has arisen even to this modest deployment of economic ideas. Economics, it is objected, is very good at modeling the measures according to which business people decide among alternative courses of action, whether these be price signals in the market, the extent to which incentives can alleviate principal-agent problems, or the effect of different organizational arrangements on transaction costs. But it is not very good at shedding light on where these alternatives come from in the first place. That is, it is not much good at explaining how individuals, firms, and organizations innovate and develop new capabilities over time.

Chandler himself played a role in articulating these criticisms. Although initially he seemed flattered by Williamson's effort to translate his ideas into the language of transaction costs,[9] eventually he came to the conclusion that the framework was too limiting. From his Penrosian perspective, it was "the specific nature of the firm's facilities and skills"—not its ability to minimize transaction costs—that mattered. For it was these organizational capabilities that "permit[ted] the enterprise to be more than the sum of its parts," that gave "it a life of its own," that made it a site of learning (Chandler 1992, 86-7). Other business historians agreed. S. R. H. Jones (1998) emphasized the limitations of the Williamson's comparative static approach for analyzing the development of business capabilities over time. William Lazonick (2003) doubted that the mainstream economics profession could ever develop theoretical tools appropriate to

[9] For example, he and Herman Daems included an essay by Williamson in their volume *Managerial Hierarchies* (1980).

this task. In his view, it would "require an intellectual revolution in economics" that was extremely unlikely to occur. Kenneth Lipartito and David Sicilia (2004) argued that the study of change and development "could never quite be brought under the abstract, generalized models of organization proposed by economic theory."[10]

There is no doubt that these critics are to some extent correct. Mainstream economic theory is not well suited for understanding the sources of innovation or the wellsprings of economic growth. But that does not mean it lacks utility even for these purposes, as the following example drawn from Chandler's own work shows. The use of simple economic models can improve scholars' ability to audit claims about dynamic processes, helping them decide what story they ought to tell.

The Standard Oil Company, according to Chandler, was one of the earliest companies to demonstrate the enormous cost savings that could be reaped by substituting the visible hand of management for the invisible hand of the market. Chandler's account of Standard's rise is a story of efficiency begetting market power begetting efficiency. Frustrated by the failure of their efforts to organize the nation's oil refiners into a cartel, Rockefeller and his associates in the Cleveland refinery decided to obtain the cooperation of their rivals "by relying on the economic power provided by their high-volume, low-cost operation." They approached the manager of one of the two railroads that served Cleveland and asked for a rebate in return for an assured volume of business. The manager agreed because "such high volume meant he could schedule the use of his equipment much more efficiently and so lose nothing by the reduced rate." Standard then

[10] Many of these scholars have been attracted to the evolutionary economics of Richard Nelson and Sidney Winter (1982). Other attempts to develop a theory suitable for analyzing the development of capabilities include Langlois and Robertson (1995) and Casson (1998). See also Langlois (2003 and 2004) and Lazonick, this volume.

used the advantage it derived from these lower costs to cartelize the industry, "invit[ing] the leading refiners first in Cleveland and later in other refining centers to join" in the benefits (Chandler 1977, 321-22).

Elizabeth Granitz and Benjamin Klein (1996) have reexamined Chandler's sources using conventional neoclassical tools and found that his account lacks plausibility. Although Standard operated "the largest refinery in the nation," there is no evidence that it had a significant cost advantage.[11] Its share of the U.S. market was only 4 percent, and its share of Cleveland production only about 15 percent. There were no barriers to entry arising from patents or the control of raw material resources. Nor do there seem at that time to have been any significant economies of scale in refining. The minimum efficient scale of operation for a still was only about 500-600 barrels, and most of the industry's growth during this early period came from new entrants (Williamson and Daum 1959, 252-308, 343). For all practical purposes, the petroleum industry was competitively structured. Refineries had U-shaped average cost curves, and bigger did not necessarily mean better.

Given this background, it is unlikely that the railroad's willingness to offer rebates derived from Standard's economic advantages and more plausible that it derived from the dynamics of competition among the small number of railroads that served the major refining regions. In the alternative account offered by Granitz and Klein, it was the railroads' (not Standard's) frustration over failed attempts to form a cartel—a persistent problem for railroads in the period—that set the events of the story in motion. Needing outside parties to police their agreement, the railroads formed the leading refineries in

[11] Indeed, Williamson and Daum's account of advances in refinery operation in the late 1860s and early 1870s focuses on other firms besides Standard, particularly Charles Pratt's "greatly admired refinery" in New York City (1959, pp. 252-86).

Cleveland, Pittsburgh, and other production centers into an association called the South Improvement Company. The Company allocated each railroad a share of the business of transporting oil and assigned participating refineries the task of ensuring that the railroads did not cheat on one another. In return, the refineries obtained both a rebate on their own shipments of oil and a drawback on their competitors' shipments.

Although the South Improvement agreement was never implemented,[12] there was a several month period (after the company was formed but before it fell apart) when prospects seemed dim for the refineries not included in the scheme. Rockefeller entrepreneurially took advantage of the situation to induce the other firms to sell out. As Granitz and Klein point out, only the competitive advantage that the member refineries stood to gain can explain why so many non-members sold their refineries to Rockefeller during these months, many of them at distress prices.[13] Emerging from this episode with effective control over the Cleveland refining industry, Standard then secretly merged with the original participating refiners in the other production centers. As a result of these acquisitions and mergers, Standard was large enough in and of itself to police the railroads' cartel agreements, and they willingly rewarded it for performing this service with the rebates it needed to maintain this position (Granitz and Klein 1996).

Although a full account of this incident would require us to venture into the realm of game theory in order to explain why the railroads needed Standard to monitor their cartels, Granitz and Klein's analysis shows how valuable even the simplest economic

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[12] The plan collapsed in the face of determined opposition from producers in the oil fields who threatened to enforce an embargo on shipments to the South Improvement Company with violence (Granitz and Klein 1996).
[13] Under normal conditions, given the petroleum industry's cost structure, refiners would not have been worried by the formation of a cartel for as outsiders they would have been able to "free ride" on Standard's high prices (Granitz and Klein 1996).

theory can be in sorting out the causal elements of an unfolding set of events. Instead of Chandler's simple account of efficiency begetting market power, we end up with a much more realistic story of how Rockefeller exploited the railroads' need for a monitor to "raise rivals costs" and secure monopoly control over his industry. It is important to underscore, however, that there is nothing in this story that precludes the possibility that monopoly ultimately begot efficiency. It may well be, as Chandler claims, that once Standard secured control of the petroleum industry, Rockefeller used the device of the trust company to reorganize its operations, consolidate production in its most efficient plants, and reduce costs through improved administrative coordination (Chandler 1977 and 1990). But it should also be noted that this part of the narrative has not yet been subjected to similar theoretical scrutiny.

As the Standard Oil example suggests, a judicious use of economic theory not only helps business historians sort out the causal elements of their stories but enables them to avoid a key pitfall to which Chandler, Lazonick, and others who favor a more dynamic approach often succumb: a tendency to write history in the Whig style and portray the achievements of businesses as if they were the end point toward which history has triumphantly been tending.[14] This tendency has not only inflected their histories of individual firms like Standard Oil but has shaped their views of the evolution of business organizations more generally. Hence for Chandler and Lazonick, the managerial hierarchies that large firms created in the early twentieth century were much more than a response to the economic environment of the times, they were a critical stage in the evolution of business—a necessary precondition for ongoing organizational learning and even for innovation itself (Chandler 1992; Lazonick 1991 and 2003).

[14] On this point, see also Hannah 2006.

In our own work (see Lamoreaux, Raff, and Temin 2003 and 2004), we have tried to show how recent economic theory could be used to avoid such a tendency toward Whiggism.[15] As we have argued, it is both easy and tempting to write business history as if what happened had to happen. It is much more difficult to write history that focuses on alternatives and choices, acknowledging the significance of possibilities that never took lasting form. But mainstream economics provides a broad arsenal of tools well suited for this purpose. Ranging from the evaluation of counterfactual hypotheses as used by neoclassically oriented New Economic Historians to the techniques of backward induction employed by game theorists, the methods of modern economics make it possible to consider (and even collect data about) roads that were never taken or alternatives that were never realized. To gain these benefits, however, even those business historians who have been most receptive to economic ideas will have to make fuller use of them. They will have push beyond the descriptive tasks for which they have hitherto mainly deployed the theory and exploit the greater power that comes from explicitly considering formal models and confronting refutable hypotheses with evidence.

Conclusion

This survey has emphasized the uneasy and sometimes tension-filled relationship that has long existed between business historians and mainstream economic theorists. Since business history first emerged as a distinct field of research in the early twentieth century, most practitioners have made little use of economics in their work and some have been downright hostile to the idea that economic theory could improve the writing

[15] For criticisms of our arguments, see Langlois (2004) and Sabel and Zeitlin (2004).

of business history. The development of new bodies of theory in the last several decades, particularly the economics of asymmetric information, transaction cost economics, and game theory, has changed the situation to a considerable extent. Although most business historians in the United States have remained indifferent or even antagonistic, there was a groundswell of interest in the new theory in Britain, Australia (and then increasingly in a number of continental European countries) beginning in the 1980s. There was also a flurry of activity in the United States and elsewhere by scholars who do not identify themselves as business historians but who have nevertheless been using the new theory to write what is in effect business history.

It is fitting, therefore, to conclude this essay by emphasizing the positive. Over the last couple of decades, scholars have used economic theory to forge new links between business history and developments in the other social science disciplines. They have revisited in illuminating ways topics ranging from early trading companies to modern financial institutions. They have engaged in lively debate about the sources of advantage held by multi-national companies, and they have challenged the idea that the family firm is an outmoded form of enterprise. They have also pioneered in the analysis of networks and other economic organizations that fall between the traditional topics of markets, on the one hand, and firms, on the other. In all of these endeavors, they have pushed outwards the frontiers of research in business history.

There is always more to be done, however, and we hope that this essay will encourage scholars to experiment with new types of theory (like game theory) that have been relatively under utilized for the study of business history. We also hope that our colleagues will be inspired to use theory more formally—that they will make the effort to pose and test refutable hypotheses, not as an end in itself but as a way of enhancing the rigor of their writing and of avoiding the temptations of determinism.

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TABLE 1

USE OF ECONOMIC THEORY AND METHODS IN ARTICLES PUBLISHED IN

BUSINESS HISTORY REVIEW

Year	Number of Articles	Percent that Cite Economics Literature	Percent that Use Economic Concepts to Frame the Analysis	Percent that Make Use of Formal Economic Models	Percent that Use Quantitative Information to Test Explicit Hypotheses
1960	20	0.0	0.0	0.0	0.0
1965	25	0.0	0.0	0.0	0.0
1970	21	4.8	4.8	0.0	0.0
1975	16	31.2	25.0	6.2	12.5
1980	14	0.0	7.1	0.0	0.0
1985	15	6.7	6.7	0.0	0.0
1990	15	6.7	6.7	0.0	0.0
1995	11	18.2	0.0	0.0	0.0
2000	12	16.7	16.7	0.0	0.0

TABLE 2

USE OF ECONOMIC THEORY AND METHODS IN ARTICLES PUBLISHED IN

BUSINESS AND ECONOMIC HISTORY

Year	Number of Articles	Percent that Cite Economics Literature	Percent that Use Economic Concepts to Frame the Analysis	Percent that Make Use of Formal Economic Models	Percent that Use Quantitative Information to Test Explicit Hypotheses
1965	12	16.7	16.7	0.0	0.0
1969	15	6.7	6.7	0.0	0.0
1975	9	22.0	0.0	0.0	0.0
1980	17	17.6	11.7	0.0	0.0
1985	16	6.2	12.5	12.5	12.5
1990	28	28.6	17.9	3.6	3.6
1995	35	8.6	11.4	0.0	2.9

NB: There were no issues in 1960 or in either adjacent year. The nearest year to 1970 was 1969. The early publishing history is given on *Proceedings of the Business History Conference* (Second Series, Volume 1) (Bloomington: Indiana University School of Business Division of Research, 1973), first [unnumbered] page. The journal published its final issue in 1999 and the organization commenced a new one, *Enterprise and Society*, in the following year. See the text for further discussion.

TABLE 3

USE OF ECONOMIC THEORY AND METHODS IN ARTICLES PUBLISHED IN *BUSINESS HISTORY*

Year	Number of Articles	Percent that Cite Economics Literature	Percent that Use Economic Concepts to Frame the Analysis	Percent that Make Use of Formal Economic Models	Percent that Use Quantitative Information to Test Explicit Hypotheses
1960	7	0.0	0.0	0.0	0.0
1965	6	16.7	16.7	0.0	0.0
1970	7	14.3	14.3	0.0	0.0
1975	8	37.5	12.5	0.0	12.5
1980	11	27.3	0.0	0.0	0.0
1985	15	20.0	0.0	0.0	13.3
1990	25	72.0	32.0	12.0	12.0
1995	23	47.8	26.0	4.3	8.7
2000	26	73.1	34.6	0.0	19.2