This PDF is a selection from an out-of-print volume from the National Bureau of Economic Research

Volume Title: Coordination and Information: Historical Perspectives on the Organization of Enterprise

Volume Author/Editor: Naomi R. Lamoreaux and Daniel M.G. Raff, Editors

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-46820-8

Volume URL: http://www.nber.org/books/lamo95-1

Conference Date: October 23-24, 1992

Publication Date: January 1995

Chapter Title: Introduction: History and Theory in Search of One Another

Chapter Author: Naomi R. Lamoreaux, Daniel Raff

Chapter URL: http://www.nber.org/chapters/c8745

Chapter pages in book: (p. 1 - 10)

Introduction: History and Theory in Search of One Another

Naomi R. Lamoreaux and Daniel M. G. Raff

This book continues a project we and our colleague Peter Temin began with *Inside the Business Enterprise,* published in 1991. The title of that volume implicitly posed a question: What actually goes on inside firms? Both common sense and a rudimentary formal education in economics suggest the simple answer "entrepreneurs combine capital, labor, and raw materials." We argued that one cannot understand firms' internal structure and dynamics—and for that matter, firms' competitive dynamics—unless one also studies how firms handle information.

The various articles in *Inside the Business Enterprise* analyzed the ways in which firms collect information about production processes and customers and the problems that their (always imperfect) methods of data collection pose. The focus was the late nineteenth and early twentieth centuries, when firms of great size and organizational complexity first emerged in significant numbers. Daniel Raff and Peter Temin set the stage by providing an overview of the relevant economic theory on imperfect information and the internal organization of firms. The rest of the essays explored specific examples of how firms coped with information problems. Margaret Levenstein traced the development of cost-accounting techniques at a pioneering chemical giant, JoAnne Yates described the methods several large manufacturing firms adopted to handle their increased flow of information, and Naomi Lamoreaux showed how information problems shaped banks' lending policies. Bradford De Long

Naomi R. Lamoreaux is professor of history at Brown University and a research associate of the National Bureau of Economic Research. Daniel M. G. Raff is associate professor of management at the Wharton School and a faculty research fellow of the National Bureau of Economic Research.

The authors thank their collaborator in this series, Peter Temin, and, equally, John Sutton, Michael Enright, and Avner Grief for helpful conversations. Geoffrey Carliner and Martin Feldstein continued to give encouragement and support. The usual disclaimer applies.

argued that monitoring by financiers at J. P. Morgan improved the performance of the turn-of-the-century consolidations Morgan financed, whereas Thomas Johnson claimed that senior managers' misinterpretation and misuse of financial accounting information has in recent years generated serious inefficiencies in physical production and even product-line planning in large-scale enterprises.

The current volume extends the focus of the first by exploring ways in which firms coordinate economic activity in the face of asymmetric information—that is, information not equally available to all parties. In any economic activity more complex than Robinson Crusoe surviving alone on the beach, coordination is a vitally important task. The first fundamental theorem of welfare economics tells us that in an ideal world of complete information and perfect competition, the market provides the optimal coordination mechanism.[1] In our imperfect world of asymmetric information, however, the market does not always perform as well. Some activities are better coordinated within firms or other complex organizations, while other activities are better coordinated by firms cooperating among themselves. The problem is to determine the circumstances under which each form of coordination is likely to be superior.

We think of this second set of inquiries as digging more deeply rather than more broadly than those of the first volume. The first in effect presumed the firm as an institution, taking as given its organizational tasks, boundaries, priorities, and menu of control mechanisms. Here we try to question these explicitly. In so doing, however, we continue the approach of the first volume, exploring our issues and developing our themes in concrete institutional and historical detail. There are good reasons to proceed in such a manner. The abstract accounts favored by economists leave out much of the activity that goes on inside firms, and scholars studying how the economy works should not be ignorant in this area. More important, much of this activity is interesting when viewed from an economic perspective—that is, it has the potential to stimulate insights and productive theoretical questions.

Like its predecessor, the National Bureau of Economic Research conference that yielded this volume brought together both economists and business historians. Our motive in running the conference in this way (and equally in publishing the revised articles) is a feeling that the time is ripe for an exchange of ideas between economists and business historians. In recent years, economists have begun to move beyond black-box conceptions of the firm and to develop an elaborate body of theory that could in principle enhance work in business history. Business history has begun a complementary transit, moving beyond the idiosyncratic studies of individual firms and industries that long characterized the field. Seen from a distance, it seems clear that history and theory are now in search of one another. Our goal is to help the process along by encouraging economists and business historians to see one another as a natural audi-

1. For a lucid treatment, see Koopmans 1957.

ence and so, we hope, to stimulate each other to write better and more useful theory and history.

The Coordination of Economic Activity

The business historian most responsible for making this dialogue possible is, of course, Alfred D. Chandler, Jr. (see especially 1962, 1977, 1990). Chandler's key innovation was to place the problem of coordination squarely at the center of the study of business history. His focus was on why the economy came to be dominated by large firms. His method was to survey individual case studies and to analyze the traits of large panels of surviving firms. Chandler argued that managerial coordination was superior to coordination by the market wherever industries were characterized by "economies of speed"—that is, wherever consistently faster throughput reduced unit costs. In such industries, vertical integration (forward as well as backward) enabled firms to avoid supply bottlenecks and dispose smoothly of output if they could coordinate their various units effectively. Investment in the organizational capacity needed to manage vertically linked enterprises was thus the key to competitive success. Firms that did not make such investments lost ground. Those that made them first tended to establish very durable positions of market leadership.

At about the same time as Chandler was developing this argument, Oliver Williamson was beginning work on the transactions-cost view of the firm. Following Ronald Coase's classic but long-ignored work of the 1930s, Williamson asked why some activities were coordinated within firms and why some were coordinated by the market (Coase 1937). Coase had hypothesized that under certain circumstances internalizing transactions within firms reduced the cost of organizing and enforcing them through the market, and Williamson attempted to give this idea more concrete expression. For example, he proposed that firms emerged to manage investments with a great deal of "asset specificity," because otherwise the sunk character of the investments exposed their owners to threats that their profits might be expropriated (Williamson 1975, 1985).

Chandler's account of the evolution of large firms stimulated Williamson to recast it in terms of his own theoretical framework, and the subsequent development of his ideas clearly derived from and, equally important, influenced in turn those of Chandler (Williamson 1981, 1985; Chandler 1990). This was the first fruit of the newly possible collaboration between business history and economic theory. There are reasons to believe, moreover, that the harvests will only grow richer. In the first place, recent developments in economic theory will enable scholars to probe more deeply the coordination problems that Chandler highlighted. Moreover, many related issues remain largely unexplored.

Chandler focused attention on large firms' dependence on managerial coordination, but he actually had surprisingly little to say about how this coordination was achieved throughout the organization. Recent research in economics on principal-agent problems, however, has raised explicitly the question of how managerial hierarchies can be most efficiently structured and run.[2] At the heart of this literature are the observations that managers may have interests that differ from those of the owners of the firm, and that the latter may have only a limited ability to check up on what the former are doing. In theory, stockholders can invest in various monitoring mechanisms. They can also structure compensation systems to create incentives for managers to operate in their interests. The important questions, then, are which combinations of monitoring and incentive systems will function best in specific situations, and what the costs to the economy of relying on managerial coordination within very large firms are likely to be. The leveraged buyout movement of the 1980s certainly raised the possibility that such coordination has potentially substantial costs, and hence the question whether other forms of coordination might actually be superior.[3] There is also a growing body of literature arguing that certain solutions to the principal-agent problem commonly adopted by American firms have had the undesirable consequence of reducing flexibility and inhibiting innovation (see, e.g., Piore and Sabel 1984; Abernathy 1978).

Principal-agent analysis can be extended beyond the problem of stockholders' versus managers' interests to interactions among managers of different ranks and indeed to all employment relations within the firm, a subject to which Chandler devoted little attention despite the great deal of managerial time and energy it absorbs. The subject is important because it links up with larger concerns such as labor relations and the evolution of control systems within firms. Once again, recent history—in this case the competitive challenge posed by new industrial powers like Japan—has raised the possibility that the solutions adopted by large American firms have been less than optimal (Lazonick 1990, 1991).

The principal-agent approach is equally useful in considering firms' relations with those who supply them with capital, another topic given little attention in Chandler's books. Despite the financial reporting requirements mandated for public companies by the Securities and Exchange Commission Act of 1934, arm's-length suppliers of capital know vastly less about the operations and prospects of companies who are seeking their money than do the managers of the companies themselves. This asymmetry naturally affects the contractual terms on which investors are prepared to part with their funds and therefore the structure of the firms obtaining finance. It also has real consequences for the population of firms and ipso facto for the population of organizational institutions, particularly as firms' need for external finance varies over the

2. For a summary of much of this literature, see Milgrom and Roberts 1992.

3. For a polemical but rigorous analysis, see Jensen 1993. For a more colorful treatment, see Burroughs and Helyar 1990.

business cycle (Calomiris and Hubbard 1993). Firms whose organizations and affairs appear less transparent (and so less trust-inspiring) to banks and investors are more likely to be winnowed out during downswings.

Principal-agent theory has its critics, or course. The standard complaint lodged by historians is that it pays no attention to time or context. In part, this criticism is simply a confused reaction to the role of simplifying assumptions in economic theory: although the earliest models were very stripped-down indeed, this was largely a matter of expository style and not necessarily a feature of the insights they generated. Moreover, the historian critics should take heart from the way the economists' literature developed thereafter. Game-theoretic methods quickly gained ascendency, and that brought a new specificity to the analysis. Precisely what each actor knew at the moment of decision mattered. What each could credibly communicate and commit to mattered too. So did who moved when. But there was more. The first game theorists had sought unique equilibrium solutions to their problems. These would inevitably be independent of history. But as research advanced, it became clear that the games frequently possessed multiple possible equilibria.[4] Because only one outcome could actually happen, theorists needed to think about selection principles. Players' expectations came to be recognized as quite important, as did the history of relations between the players.[5] Time and context mattered after all.

The essays in this volume and its predecessor have much more in common with game theory in this later phase than they do with the early models that historians have criticized. But they are still much more oriented to questions of organization than to questions of development. There is, however, an emerging body of theory that focuses attention on developmental issues, particularly on how expectations and organizational routines lead over time to the creation of competences that are difficult for other firms to replicate.[6] Such competences contribute to the long-term sustainability of profits under conditions of product-market competition. The new theory promises a foundation for comparative historical work at the level both of firms and nations. We plan to assess its usefulness in subsequent work.

The Essays

The ideas about imperfect information and its consequences that we described above provided the impetus for the current volume. Although only some of the essays employ them, or even formal economic theory, in a direct or self-conscious way, all are motivated by questions growing out of this literature. They provide much detail on the organization of firms and markets that

4. The problem is even worse with repeated games, the most natural setting for formalizing the sort of issues we study.

5. The classic of this idea, though it first appeared well before the literature we are discussing here, is Schelling 1960. For a recent textbook treatment, see Kreps 1990.

6. Its roots lie in Nelson and Winter 1982. See, for example, Teece, Pisano, and Shuen 1994.

should be new to economists and historians. They also contribute collectively to the development of a more sophisticated view of economic coordination under conditions of imperfect information—particularly of the role played by large managerial firms.

Part I focuses on coordination problems within the firm. In the first essay Daniel Raff addresses employment relationships in the early-twentieth-century automobile industry. He shows that as technology changed—as the industry moved from artisanal production to Ford-style mass production—the value of coordinating the efforts of employees also changed and so did the compensation systems manufacturers employed. Under a system of artisanal production, with only minor complementarities between jobs, a decentralized system of task and effort management may well have been a profit-maximizing one. With increasing interdependence between and sometimes within work groups, however, the value of coordination grew. The central idea of the paper is that compensation systems are incentive systems and, as such, are useful to managers as instruments of control. With the new technology, the old instruments were expensive in their information-processing demands. The new compensation systems helped managers achieve their desired level of coordination through economizing on information processing in their task of aligning workers' incentives with managers' goals.

Daniel Nelson's essay offers a contrasting view of the diffusion of new coordination and compensation schemes within the firm. Whereas Raff emphasizes the importance of changing production techniques, Nelson argues that the coordination techniques employed by firms may be affected by broader intellectual movements and by the motives of the leading figures behind them. He links the rise and decline in the popularity of incentive pay schemes to the rise and decline of the industrial engineering movement. The article explains the spread of these schemes in terms of incentives for quick results built into the commercial relationships between industrial-engineering consultants and their client firms. The consultants advertised "a complete mental revolution," but they focused their efforts on changes that could be implemented promptly and would have observable consequences, a very different thing. Together, the Raff and Nelson papers encourage the reader to think about compensation systems as a part of a larger organizational design problem for the firm. They also raise the question of why radically new ideas and profound organizational innovations might, as they often do, actually diffuse quite slowly.

Nelson's article also raises the possibility that the conflicting ideas and interests of different managers within a firm might inhibit the adoption of innovative techniques. In the concluding essay of part I, Bernard Carlson uses the case of the Thomson-Houston Electric Company to argue that similar disagreements could prevent a firm's managerial hierarchy from functioning smoothly. Even when managers were all thoroughly committed to the success of the enterprise, their different perspectives within the firm gave rise to different visions of what policy should be—conflicts that had to be mediated for the firm to operate successfully. As Carlson shows, the history of a firm's institutions must be understood at least in part as the history of the comparative influence of these interests within the firm. The relative position of the contenders in one period affected the balance of power in the next. In other words, path dependency plays an inevitable and in fact highly salient role in such a history.

Part II extends the book's focus to the institutions that define the boundaries of firms and so to the location and porosity of those boundaries. Michael Enright's chapter is a contribution to the literature on alternatives to the large integrated firm. It shows that the markets-versus-firms distinction is far too simple. Related industries are often clustered near each other in a highly stable fashion, creating the informational conditions that permit vertically disintegrated firms to develop a variety of heterodox coordination mechanisms. Enright discusses three case studies and identifies the benefits in flexibility that can derive from alternative methods of coordination. He also points out the potential costs of such arrangements if investment in local alliances results in conservatism about change.

David Mowery turns the reader's attention from the coordination of work and production to the coordination of technical change. He explains in information-theoretic terms why large firms developed their own research laboratories rather than buying research from independent providers. He also points out that these facilities did not completely internalize the R&D function. Rather, labs provided firms with the capability they needed to engage in the information-intensive business of assessing technical developments in the external environment. Mowery argues that after World War II large firms increasingly turned within themselves and relied more exclusively on innovations generated by their own research organizations. This change, he suggests, was a response to government regulation rather than to strictly economic factors and may have negatively affected the pace of technical change.

Tony Freyer continues Mowery's point that the regulatory environment can have an important effect on the boundaries of firms and the institutional mechanisms that firms choose to coordinate their activities. He argues that the different paths that antitrust law took in the United States and Britain encouraged the development of large firms in the former nation but not in the latter. Tolerance for cartels in Britain permitted small firms to survive and prosper. By contrast, the more vigorous anticartel policies pursued by the U.S. government forced firms that engaged in anticompetitive practices to merge with each other and bring within their bounds all the elements they needed to coordinate the industry. Ironically, then, antitrust policy in the United States operated to stimulate the very concentrations of capital it originally aimed to prevent.

Part III explores problems of coordination in the financial markets. Kenneth Snowden shows that imperfect information posed severe problems for firms that attempted to coordinate interregional lending in the mortgage market. Although some kinds of organizations functioned more effectively than others, private solutions to information problems inevitably had a destabilizing effect under certain unfortunately recurrent conditions. Only government intervention solved the underlying problems of asymmetric information.

The final article in the volume, by Charles Calomiris, compares the role that banks in the United States and Germany played in economic development. Because of regulatory restrictions, Calomiris argues, banks in the United States took a form that was much less capable of solving the information problems (referred to above) that firms face when they seek external finance. As a result, the cost of capital was significantly higher in the United States, and a more restricted group of firms had access to the equity markets.

Conclusion

The aim of this volume is to highlight some of the complexities that need to be addressed in the analysis of economic coordination under conditions of imperfect information. As the contributors show, methods of coordination vary with production technique. They can also be affected by intellectual movements and by the activities of those movements' entrepreneurs. Bigness itself is not a guarantee of superior coordination, for large multifunction firms have unique coordination problems that have to be worked out. In some situations large firms function better if boundaries are porous. Moreover, in some situations small, vertically disintegrated firms have advantages over managerial hierarchies.

The contributors have also shown that regulation can have important consequences for the way economic activities are coordinated. Implausible as it may seem to some, regulation can sometimes have a positive effect. The interregional mortgage market depends upon it, for example. The information-oriented perspective developed in this volume helps explain why, and it also helps us understand how regulation can under different circumstances force firms to choose less effective coordination mechanisms. The cases of commercial banking and technical change are good examples.

The study of economic coordination has important practical implications, but these can only be uncovered and effectively developed with appropriate conceptual tool kits. Economists have recently improved the helpfulness of the available tools, but the possibilities are far from exhausted. Here business historians have a great deal to offer. We hope that they will be stimulated by recent theoretical developments to ask more cogent questions of their evidence and that their work will in turn stimulate economic theorists to develop more useful models. Only through such dialogue will we gain greater understanding of the logic and real-life dynamics of the allocation of economic activity within and among firms.

References

Abernathy, W. J. 1978. *The Productivity Dilemma: Roadblock to Innovation in the Automobile Industry.* Baltimore: Johns Hopkins University Press.

Burroughs, B., and J. Helyar. 1990. *Barbarians at the Gate: The Fall of RJR–Nabisco.* New York: Harper and Row.

Calomiris, C. W., and R. G. Hubbard. 1993. Internal Finance and Investment: Evidence from the Undistributed Profits Tax of 1936–1937. NBER Working Paper no. 4288. Cambridge, MA: National Bureau of Economic Research.

Chandler, A. D., Jr. 1962. *Strategy and Structure: Chapters in the History of the American Industrial Enterprise.* Cambridge: MIT Press.

———. 1977. *The Visible Hand: The Managerial Revolution in American Business.* Cambridge: Harvard University Press.

———. 1990. *Scale and Scope: The Dynamics of Industrial Capitalism.* Cambridge: Harvard University Press.

Coase, R. 1937. The Nature of the Firm. *Economica,* n.s., 4:386–405.

Jensen, M. 1993. The Modern Industrial Revolution, Exit, and the Failure of Internal Control Systems. *Journal of Finance* 48:831–80.

Koopmans, T. C. 1957. *Three Essays on the State of Economic Science.* New York: McGraw-Hill.

Kreps, D. 1990. *A Course in Microeconomic Theory.* Princeton: Princeton University Press.

Lazonick, W. H. 1990. *Competitive Advantage on the Shop Floor.* Cambridge: Harvard University Press.

———. 1991. *Business Organization and the Myth of the Market Economy.* New York: Cambridge University Press.

Milgrom, P., and J. Roberts. 1992. *Economics, Organization, and Management.* Englewood Cliffs, NJ: Prentice-Hall.

Nelson, R. R., and S. G. Winter. 1982. *An Evolutionary Theory of Economic Change.* Cambridge: Harvard University Press.

Piore, M. J., and C. F. Sabel. 1984. *The Second Industrial Divide: Possibilities for Prosperity.* New York: Basic Books.

Schelling, T. C. 1960. *The Strategy of Conflict.* Cambridge: Harvard University Press.

Teece, D. J., G. P. Pisano, and A. Shuen. 1994. Dynamic Capabilities and Strategic Management. Manuscript.

Temin, P., ed. 1991. *Inside the Business Enterprise: Historical Perspectives on the Use of Information.* Chicago: University of Chicago Press.

Williamson, O. E. 1975. *Markets and Hierarchies: Analysis and Antitrust Implications.* New York: Free Press.

———. 1981. The Modern Corporation: Origins, Evolution, Attributes. *Journal of Economic Literature* 19:1537–68.

———. 1985. *The Economic Institutions of Capitalism: Firms, Markets, and Relational Contracting.* New York: Free Press.

This Page Intentionally Left Blank