Book Reviews

Firms, Contracts, and Financial Structure. Oliver Hart. Oxford, U.K.: Clarendon Press, 1995. 228 pp., $49.95 (hardcover), $19.95 (paper). ISBN 0-19-828850-6.

Why does General Motors make its own glass but not its own tires? Why are some firms heavily debt financed while others are not? These questions are examples of some of the most important issues of corporate finance: "What determines the firm's boundaries?" and "What determines the firm's financial structure?" The growth of the literature on these questions has, of late, been astonishing.[1] This growth has been fueled largely by the development of the new "economics of contracts" and the realization that those techniques can illuminate the issues in a way not previously available. Oliver Hart is one of the foremost practitioners of the "contracts approach" to corporate finance. His new book, based in part on his Clarendon Lectures given at Oxford University in May 1993, summarizes, synthesizes, and puts in context Hart's research on the two issues mentioned previously. The exposition takes advantage of Hart's and his collaborators' (Sanford Grossman and John Moore) ability to find simple models that capture the essence of their ideas, with a liberal use of numerical examples. As a result, the material is easily accessible to anyone with even a little training in microeconomics and requires almost no background in game theory. Although it is not clear to whom the book is addressed, I expect it to be essential reading for any economics or finance Ph.D. student interested in corporate finance. Thus, this volume should contribute to the development of the contracts approach to corporate finance.

The book consists of two more or less independent parts. They are related in that both apply a contractual

[1] My colleague, Gene Fama, maintains that when he began teaching finance a little over 20 years ago, one could spend at most a week on corporate finance. After explaining the famous irrelevance result of Miller and Modigliani, there was little left to say. Now, of course, most MBA programs have several courses devoted to aspects of corporate finance.

The Review of Financial Studies 1996 Vol. 9, No. 4, pp. 1271–1282

paradigm to explaining observed firm behavior. The first part considers the issue of firm boundaries, and the second considers the issue of financial structure. Hart focuses on the idea that real-world contracts are of necessity incomplete. That is, no actual contract can spell out exactly what will happen in every possible contingency. He argues that contractual incompleteness implies that the allocation of power within a relationship (that is, power to determine the outcome when an unaccounted-for contingency arises) affects the payoffs of all parties to the relationship. Power is, in turn, a function of who has control rights over resources. The allocation of control rights to the firm or to an independent entity determines whether a resource is included in the firm or contracted for externally. Optimal allocation of control rights among users of a resource then explains the scope of the firm. The same considerations apply to financial contracts as well. The financial structure of the firm is seen to be an outgrowth of the attempt to allocate control over assets optimally among claimants to the firm's cash flows (as opposed to users of those assets). Thus, for example, debt provides creditors with the power to seize and sell assets when covenants are violated, and equity allows stockholders to replace management when they see fit. The two sections of the book, although sufficiently independent to be read separately, fit together neatly within the incomplete contracts approach.

The ideas in the previous paragraph are set forth in an introductory chapter. Part I is devoted to understanding firm boundaries. This part contains four chapters. The first is a survey of more traditional theories of the firm, including the neoclassical approach, agency theory, and transaction cost theories. Hart argues that the first two models have little to say about the issue of firm boundaries. The transaction cost idea, however, forms the basis for the theory set out in Chapters 2 and 3. This approach, generally associated with Coase and Williamson, argues that transaction costs lead to incomplete contracts. These, in turn, lead to ex post holdup problems. Hart argues that the transaction cost model stops short of providing a theory of firm boundaries because it does not consider how the allocation of property rights can attenuate holdup problems. This shortcoming is remedied in Chapters 2 and 3.

Chapter 2 sets out the Grossman, Hart, and Moore theory of firm boundaries. Here one finds a simple two-player (buyer and seller) model in which contractual incompleteness and holdup problems cause underinvestment in human capital (relative to first-best). The extent of the underinvestment is affected by who controls which assets. The effect of control on investment leads to a theory of asset control (that is, firm boundaries) with interesting implications. For example, a player (firm) should not control an asset if her investment

choice is insensitive to the division of the surplus. Another example is that independent assets should not be combined under one player's (firm's) control, while strongly complementary assets should be combined. This may help us to understand why GM makes its own glass but not its own tires. A given tire can easily fit cars of many manufacturers, that is, there is little complementarity between the assets used to make tires and those used to make cars. Glass, on the other hand, must be tailor-made to fit precisely. That is, there is significant complementarity between the assets used to manufacture cars and those used to make auto glass. Chapter 2 ends with a discussion of the extent to which this theory can explain observed phenomena. Hart makes a convincing case that the incomplete contract approach can illuminate the fundamental question of what determines firm boundaries.

A number of issues, such as delegation and hierarchies, the effect of asset control on workers' incentives, etc., are left out of the discussion in Chapter 2. These topics are taken up briefly in Chapter 3. One of these, the question of whether control rights should be bundled with residual cash flow claims, foreshadows the discussion of financial structure in Part II. Here Hart argues that control and residual claims are complementary and hence should be bundled. Later, in Part II, Hart considers the role of securities in allocating control rights. There he shows that residual cash flow claims and voting rights (a form of diffuse control) should be bundled proportionately (the "one-share-one-vote" rule). This is a nice insight that reinforces the connection between the theory of firm scope in Part I and the theory of financial structure in Part II. The other points touched on in this chapter are also quite thought provoking.

Chapter 4 reviews the foundations of the incomplete contracting approach to the theory of the firm. Here Hart "consider[s] some natural ways to get round the incompleteness of contracts, and explain[s] why in many cases they won't work" (p. 73). This chapter misses a chance to focus attention on an important aspect of the approach that has received scant notice, namely the nature of the ex post bargaining that occurs because ex ante contracts are incomplete. The negotiations that determine the resource allocation in uncontracted-for situations are taken to be exogenous in the incomplete contracts literature. Hart assumes Nash bargaining (essentially that surplus is split 50-50 with no explicit bargaining game) in Chapters 2 and 3. In Chapter 4, Hart considers a mechanism in which each player is allowed to make a take-it-or-leave-it offer with probability one-half. Both these procedures result in situations where an investing player receives only part of the return on his investment (50% in the specific examples mentioned), with underinvestment being the inevitable outcome. If, in the model of Chapter 4, the buyer were allowed to make

a take-it-or-leave-it offer with probability one, she would capture the entire investment return. In this case, there would be no underinvestment. This is an especially simple example in which commitment to an appropriate renegotiation procedure entirely removes the effect of contract incompleteness. Even in a more complicated model,[2] however, Artur Raviv and I have shown that a carefully constructed renegotiation game can often mitigate the deleterious effects of contract incompleteness [see Harris and Raviv (1995)]. This point is worth thinking about in a variety of incomplete contract models (see below for an application to financial contracts).

Part II applies the incomplete contracting approach to the issue of financial structure. Financial structure here is painted with a broader brush than has been the case traditionally. Instead of trying to explain what determines the relative proportions of standard debt and equity contracts in firms' capital structure, Hart seeks to understand as well why these contracts are so popular. This leads him to focus on aspects of the contracts largely ignored in the traditional literature, namely aspects relating to the allocation of control rights over assets. As mentioned, Part II shares this concern over control rights with Part I. Unlike in Part I, however, relationship-specific investments and holdup problems play no role in the analysis in Part II. Instead, contracts are designed to deal with the agency problem created because individuals receive private benefits from managing assets but have insufficient wealth to be sole owners of the assets. Financial contracts with suppliers of wealth allocate both cash flows and control rights with a view to alleviating the resulting inefficiency.

Chapter 5 considers the case of a single entrepreneur and a single investor (or a small group of investors acting in concert). Several models are considered, starting with one proposed by Aghion and Bolton (1992). The Aghion and Bolton model explains why control of assets should be assigned on a contingent basis with investors taking control in some states. This is an important feature of debt contracts, but the model's other implications are not consistent with observed debt contracts. For example, the model does not predict that control shifts to the investor as a result of failure to make contractually specified payments. This leads to a consideration of the Hart and Moore (1989) model. The contract emerging from their analysis more closely resembles actual debt in that the investor acquires and liquidates assets when the entrepreneur fails to make a contractually specified payment.[3] Variants of this model are used to extend the

[2] In fact, we use the Hart and Moore (1989) model that forms the basis for much of Part II of the book.

[3] As noted above, however, Harris and Raviv (1995) show that the renegotiation scheme assumed

analysis to multiple periods and uncertain environments. There is a brief discussion of how free-rider problems among multiple investors can make strategic default less attractive (this phenomenon is known as a "hard budget constraint"). The chapter concludes with a discussion of related work and an appendix on the "Costly State Verification" (CSV) approach to explaining debt first proposed by Townsend (1979). Hart criticizes the CSV approach as a theory of debt on a number of grounds. Most telling, perhaps, is the criticism that the model is unlikely to yield debt-like contracts when extended to multiple periods. Although there are problems with many of the models presented in this chapter, researchers will continue to find them a useful jumping-off point for understanding debt.

Although Chapter 6 is billed as focusing on the implications of multiple investors, it seems to be mainly concerned with explaining the quantity and seniority of debt. Several models are considered in which managers have a preference for overinvesting. Probably the most interesting section of Chapter 6 is Section 5. The ostensible purpose of Section 5 is to show how the models of this chapter can explain observed phenomena. In fact, one is left with the impression that these models have little or no empirical content. Here's a sample of some of the conclusions in Section 5. Model 1, with a number of strong additional assumptions, can explain the observed negative relationship between profitability and debt. As Hart points out, however, "a small change in the assumptions could generate a positive relationship" (p. 142). Hart goes on to point out that "Model 2 does not predict a clear relationship between profitability and debt either" (p. 142). Later (p. 143) Hart admits that "the theory can explain why firms with stable cash flows have more debt, but it can also explain the opposite." Finally (p. 145), Hart states that although model 1 is consistent with observed equity price increases in the event of debt-for-equity swaps, "under a different information structure, model 1 predicts that such swaps can reduce the value of equity." One rarely sees such thorough criticism of a model from a referee, much less the model's author.

Any contract that calls for specific actions by the parties to the contract must specify, at least implicitly, what results from nonperformance. For debt contracts, this specification is contained in the bankruptcy code of the relevant jurisdiction. Chapter 7 takes up the issue of bankruptcy procedure. The design of bankruptcy procedures has recently commanded considerable attention, both among econo-

by Hart and Moore is inefficient, and, when the parties can commit to a renegotiation scheme, the resulting optimal contract does not resemble debt.

mists and in the general public. This is due, no doubt, to the need to formulate such procedures as part of the general problem of defining property rights in the newly "liberated" countries of the former Soviet block. Chapter 7 sets out the goals of a bankruptcy regime, describes and critiques some existing procedures and proposes an alternate approach based on Aghion, Hart and Moore (1992, 1994, 1995) and Bebchuk (1988). Hart calls this approach the "Aghion-Hart-Moore" (AHM) procedure. The essence of the procedure is twofold. First, noncash bids for the company (that offer securities in the post-bankruptcy company) are allowed. Second, all debts are replaced by equity and options (senior debt holders receive the equity, and each class of junior claimants receives an option to buy the security of the class above). Noncash bids allow for reorganization and/or liquidation, and the new capital structure creates a homogeneous class of claimants while preserving absolute priority. The AHM procedure is certainly appealing, but its insistence on preserving strict absolute priority is not necessarily an advantage: sometimes efficiency requires violation of absolute priority. This can occur because such violations give managers in control of the firm incentives not to take actions harmful to firm value. Indeed in the Hart and Moore (1989) model of Chapter 5, one can argue that violations of absolute priority can increase efficiency [see Harris and Raviv (1995)]. Still, the discussion in Chapter 7 is an important step in developing a normative theory of bankruptcy procedures.

Despite the title of Chapter 6, the real analysis of multiple investors in Hart's book is set out in Chapter 8. Here the argument for one-share-one-vote contained in Grossman and Hart (1988) and Harris and Raviv (1989) is aptly exposited. As mentioned, the logic is related to the theory of firm boundaries presented in Part I: since cash flow claims and voting rights are highly complementary assets, they should be bundled. Put another way, one-share-one-vote "forces someone who wants to obtain control of the company to acquire a share of the company's dividend stream commensurate with this control" (p. 189). The chapter concludes with a very brief mention of some empirical evidence and some suggestions for further work in the area. Chapter 8 provides a strong finish for the book.

Firms, Contracts, and Financial Structure provides an excellent exposition of the incomplete contracts approach to the theory of the firm. In my view, Part I on firm boundaries is the stronger half. In Part II, Chapters 5 (the basic model of debt), 7 (on bankruptcy), and 8 (on voting rights) are also quite useful. At the risk of pointing out the obvious, let me remind the reader that this book is not a comprehensive examination of the theory of the firm nor even of the incomplete contracts approach. Instead, it is a fine survey of the author's contri-

butions to the theory of firm boundaries and financial structure. As such, I commend it highly.

Milton Harris
University of Chicago

References

Aghion, P., and P. Bolton, 1992, "An 'Incomplete Contracts' Approach to Financial Contracting," *Review of Economic Studies*, 59, 473–494.

Aghion, P., O. Hart, and J. Moore, 1992, "The Economics of Bankruptcy Reform," *Journal of Law, Economics and Organization*, 8, 523–546.

Aghion, P., O. Hart, and J. Moore, 1994, "Improving Bankruptcy Procedure," *Washington University Law Quarterly*, 72, 811–827.

Aghion, P., O. Hart, and J. Moore, 1995, "Insolvency Reform in the UK: A Revised Proposal," *Insolvency Law and Practice*, 11, 4–11.

Bebchuk, L., 1988, "A New Approach to Corporate Reorganizations," *Harvard Law Review*, 101, 775–804.

Grossman, S., and O. Hart, 1988, "One Share-One Vote and the Market for Corporate Control," *Journal of Financial Economics*, 20, 175–202.

Harris, M., and A. Raviv, 1989, "The Design of Securities," *Journal of Financial Economics*, 24, 255–287.

Harris, M., and A. Raviv, 1995, "The Role of Games in Security Design," *Review of Financial Studies*, 8, 327–367.

Hart, O., and J. Moore, 1989, "Default and Renegotiation: A Dynamic Theory of Debt," Working Paper 520, Massachusetts Institute of Technology.

Townsend, R., 1979, "Optimal Contracts and Competitive Markets with Costly State Verification,"*Journal of Economic Theory*, 21, 265–93.

Strong Managers, Weak Owners. Mark Roe. Princeton, New Jersey: Princeton University Press, 1994. 324 pp., $39.50 (cloth).

The dominant paradigm in corporate finance theory is that of Berle and Means (1932) who argue that modern public corporations, by sheer virtue of their size, entail a separation between the shareholding owners of the firm — those who own the property rights — and the managers who make the day-to-day decisions — those with the effective control rights. Berle and Means recognize that when property rights and control rights are separated, it is difficult to say exactly what "control" means. "Control divorced from ownership is not . . . a familiar concept. . . . Since direction over the activities of a corporation

is exercised through the board of directors, we may say for practical purposes that control lies in the hands of the individual or group who have the actual power to select the board of directors" (p. 69). They then identify five types of control: (1) control "through almost complete ownership;" (2) majority control; (3) "control through a legal device without majority ownership;" (4) minority control; (5) management control. Analyzing the 200 largest U.S. corporations in December 1928 they find that 44% by number and 58% by total assets are controlled by management. These numbers rise to 65% and 80%, respectively, when control through a legal device (e.g., pyramiding) is included. As a result, this "explosion of the atom of property destroys the basis of the old assumption that the quest for profits will spur the owner of industrial property to its effective use" (p. 9).

Since Berle and Means the separation of ownership and control has been formalized in principal-agent models where the shareholders are viewed as the principal and the manager as the agent. The principal/shareholders do not have "control" since the agent/manager makes an effort that is not observable or not verifiable. This model does obtain the result that industrial property is not put to its first-best use. Based on this paradigm, research effort has been aimed at detailing how shareholders design institutions to improve the efficiency of managers, given a separation of ownership and control. Executive compensation contracts, takeovers, board composition, and design of the corporate charter are some of the institutions which shareholders can use to try to link the agents'/managers' control with their interests. Also, most empirical work in corporate finance is based on the Berle and Means world view.

Mark Roe, in his new book, *Strong Managers, Weak Owners*, reconsiders the Berle and Means thesis. The book intertwines legal and political history with law, economics, and politics. The interdisciplinary nature of the analysis, like Berle and Means' original analysis (Berle was a lawyer and much of their book is legal history), makes the book unique. The basic point of Roe's book is that "the Berle-Means corporation cannot be fully explained by economics alone" (p. 170). What Roe means is that the separation of ownership and control described by Berle and Means is not a separation inherent in capitalism. Berle and Means take the dispersion of stock ownership as given. Roe wants to explain it, that is, why isn't there more blockholding. That there is something to explain is evidenced by the fact that other advanced capitalist economies appear to be organized quite differently. Roe sees Germany and Japan as examples of advanced capitalist economies where there is more pervasive block shareholding and hence a less obvious separation of ownership and control than in the United States. In fact, one may view Roe's book as addressing the question of why

corporate finance in the United States appears to be quite different from Japan and Germany (the other two economies that are discussed in the book).

Roe proposes a political paradigm to explain the American system, namely, that it is the outcome of deeply held populist attitudes emanating from both the populist movement in the last half of the nineteenth century, and the widespread general view, beginning with the founding fathers, that concentration of economic power in large institutions should be limited. [On the populist movement see Goodwyn (1978).] The idea that populism (and federalism) shaped the landscape of American capitalism is hardly new. Historians have understood and debated the details of this for some time [see, for example, Hays (1957), Pollack (1962), and Porter (1973)]. What is new here are the details of how the populist sentiment led to restrictions on financial institutions playing any influential roles in controlling managers. Banks, insurers, mutual funds, and pension funds were all restricted by legislation. The ability to engage in hostile takeovers has also been constrained; sometimes less, sometimes more. Fascinating parts of the book include chapters on the legal histories of banks, insurers, mutual funds, and pension funds. Later in the book, Roe in much less detail contrasts the institutions of corporate governance in Germany and Japan with those of the United States. He stresses the activism of large financial firms as shareholders in nonfinancial firms in Germany and Japan. "Senior managers in Germany and Japan face frequently active intermediaries that, among other things, wield the votes of large blocks of stock" (p. 169).

The emphasis of Roe's book on explaining the origins of institutions and on comparative finance are important. Comparative finance has recently emerged as important as a public policy issue (e.g., should pension funds be more active?), but it is also important to know whether and to what extent there are political constraints on the organization of firms. Such issues raise important theoretical questions since it is difficult to separate features of firms that are prevalent in all capitalist economies from features that are idiosyncratic manifestations of one nation's history. For example, Jensen and Murphy (1990) argue that "political forces . . . implicitly regulate executive compensation by constraining the type of contract that can be written between management and shareholders" (p. 227). Thus, in order to assess whether the principal-agent model has failed to explain executive compensation means, at least in part, that the role of politics has to be recognized. This problem is pervasive in corporate finance.

Roe's thesis concerning the Berle and Means corporation and the structure of shareholding in Germany and Japan also seems to suffer a bit from this problem of differentiating between inherent features

of advanced capitalist economies and features that are specific to a national form. When Berle and Means analyzed control in the top 200 corporations in the United States they had no category explicitly for financial institutions' blockholding. Recall that they were writing before the passage of the Glass-Steagal Act, so bank stock ownership was legal. Presumably, such cases would fall into the category of "control through a legal device without majority ownership," but their focus is on pyramiding. Berle and Means considered this category to be ambiguous in terms of its implications for managerial power. In this case, "[control] may rest to a very considerable extent on the ownership of a large minority stock interest, or, when stock ownership is widely distributed, it may lie in the hands of management. No sharp dividing line exists" (p. 80). In fact, it is interesting that they did not discuss banks or other financial institutions at all. Roe's arguments concern why blockholding financial firms do not exist in the United States. Roe's view is that block ownership per se is evidence against the Berle and Means view of the firm. This seems problematical. In Germany and Japan, banks own sizable blocks of stock (relative to the United States), but it is not clear that these "owners" act in the same way that they would if the blockholder were a rich individual. It remains an open (empirical) question as to whether bank *managers* act in the shareholders' interest. Thus, whether there is any less of a separation of ownership and control in Germany and Japan because of the presence of blockholding financial firms is not clear.

Another way to put the issue concerns the role of the stock market, which plays more of a role in the United States than it does in Germany or Japan. Why is the stock market more central in the United States than in Germany? Is this the counterpart to the absence of blockholding by financial firms in the United States? Another critic of Berle and Means, Demsetz (1983), implicitly argues that the role of the stock market is important. Demsetz argues that the Berle and Means notion that a dispersed set of shareholders is inherent in large firms is incorrect. Rather, Demsetz argues that the structure of shareholding is endogenous; blocks will arise when and as they are needed to control managers. This is one function of the stock market: blocks can be formed when needed because of liquidity provided by the market. Because of risk aversion it is not likely that a rich individual will hold a block at all times, but only when there are large benefits from being a blockholder. Demsetz and Lehn (1985) provide some evidence for this viewpoint. In Germany and Japan, individuals hold diversified portfolios by holding claims on banks, but then they must rely on the bank to act in their interest with respect to any particular firm in which the bank holds a block. Reliance on the bank would seem to be incentive compatible if the bank has no where to sell the

stock (since the stock market for the stock is thin or nonexistent). With no way to sell the stock, the bank has more of an incentive to monitor.

Notably, Berle and Means attach no particular influence to the stock market. They have one short chapter on the function of the market and it does not stress any control features. Rather, the market is "primarily designed to secure liquidity and . . . [permits] an open market appraisal through the operation of buyers and sellers" (p. 297). Roe's chapter on takeovers (Chapter 10) is also short and not so convincing. He stresses the role of politics in the takeover wave of the 1980s. The emphasis is on the populist and federalist basis of the antitakeover laws. But it is not the case that the populist basis of politics leads only to an anti-financial firm viewpoint. Populism as a political sentiment also favors tighter control over large corporations since they, like banks, could be powerful in the absence of restrictions. As Porter (1973) notes: "Throughout our national history, Americans have shaped much of their politics around one or another variant of the struggle against big business. The lineage of ideas about the evils of large-scale business goes back at least as far as Thomas Jefferson" (p. 2). This sentiment has manifested itself in antitrust legislation, perhaps making many firms smaller than they would otherwise be. Antitrust legislation, resulting in smaller firms, may well mean that the stock market is more important in the United States since blocks are easier to assemble in the market when they are needed.

The questions of how politics and economics are related, what the roles of financial firms and stock markets are, and what features of capitalism are inherent in capitalism rather than manifesting national characteristics are all part of Roe's book. Their interdisciplinary nature makes for great reading.

Gary Gorton
University of Pennsylvania

References

Berle, A., and G. Means, 1932, *The Modern Corporation and Private Property*, McMillan, New York.

Demsetz, H., 1983, "The Structure of Ownership and the Theory of the Firm," *Journal of Law and Economics* 16, 375–390.

Demsetz, H., and K. Lehn, 1985, "The Structure of Corporate Ownership: Causes and Consequences," *Journal of Political Economy* 93, 1155–1177.

Goodwyn, L., 1978, *The Populist Moment*, Oxford University Press, Oxford.

Hays, S., 1957, *The Response to Industrialism: 1885–1914*, University of Chicago Press, Chicago.

Jensen, M., and K. Murphy, 1990, "Performance Pay and Top-Management Incentives," *Journal of Political Economy* 98, 225–264.

Pollack, N., 1962, *The Populist Response to Industrial America*, W. W. Norton, New York.

Porter, G., 1973, *The Rise of Big Business*, AHM Publishing, Arlington Heights, Ill.