PN-ABL-114
87225

IRIS Reprint

The Center for Institutional Reform and the Informal Sector (IRIS) has two main purposes: expanding knowledge about institutions in economic development through research, and assisting reform efforts in the third world and in countries undergoing transitions to a market economy. The premise of the IRIS Center is that in unsuccessful economies the existing rules establish poor incentives, often forcing economic activity into the informal economy, and that appropriate reforms improve economic performance. IRIS is especially concerned with the legal and policy framework needed for democratic societies with competitive markets.

IRIS
7100 Baltimore Avenue, Suite 510
College Park, MD 20740
(301) 403-8153

CENTER FOR INSTITUTIONAL REFORM AND THE INFORMAL SECTOR

University of Maryland at College Park

Center Office: IRIS Center, 7100 Baltimore Avenue, Suite 510, College Park, MD 20740
Telephone (301) 403-8153 • Fax (301) 403-8163

THE CORPORATION AND THE ECONOMIST

February, 1992

Dennis C. Mueller

IRIS Reprint No. 11

This publication was made possible through support provided by the U.S. Agency for International Development, under Cooperative Agreement No. DHR-0015-A-00-0031-00.

Author: Dennis C. Mueller, University of Maryland at College Park

Reprinted from *International Journal of Industrial Organization*, Vol 10, 1992.

International Journal of Industrial Organization 10 (1992) 147–170. North-Holland

The corporation and the economist

Dennis C. Mueller*

University of Maryland, College Park, MD, USA

Received October 1991, final version received February 1992

This paper reviews what economists have said about the corporation and economic research on the corporation over the last 200 years. It argues that economists have made less progress in understanding the corporation than they might have due to their unwillingness to adopt a more realistic set of behavioral assumptions about managerial motivation. This point is illustrated by reference to the principal/agent literature and to articles in recent issues of leading industrial organization journals. A richer behavioral foundation for research on the corporation, and in economics more generally, is advocated.

The recent collapse of communist systems has been widely interpreted as a verification of the superiority of market institutions, the triumph of capitalism as it evolved in the West over communism as it developed in the East. It is difficult to imagine capitalism's evolution over the last two centuries without the concomitant development of the corporation [Williamson (1985)]. If capitalism has triumphed then so too has the modern corporation.

Although Adam Smith would not have been surprised to learn of the greater efficiency of market-based systems, he would not have expected the corporation in its present form to have been the optimal organizational structure (see below). Could Smith have been so incisive in his analysis of market institutions, and yet been so inaccurate in his appraisal of the corporation? Alternatively, may Smith have been correct about corporations too, and capitalism have succeeded in spite of rather than because of the development of the corporation?

I shall not attempt to answer this weighty question in this essay. Rather my point is that we, the economics profession, are ill-equipped to answer this and other important questions about what corporations are and do because we have come to rely too much on abstract simplification of what corporate actors do (or ought to do). To understand the corporation, and the important role it plays in the economy, we need to build our analysis on a

Correspondence to: Dennis C. Mueller, Department of Economics, University of Maryland, College Park, MD 20742, U.S.A.

*My gratitude for their helpful comments goes to Keith Cowling, Paul Geroski, Robin Marris, Manfred Neumann, and Hiroyuki Odagiri, and for financial support to the IRIS program.

0167–7187/92/$05.00 © 1992—Elsevier Science Publishers B.V. All rights reserved

2

richer behavioral foundation. I begin, appropriately enough, with Adam Smith.

1. The Early Economists

Corporations, or joint stock companies as they were commonly called at the end of the 18th century, were relatively rare when Adam Smith wrote *The Wealth of Nations.* But Smith had seen enough of them to offer the following observations:

> The directors of such companies... being the managers rather of other people's money than of their own, it cannot well be expected, that they should watch over it with the same anxious vigilance with which the partners in private copartnery frequently watch over their own. Like the stewards of a rich man, they are apt to consider attention to small matters as not for their master's honour, and very easily give themselves a dispensation from having it. Negligence and profusion, therefore, must always prevail, more or less, in the management of the affairs of such a company ... It is upon this account that joint stock companies for foreign trade have ... very seldom succeeded without an exclusive privilege; and frequently have not succeeded with one. Without an exclusive privilege they have commonly mismanaged the trade. With an exclusive privilege they have both mismanaged and confined it. (1776, p. 700)

John Stuart Mill regarded this conclusion as 'one of those overstatements of a true principle, often met with in Adam Smith' (1885, p. 140). Nevertheless, he did regard the principle as true. After discussing the advantages of joint stock companies, Mill took up 'the other side of the question'.

> [I]ndividual management has also very great advantage over joint stock. The chief of these is the much keener interest of the managers in the success of the undertaking.
>
> The administration of a joint stock association is, in the main, administration by hired servants. Even ... the board of directors, who are supposed to superintend the management ... have no pecuniary interest in the good working of the concern beyond the shares they individually hold, which are always a very small part of the capital of the association, and in general but a small part of the fortunes of the directors themselves; and the part they take in the management usually divides their time with many other occupations, of as great or greater importance to their own interest; the business being the principal concern of no one except those who are hired to carry it on. But experience shows, and proverbs, the expression of popular experience,

> attest, how inferior is the quality of hired servants, compared with the ministration of those personally interested in the work, and how indispensable, when hired service must be employed, is 'the master's eye' to watch over it. [Mill (1885, pp. 138–9)]

Smith and Mill were, of course, giants in the development of economics. That each had a brilliant mind goes without saying. But their genius stemmed not only from their capacity to reason. Each was also a keen observer of people and institutions. Each could draw generalizations from what he saw, that others would recognize to be true upon hearing. Another good example of this is Smith's statement that 'People of the same trade seldom meet together, even for merriment and diversion, but the conversation ends in a conspiracy against the public, or in some contrivance to raise prices' (p. 128). As with his statements about corporations, what is particularly interesting about this famous passage is that it is not a proposition about what businessmen under certain assumptions will do. It is a statement about what they *do do*. Smith's classic treatise is not a series of hypotheses about how individuals and markets will behave, but rather a treasure chest of observations of how they do behave.

Alfred Marshall was, of course, first and foremost a keen observer of the world of business. He, too, had some doubts about the efficiency of joint stock companies. In a section entitled 'Temptations of joint stock companies to excessive enlargement of scope' of Industry and Trade (1923) Marshall notes that 'unfortunately many [outside directors] are unable to give the large time and energy needed for obtaining a thorough mastery of the affairs of the companies for which they are responsible' (p. 321). The slack thereby created can lead to 'excessive enlargement of scope', because company managers 'cannot always approach a proposal for enlarging an existing department, or starting a new one, without some bias' (p. 322). Nevertheless, he was much less concerned about the potential inefficiencies of the corporate form that Smith and Mill, for he judged there to be a countervailing development that protected the 'powerless' position of the shareholders.

> It is a strong proof of the marvelous growth in recent times of a spirit of honesty and uprightness in commercial matters, that the leading officers of great public companies yield as little as they do to the vast temptations to fraud which lie in their way ... There is every reason to hope that the progress of trade morality will continue... [Marshall (1920, p. 253)]

The founders of neoclassical economics on the western side of the Atlantic were also sanguine in their views about the newly emerging corporations and trusts formed through merger. Anticipating the reasoning underlying transaction costs economics and the 'new learning' in industrial organization by 80

4

years they deduced that Darwinian competition could be relied upon to select only the most efficient combinations of assets.[1]

The next landmark in our intellectual history is the publication of *The Modern Corporation and Private Property* in 1932. As with many of the arguments contained in *The Wealth of Nations*, much that Berle and Means wrote about the corporation was known at the time they wrote. But they combined their thesis with an exhaustive history of the evolution of the corporate legal form, and amassed data demonstrating the extent of the separation of ownership from control, and the rise in aggregate concentration that had occurred in the first third of the 20th century. If the dangers of dispersed shareownership forewarned by Smith and Mill were real, Berle and Means's book suggested the United States had much to fear.

The bulk of *The Modern Corporation and Private Property* must have been written in the late 1920s, and thus the book cannot be construed as an account of the collapse of 1929. But the timing of the book's publication could not have been better. Berle and Means's evidence of the potential for managerial abuse of discretion created by the separation of ownership and control resonated with the thunder of falling stock prices and profits. The handful of examples of the abuse of managerial power presented by Berle and Means were duplicated and dwarfed by the accounts appearing daily in the business press.[2] The modern corporation appeared to have fulfilled the worst fears of Smith and Mill, and dashed the hopes of Marshall. The Victorian noblesse oblige that Marshall saw protecting shareholders as late as 1920 had by 1930 vanished, at least in the United States.

2. The 'marginalist' controversies

With the publication of the Berle and Means book, the Great Crash and its aftermath of revelations of misuse of position by managers, the issue of corporate efficiency could not be ignored, or so it would seem. But most economists did ignore it.[3] For by the 1930s, the neoclassical revolution, in which Alfred Marshall had played such an important part, had triumphed. When a new issue would arise, the economist would no longer turn to his first-hand knowledge of the relevant facts and institutions for addressing this issue, or lacking first-hand knowledge proceed to gather it. The economist's first reaction would now be to turn to one of the models he had used to

[1]See in particular the quote of John Bates Clark in Letwin (1965, p. 74) and Stigler (1950, p. 76).

[2]On these see Galbraith (1972).

[3]When not ignoring it, the economist would often ridicule it. As late as 1982, at a conference held ostensibly to 'celebrate' the fiftieth anniversary of the publication of *The Modern Corporation and Private Property*, the tenor and tone of the papers [illegible]als that many came not to praise the book but to bury it (see special issue of *Journal of Law and Economics*, June 1983). Douglas North's (1983) comment is a nice exception.

5

analyze similar problems in the past. The neoclassical models had proved themselves to be insightful analytic tools for laying bare the basic elements of certain economic problems. To achieve their pedagogic potential they needed to be kept simple, however, and so it was often the case that individuals were assumed to choose a single instrument (e.g. price) to achieve a single goal (profit). Thus, although the managerial corporation was by the 1930s the dominant economic institution of Western capitalism, the firm (entrepreneur) remained the main business actor in the economics literature, and it (he) maximized profit.

The 1930s were difficult for mainstream economics to digest. Much seemed to be happening that the newly developed neoclassical models could not explain. Keynes's response is the most famous reaction, of course. But attacks on the micro front were also afoot. A number of economists were troubled by the failure of prices to fall to eliminate significant amounts of excess supply. Gardner Means (1935) attempted to account for this with the thesis that large corporate firms in oligopolistic markets enjoyed considerable freedom to *administer* prices independently of market forces. Hall and Hitch were struck by interview evidence that revealed a gross inconsistency between 'the way in which business men decide what price to charge for their products and what output to produce' and the behavior assumed in neoclassical models (1939, p. 12). They concluded that the evidence 'casts doubt on the general applicability of the conventional analysis of price and output policy in terms of marginal cost and marginal revenue, and suggests a mode of entrepreneurial behavior which current economic doctrine tends to ignore' (p. 12). They offered as an alternative to marginal analysis a 'full cost' or mark-up model of pricing. Richard Lester was left with 'grave doubts as to the validity of conventional marginal theory and the assumptions on which it rests' from the answers given by 58 entrepreneurs from the South to a questionnaire circulated in the mid-1940s (1946, p. 81). Kaplan, Dirlam, and Lanzillotti (1958) conducted interviews of chief executives in the late 1940s and mid-1950s and uncovered a variety of objectives and rules of thumb for setting prices, that did not resemble marginal cost equals marginal revenue. Thus evidence gathered over two decades and two countries on how managers actually do set prices directly contradicted the assumptions upon which most economic modeling of pricing was at that time, and is today, based.

Not surprisingly these challenges to the mainstream view as to how prices are set were vigorously repelled [Machlup (1946, 1947); Kahn (1959)].[4] What is interesting is that the defenders of the neoclassical model offered neither contradictory interview and questionnaire evidence to support their positions, nor other systematic evidence that would allow one to reject one

[4]Friedman's (1953) and Becker's (1962) famous essays could also be cited here.

6

hypothesis and not the other. Rather the argument was made that it was not important that individuals consciously maximize as posited in the economic models, but that they act *as if* they did. Examples from the interview/questionnaire evidence or from everyday life were then used to suggest that indeed the data would sustain, if systematically garnered, the neoclassical model.

Although the direct rejoinders to the attacks on marginalist pricing models did not present data to support their positions, others did. Among these, the most famous perhaps is George Stigler's (1947) demolition of the Hall–Hitch–Sweezy kinked-demand schedule explanation of price rigidity in the 1930s. Stigler argued quite correctly that a kink should only exist for oligopolies, and thus that the relationship between price changes and concentration or number of sellers should be U-shaped. Only oligopolies should change price less frequently than profits maximization would imply. Stigler presented data on numbers of price changes in markets with different numbers of firms that dramatically rejected the U-shape prediction. The number of price changes in a market increased directly with the number of sellers. In the two markets with one seller, aluminum and nickel, there were respectively two and zero price changes between June of 1929 and May of 1937.[5]

It is difficult to believe that in a decade as unusual as the 1930s that the demand for a basic industrial product like nickel did not shift sufficiently to induce at least one change in the profit-maximizing price for this monopolist, especially since the coefficient of variation of output for this industry was 6th largest of the 21 industries Stigler examined. The Stigler results, while destroying the kinked-demand schedule hypothesis, raised the puzzling question of why price rigidity increases with concentration, and Stigler admitted 'that the neoclassical theory does not provide a satisfactory explanation for this extraordinary rigidity of monopoly prices' (p. 428). The lesson drawn by the profession from Stigler's paper was, however, only that the data had rejected the challenges to neoclassical theory offered by Hall, Hitch and Sweezy. That the data were equally inconsistent with what neoclassical theory predicted was ignored.

In the mid-forties one would not have had to cast about far to find a hypothesis that fit these results, however. Gardiner Means's administered price hypothesis argued that large corporations held prices constant for long periods in markets dominated by a 'relatively small number of concerns' (National Resources Committee (1939, p. 143)], as quoted in [Scherer (1980, p. 350)]. That George Stigler would not immediately seize upon the administered price hypothesis to explain his results is not surprising. Indeed,

[5]Stigler's initial findings have been substantiated in several other studies, e.g., Simon (1969); Primeaux and Bomball (1974); and Primeaux and Smith (1976).

7

a generation later he and James Kindahl (1970) were to publish a major empirical study that they claimed refuted the administered price thesis. In fact, the price rigidities that were observed could be reconciled with 'traditional theory', if the latter was appropriately modified by additional assumptions regarding long term contracts and transaction costs [Stigler and Kindahl (1973, p. 719)]. Both Means (1972) and Leonard Weiss (1977) followed with empirical studies that they claimed were consistent with the administered price thesis. Many additional studies examined the flexibility of prices. I shall not dwell on this literature,[6] but merely assert that the work of that period did not produce a resounding victory for the marginalist model in any standard form. But the profession proceeded ahead as if it did.

3. The managerialist challenge

The attacks on economic orthodoxy just discussed all questioned the implications of profits maximization with regards to pricing decisions. In the fifties and sixties studies appeared that directly questioned the profits maximization assumption, and directly or indirectly the predictions of neoclassical economics regarding decisions other than price. William Baumol (1959, 1967) hypothesized that managers maximized sales; Oliver Williamson (1963) added staff and emoluments to the managers' objective function; Robin Marris (1963, 1964) added the growth of the firm. Cyert and March (1963) posited four objectives in addition to profits pursued by the firm. Most fundamentally, Herbert Simon (1957, 1959) argued that managers did not maximize any objective function at all, they *satisficed*.

What needs to be stressed about each of these examples is that they all stemmed from observations about how managers and corporations actually behave. Simon's satisficing hypothesis originated from his work in psychology and his study of organizational behavior. His colleagues, Cyert and March, built on Simon's behavioralist approach and set out to describe the decisionmaking processes in actual, large corporations rather than to model an ideal, representative firm. To do so they constructed programming models of actual corporate decision structures. Williamson, a student of Simon, was also seeking a more realistic description of the 'managerial preference function' than existed at that time. Baumol's hypothesis arose from observations about the importance of sales to managers as an index of the health of their firm, and as a source of status (1967, pp. 44–8). Marris launches his study with a lengthy review of the literature on organizational behavior, which dwells on motivation, compensation formulae and the like.[7] Thus

[6]For s survey of it see Scherer (1980, pp. 350–362), and a more recent reexamination [Carlton (1986)].

[7]I also know from personal conversations with him that his thinking on these matters has been importantly influenced by personal experiences with a firm in his family.

8

each was seeking to model in a more accurate way the behavior of managers as they had actually observed it, or as they had come to understand it from reading a literature that came from outside of economics.

These challenges to economic orthodoxy were dismissed with arguments similar to those used to repel the attacks against marginalist price theory [Baldwin (1964); Peterson (1965); Machlup (1967)]. But empirical evidence was also brought forward. It is here that I enter the story.

In the late 1960s I was working on a series of case studies of how corporations grow. I had chosen to study growth by product innovation – Xerox; growth by marketing innovation – E.J. Korvette, the first discount department store; and growth by mergers – Litton Industries, and four others. I had interpreted the results from the cash flow model of firm expenditures in my dissertation as consistent with the assumption that managers maximize shareholder wealth (1967, p. 61). But as I interviewed the managers of Xerox, and studied the decisions and performance of each company, the behavior I observed did not accord with this assumption. Eugene Ferkauf, the entrepreneurial founder of E.J. Korvette, the first discount department store, was bent on expanding his department store chain as rapidly as possible. Between 1961 and 1965 its sales quadrupled as new stores opened almost every week. Its earnings per share also doubled, but its shareholders did not share in this prosperity. The firm paid no dividends, and its share price, although gyrating greatly, averaged no more in 1965 than in 1961. The Dow-Jones rose 32 percent during the five years.

Of the five companies I studied that had chosen mergers as a strategy for growth, only Litton Industries seemed to have done well for its shareholders. This company began to unravel as I studied it, however. Xerox's managers also appeared to have growth uppermost in their minds.[8] But given the investment opportunities Xerox faced in the 1960s, its shareholders also benefited from the rapid expansion of the company. This example illustrates an important subtlety with respect to the managerial-discretion-growth hypothesis, the conflict between shareholders and managers over investment rates is not necessarily greatest for the fastest growing firms. A simple correlation of company growth and shareholder returns does not suffice to test whether managers overinvest in their firms. Overinvestment must be measured *relative* to the investment opportunities of a firm. The most egregious examples of overinvestment may come from firms that are hardly growing at all, firms that should be rapidly shrinking. An example was one of my merger case studies. In the mid-fifties Borg Warner's management learned that it would lose a large chunk of its sales in a couple of years, because Ford would begin manufacturing its own automatic transmissions.

[8]I recall a senior vice president for the company looking at me incredulously, when I suggested that excess cash could be returned to the shareholders without paying the tax penalty on dividends by repurchasing its own shares. That would be 'self-cannibalization' he exclaimed.

The response of Borg Warner was not to downsize its operations drastically and pay its continuing cash flows out to its shareholders, but to add 27 new divisions, 15 through mergers. It was years before the company's shares would recover from this action. I concluded from these observations that the conglomerate mergers of the 1960s were better explained by Marris' growth maximization hypothesis, than by the stockholder wealth maximization hypothesis [Mueller (1969)], and that the managerial-growth hypothesis could usefully be interpreted in a life-cycle framework (1972).

In 1970 William Baumol and colleagues published estimates of rates of return on reinvested cash flows during the 1950s and 1960s ranging from 2 to 6 percent. These returns were significantly below both the returns shareholders were earning over this period, and the returns Baumol et al. estimated on new debt and equity issues. They corroborated the hypothesis that managers not subject to the discipline of external capital markets would, relative to what was optimal for their shareholders, overinvest their internal cash flows. As with every study that seemingly contradicts the conventional wisdom in economics, the Baumol et al. results were immediately challenged, and several additional studies followed.[9] In one of these Henry Grabowski and I brought in the life-cycle hypothesis (1975). Mature firms in industries with mature technologies earned significantly lower returns than young firms in industries with newer technologies. Our results also cast light on another paradoxical finding in the literature – the seemingly 'irrational' preference of shareholders for dividends over retained earnings. For our sample, it was only the shareholders of mature companies earning relatively low returns on investment who preferred dividends to retentions. The preference for dividends tended to disappear in the subsample of firms earning high returns on investment. Shareholders were not so irrational after all.

In the late 1960s Dale Jorgenson developed and tested a neoclassical theory of investment. The key determinant of investment for the shareholder-wealth-maximizing firm was the Modigliani and Miller cost of capital. Cash flow had no place in a neoclassical investment equation.[10] Of course Jorgenson was right. But cash flow did belong in the investment equation for the managerial firm, since it was this source of capital over which managers could exercise the most discretion. My dissertation results did not support

[9]This literature is reviewed in Mueller (1987, pp. 35–43).

[10]The pioneering study of the role of cash flow in an investment equation is of course that of Kuh and Meyer (1957). Although they interpret the strong performance of cash flow in their investment equations as consistent with the profits maximization hypothesis, cash flow enters their list of possible explanatory variables not as a result of the application of the marginal analysis, but because 'by far the most outstanding aspect of ... direct inquiries [about the determinants of investment] is their virtual unanimity in finding that internal liquidity considerations and a strong preference for internal financing are prime factors in determining the volume of investment' (p. 17). The third of the three reasons they give for the strong preference for internal funds is 'the hierarchical structure and motivations of corporate management which make outside financing asymmetrically risky for the established or in-group' (pp. 17–18).

the shareholder-wealth-maximization hypothesis as I had first asserted, but they did fit nicely into the managerial framework that I had come to adopt. Although the neoclassical cost of capital invariably outperformed cash flow in Jorgenson's articles, J. Walter Elliot (1973) in a comment on one of Jorgenson's papers [Siebert (1968)] using a sample of 184 firms rather than just the 15 Jorgenson had used, and estimating both cross-section and time series equations, found that cash flow was the best of four explanatory variables. The neoclassical cost of capital ranked third or fourth. Grabowski and I (1972) also found that cash flow was superior to two different measures of the neoclassical cost of capital in a simultaneous equations model of firm expenditures.

Thus, a pattern of empirical results was visible in the 1970s that was fully consistent with a managerial discretion/size-growth maximization hypothesis about the corporation. The greater a corporation's cash flow, the more it spent on capital equipment and R&D; reinvested cash flows earned relatively low rates of return; mature corporations earned lower returns than young companies or those with new technologies; the market priced the shares of mature firms in a way that implied a preference for greater dividends and less reinvested cash flows. At the same time evidence was accumulating to suggest that the conglomerate merger wave of the 1960s had reduced corporate efficiency. The wealth of the shareholders of acquiring firms steadily declined relative to other shareholders as the market learned more and more about the conglomerate mergers.[11]

But this pattern of evidence either went unnoticed or, if it was discerned, failed to dislodge the view that managers maximized profits or shareholder wealth. The managerial theories joined the mark-up pricing models that had preceded them, as valiant but futile attempts to replace the simplistic view of managerial decisionmaking that characterized the neoclassical model of the firm. It should be noted, that this outcome is not peculiar to the field of industrial organization. Robert Frank (1985) focuses on the inadequacy of neoclassical theory in explaining wage patterns within firms, but notes also, citing Mayer (1972), that 'the evidence for [a] relationship [between income and savings] is so strong and so consistent that it would appear difficult for proponents of the permanent income and life-cycle [saving] theories to continue to insist that savings rates are unrelated to income. Yet these claims persist in all major undergraduate and graduate texts in macroeconomics' [Frank (1985, p. 160)]. Thus despite a broad consensus among economists that hypotheses should be formulated in such a way so that they can be 'rejected by the data', no empirical evidence is ever deemed strong enough to reject a hypothesis that assumes that agents maximize one of the standard

[11]See the extensive references in my surveys [Mueller (1977, 1987, pp. 51–73)].

11

behavioral objectives, i.e., in the case of the firm profits or shareholder wealth.

4. The principal–agent model

While the empirical side of the managerialist challenge to neoclassical orthodoxy was playing itself out in the journals during the 1970s, a development in economic theory of great significance occurred. The principal–agent model was born, and a vast literature was launched. I, personally, consider this one of the important contributions to economics in the past generation. When properly applied it is a most useful analytical tool. The somewhat critical remarks I shall make concern its applicability and occasional misuse in analyzing the corporation and are not intended as general criticisms of the model and those who have developed it.

When first considering the model, it is difficult *not* to think of it as describing the ownership/control dichotomy that concerned Berle and Means. But unlike their work and the rest of the literature discussed here, the principal/agent model did not arise from observations about how the relationship between shareholder and manager evolves in real world situations. Instead the model is developed under abstract assumptions familiar to all. The principal is concerned only with his wealth or the utility of his wealth. The agent with his wealth and the disutility of the effort expended on behalf of the principal. One or both may be risk averse. The principal cannot fully monitor the agent and thus must try to induce the agent to maximize the principal's wealth or utility by incorporating the proper incentives into the employment contract.

An example of a firm that does fit the model is a typical U.S. baseball team. These teams are often owned by a single person, or small group who do literally hire someone to manage the team for them. Although all aspects of managerial effort cannot be monitored, good proxies exist, e.g., the number of games the team wins. Incentive clauses based on number of wins can be devised, and the threat of terminating the contract can also serve as an important incentive, since the owners of other teams can also easily observe the number of wins by the team. Some combination of contract incentives and the threat of contract termination with a competitive market for managers can be expected to 'solve' the principal/agent problem to the principal's benefit.

Now consider the problem as it manifests itself in the large corporation. The shareholder wants the manager to control costs and set prices to maximize profits, and to invest to the point where the marginal return on investment equal's the return the shareholder can earn on investments of comparable risk elsewhere, returning the residual cash flow to the shareholder as dividends or through stock repurchases. The shareholder does not

possess information to monitor costs and investment returns, however. He faces the twin dangers that costs like managerial perks are not controlled, and thus that reported profits are less than they could potentially be, or that less of those profits that are declared are returned to the shareholder as managers pursue excessive growth. *If* the shareholder were to design an incentive contract to mitigate this principal/agent problem, he would tie managerial compensation to some combination of reported profits and share price, as perhaps with stock bonuses. He would also retain the authority to fire the manager for poor performance, for example, by giving managers only *nonvoting* shares.

Are these the contracts we observe? No. Managerial compensation is only weakly tied to shareholder wealth [Marris (1964, ch. 2); Jensen and Murphy (1990)]. Nonvoting shares if they are issued go to the shareholders. Increasingly popular among managers in recent years have been multiple-vote, super shares. Why the discrepancy between theory and fact? It arises because the fundamental premise of the principal/agent model, in the case of the shareholder and manager, is false. Shareholders do not write the contracts that define managerial compensation, do not hire and fire managers. To a considerable degree managers select themselves and design their own contracts [Vancil (1987)]. The point Berle and Means sought to make is that *control* of the 'contract' linking managers to shareholders lies with the managers. With respect to the modern corporation, the principal/agent model is a useful *normative* theory. It describes the contracts that ought to exist, if shareholders were to write them. Since it is the agents themselves who write the contracts in most instances, however, the principal/agent model has limited usefulness in describing the actual contracts observed.

5. The methodology of modern economics

Samuelson has called economics 'the queen of the social sciences', and most economics are quick to don the queen's mantle when we meet colleagues from the other social sciences. One can identify five components to the economist's scientific practice. (1) A behavioral assumption about the basic goals of the actor, e.g., profits, utility, wealth. (2) Some auxiliary assumptions particular to the problem at hand, e.g., the actor is risk averse, the actor lives two periods, asymmetric information. (3) The rule the actor employs to achieve his goal, e.g., optimization. (4) The constraints faced by the actor, e.g., markets are in competitive equilibrium. From these four elements predictions are drawn. Step (5) is to confront these predictions with data and see how well they explain them.

Now one might expect that scientific progress would consist of advances in all five of these components. But economics has focussed on the last four.

Dramatic increases in sophistication in optimization methods, in the specification of equilibrium conditions, and econometric techniques have taken place. The behavioral foundations of the discipline have been largely neglected, however. Indeed, in economics good scientific method today virtually *requires* that one start with one of the small set of naive assumptions about individual motivation that the profession holds dear. Upon this slenderest of behavioral foundations intricate optimization techniques and equilibrium conditions are then heaped. The strength of a theory varies directly with the simplicity of its behavioral assumptions, *and* the complexity of the edifice built upon them. Any model that does not choose one of the standard objectives assumed for agents is dismissed as *ad hoc*. Only if the predictions of a model are not supported by the data, is its realism examined. At this juncture auxiliary assumptions are added sufficient to make the model 'work'.[12] The consequence of this disjoint development of economic methodology has been that we have made less rapid progress in understanding and predicting economic events than we could have.

To illustrate let me return to the conglomerate merger wave of the 1960s. One might have expected the economist's initial reaction to conglomerate mergers to have been one of extreme skepticism. It was difficult to see how they could produce large increases in market power, and it hardly seemed likely that the efficiencies claimed for them – often very vague – could overcome the transaction costs and organizational inefficiencies in absorbing the acquired companies *and* the 20 percent or so premia paid to their shareholders. Even the risk-spreading gains from diversification could be more cheaply achieved by shareholders in other ways. Yet, echoing the neoclassical economists of the turn of the century, who regarded the trusts as a consequence of technical progress [Letwin (1965, p. 73)], the economist of the 1960s proclaimed a 'revolution in managerial technology' and extolled the social benefits from conglomeratization as loudly as the managers of the conglomerates themselves.

It is tempting to speculate on whether it was the economist's methodology or his ideology that produced this unquestioning attitude toward the corporation. His methodology certainly pointed in that direction. If managers maximize shareholder wealth, and markets work – the prevailing assumptions in economic modeling 20 years ago as today – and if market power increases cannot be easily discerned, then mergers must promise efficiency increases. They must be present whether one sees them or not. Hence the invention of the word 'synergy' to designate efficiencies we can neither observe nor describe. But the commitment to *laissez faire* that

[12]Psychologists daring to critique the behavioral assumptions underlying rational actor models have been confronted with such a dazzling array of auxiliary assumptions that they might well regard the exercise as futile. See Tversky and Kahneman (1987, pp. 89–91).

14

economics instills, also generates a conservative stance with respect to the corporation. Leave managers alone and social welfare will be maximized.

Faith in the invisible hand of a *laissez faire* market system often leads economists to adopt an almost Darwinian attitude toward the competitive process. The goals of individual actors are not important for predicting outcomes of the competitive process, since only efficient corporate sizes and structures will survive [Alchian (1950; Stigler (1958)].[13] By this criterion the organizations created by the conglomerate merger wave were highly inefficient. Plant sizes peaked in the 1960s merger wave and then declined by roughly 50 percent [Davis and Haltiwanger (1991, fig. 4A, B)]. The merger wave of the 1960s was followed by a spin-off wave in the 1970s, as conglomerate companies, sometimes facing bankruptcy, unloaded acquired units with falling profits and productivity [Ravenscraft and Sherer (1987); Lichtenberg and Siegel (1987)].[14] In the 1970s the U.S. economy experienced its worst economic performance since the 1930s. Productivity declined, and one market after another was lost to foreign competition. Could the poor performance of U.S. corporations in this decade be explained in part by the mergers of the previous decade that created inefficient organizational structures? Might not the Great Depression have been longer and deeper, because it followed a merger wave fed by stock market speculation that may too have created inefficient corporate structures? Questions an economist should be able to answer, but questions the economist does not even ask, since in his models an inefficient organizational unit could never even be created.

Indeed, as late as 1983 Michael Jensen and Richard Ruback would survey the 'scientific evidence' on mergers, i.e. event studies, and conclude that they

[13]One might question how, if corporations are so inefficient, they have succeeded in growing so large during the twentieth century? This question is too broad to answer in a single paper, let alone in a footnote. A full answer would, however, emphasize the importance of first-mover advantages, market power, and the corporate firm's life cycle.

At a firm's inception it faces significant uncertainties and high probabilities of failure. Its owners and suppliers of capital may recognize that, if the firm succeeds it may grow to be very large, ownership and control will become separate, and the wealth of the shareholders will be lower than its potential value. But these events are decades away and their financial consequences are dwarfed by the uncertainties over whether the firm will survive.

If the firm does survive and the innovation or ideas that spawned it are important, the wealth gains to the initial suppliers of capital are likely to be huge. Should managerial discretion problems later arise, the initial suppliers can sell their shares and still have earned a substantial above normal return. If the extent of the managerial discretion problem is realized, subsequent generations of shareholders need not suffer losses, since share prices at the time they buy them will reflect the loss in potential profits or dividends due to managerial discretion. The first mover advantages in market power and learning-by-doing efficiencies may allow the firm to survive despite the management's failure to maximize shareholder wealth.

If more efficient firms increase market share and less efficient ones lose it, then the United States Steel company was a less efficient firm from its creation in 1901 through a merger that consolidated 180 plants to the present day. Yet it has survived.

[14]See also Caves and Barton (1990) and Lichtenberg and Siegell (1990), who report that plants owned by diversified firms have lower productivity than other plants.

increased shareholder wealth, and that these increases could not be attributed to increases in market power, and thus presumably signified increases in corporate efficiency. They could find no evidence that 'managerial actions related to corporate control ... harm stockholders' (p. 47). Event studies assume that acquiring company managers maximize shareholder wealth, that the stock market is efficient, and thus that all wealth changes *caused* by a merger occur *at the time of the merger*, and are measured by changes in shareholder wealth at that time. So strong is the belief that managers maximize shareholder wealth and in stock market efficiency, that the 'scientific approach' can ignore evidence that acquiring firm share prices, acquired firm profits, and acquired firm market shares all decline following mergers [Mueller (1977, 1987); Caves (1989)]. The studies Jensen and Ruback surveyed were mostly written in the 1970s, and included mergers from the 1960s. Thus, the event studies also implied that the congiomerate mergers of the 1960s increased economic efficiency.

Influenced no doubt by Jensen and Ruback's survey, Ronald Reagan's Council of Economic Advisors would conclude in 1985, that 'The evidence is strong that takeovers generate aggregate net benefits to the economy' (1985, p. 198). Reagan's Secretary of Commerce would in turn request that Section 7 of the Clayton Act be repealed so that still more mergers would be possible. Despite the fact that our corporations were much larger than Japanese firms on average at the end of the 1960s, and undertook 10 mergers for every one in Japan, the loss in markets that followed was attributed to there being too few mergers in the United States, and our corporations being too small.

6. A sampling of current research

To bring my chronology of research on the corporation up to date, I have examined four recent issues of the three leading *IO* journals – the *IJIO*, the *JIE*, and the *Rand* – for evidence related to the questions I have posed. Not surprisingly, the bulk of the articles in all three journals were theoretical, and when the behavior of firms was involved, profits maximization was (sometimes implicitly) assumed. The March 1991 issue of the *JIE* contains five articles relevant to my theme, however. The first examines survey data for the kind of asymmetric anticipations of rivals' responses to price changes that implies a kinked-demand schedule, and finds them [Bhaskar et al. (1991)]. The authors do not interpret this finding as support for the behavior assumed by either Sweezy or Hall and Hitch, however, because the earlier theory suffered from the 'major weakness ... that responses of rivals are not derived from any form of maximizing behaviour, and hence are *ad hoc*' (p. 242). In the intervening years models have appeared in which the kind of behavior Sweezy, Hall and Hitch described constitutes a perfect equilibrium

for profit maximizing firms with the addition of some auxiliary assumptions. It is this explanation that the authors favor.

The second paper seeks to explain price and capacity decisions in the North American newsprint industry [Booth et al. (1991)]. The authors' knowledge of the industry leads them to propose a full cost or mark-up price model, and the data support this formulation rejecting a marginal-cost pricing model.

Slusky and Caves (1991) examine the determinants of premia paid in mergers. They find no evidence of synergies from the mergers, but do find that the size of premia are affected by the nature of the relationship between shareholders and managers.

In the fourth article, a direct test of the profits maximization hypothesis is attempted using ticket price data for National Hockey League teams [Ferguson et al. (1991)]. The authors claim support for the profits maximization hypothesis, although one of the auxiliary assumptions they make is that all costs are fixed (p. 299). Thus profits and revenue maximization are equivalent in their model, and the data would not reject Baumol's alternative to profits maximization. The fifth paper introduces managerial slack into a profits maximization model of public owned firms [Fraja (1991)].

Three papers from recent issues of the *Rand Journal* are related to my argument. Rose and Joskow (1990) use a hazard rate model to explain innovation diffusion in electric power generation. Stochastic models of this sort are commonly employed in studies of innovation and innovation diffusion. They presume neither profits maximizing behavior on the part of actors nor market equilibria Bagwell's (1991) study of stock repurchases raises at least the possibility that managers are protecting their own interests rather than their shareholders by increasing the costs of a hostile takeover.

Salant and Goodstein (1990) begin from some real world observations of committees, the Genossenschaften for the Rhine River in Germany and agricultural cartels, institutions that failed to select the core of a voting game. This contradiction of the usual rational, self-interest behavioral postulate led the authors to conduct some classroom experiments with two other voting procedures. The subjects failed to choose the core in the first set of experiments, and again in a modified second set. But when the rationality assumption was weakened by adding the auxiliary assumption of threshold effects, the modified model did predict the outcomes fairly well.

Motivational issues came up in only two papers in the four *IJIO*s that I examined. Estrin and Perotin (1991) brought in other managerial objectives in their discussion of the publicly owned firm. Mizuno and Odagiri (1990) chose a satisficing model as the best way to simulate advertising decisions of corporations as they had observed them.

What is interesting about this sampling of recent IO papers is that, alternatives to profits and shareholder wealth maximization, or to strong

rationality assumptions are considered only by those directly engaged in empirical work, or by those trying to relate their model to particular phenomena observed in the real world. The inadequacies of the simplistic motivational assumptions upon which much economic modeling builds become apparent to those who seek to explain the complexities of actual human behavior and institutions.

7. Implications and conclusions

Alas the real world does refuse to conform to our models. Thus the economist of today is forced to reconsider the economic regularities observed by his predecessors. Recently several papers have appeared that attempt to explain the cyclical movement of prices, and relate it to such factors as industry concentration; cash flow models of investment are again in vogue; the managerial-life-cycle model of the firm has been reincarnated and rechristened as 'the free cash flow' theory [Jensen (1986)]. The new versions of these models often differ from their predecessors in that they assume profits or shareholder wealth maximization, thus avoiding the dreaded label *ad hoc*, and rely on auxiliary assumptions to produce deviations from the simplified version of the neoclassical model. There is nothing inherently wrong with this practice, so long as the model does explain the data well. But if the assumed motivation and auxiliary assumptions do not correspond closely to the actors and institutions they purport to explain, the danger exists that the model will fail to explain other data, or that erroneous implications will be drawn from the results.

Let me illustrate this point. Internal cash flows can be rationalized as a determinant of investment for a firm whose managers maximize shareholder wealth by assuming that potential suppliers of external capital cannot learn what the expected return on a corporation's investment is. The managers know that the expected returns on investment exceed the returns their shareholders can earn on investments of comparable risk, i.e. the neoclassical cost of capital, but confront a skeptical capital market that pays less for new equity than its true value. The firm effectively faces a steep rise in the cost of capital when it exhausts its cash flow. It thus invests from internal cash flows, and investment varies over time and across firms with the level of cash flows.

The managerial-growth-maximization hypothesis also assumes that there is a steep rise in a firm's cost of capital when it goes outside for funds, but the rise occurs in this model because the managers can use internal funds at their discretion, and thus implicitly place a low opportunity cost on them [Mueller (1969, 1972)]. If they are not invested they must be 'given away' to the shareholders. Asymmetric information may also exist, but it need not arise because the external capital market could not evaluate information on

investment opportunities, if it had it. Rather it may not have information on the firm's investment opportunities, because the managers choose to conceal this information, since to reveal it would reveal that they are investing at rates of return far below shareholder opportunity costs.

Both hypotheses predict that managers that have greater cash flows spend them. But in the asymmetric-information-neoclassical model, the funds are invested at rates of return above shareholder opportunity costs, in the managerial model at returns below opportunity cost. If the behavioral and institutional assumptions underlying the asymmetric information hypothesis are correct, society would benefit from giving these cash starved managers greater access to internal fund flows – lower corporate income taxes, and investment tax credits are called for. If the managerial hypothesis is correct control over internal cash flows should be taken away from managers, as say through a tax on undistributed profits. The differences between the two models are large and the implications for social welfare could be great. Elizabeth Reardon and I (1990) have estimated cumulative rates of return on investment for 699 large corporations from 1970 through 1988. For 8 out of 10 the estimated return on investment was less than shareholder opportunity costs. Thus 8 out of 10 conform to the predictions of the managerial model.[15] A policy that erroneously gave managers more cash to invest without the discipline of the external capital market could, according to our results, substantially worsen the performance of the U.S. economy. The choice of starting assumptions for a model does make a difference.

My message is not new. The *Methodenstreit* among economists from Continental Europe at the end of the 19th century was between those seeking to retain a broad behavioral base and inductive methodology for economics and the neoclassical economists, who advocated a deductive approach using the newly invented marginal analysis. In 1952 Andreas Papandreau pleaded for a more general preference function for the firm than just profits (p. 211). Richard Nelson and Sidney Winter (1982) have recommended abandoning both profits maximization, indeed maximization modeling of any kind, and market equilibria as assumptions in economic modeling because of their lack of realism. Directly or indirectly, Hall, Hitch, Lester and all of the others whose work I have cited were arguing for the same methodological changes. My brief survey of the current literature indicates that the message has not gone totally unheard. But those who build from a broader behavioral base are in a distinct and perhaps shrinking

[15]We measure returns using market value data. If the capital market cannot evaluate investment opportunities, and consistently underestimates returns, then market value may not rise as much as it should when an investment is made. Our method will underestimate returns when the investments occur. But *ex post* the market should discover what the true returns are as the investment pays off. Since we cumulate market value changes and investment over 19 years much of what we estimate is the combined reaction of the market to an investment both at the time it is made, and afterward.

minority among IO economists. Why then have I bothered to repeat this sermon that so many others have given before me?

Old dogs do not learn new tricks, but young ones can. Any IO economist who is more than a couple of years past his formal training and has published his first article or two has probably already adopted the methodology that he will use throughout his career, and in all likelihood it is the one I have criticized. What I have said may insult, irritate or amuse him, but it will not change the way he goes about doing his research. I speak not to him, but rather to the economist to be, to the economist just starting her career, who seeks to understand the behavior of individuals and institutions in the world around her. Let me illustrate my point with one last example.

Suppose a member of *EARIE* not from Japan were commissioned by a large foundation to do a study of the Japanese corporation in the hopes of gaining an understanding of why it is that it seems to do so well in competition with corporations from North America and Europe. The grant was generous and allowed the purchase of all available data on Japanese corporations, research assistant help, travel and the like. How should our *EARIE* member proceed?

Consider the following alternatives. He constructs a model in which managers maximize shareholder wealth, shareholders are risk averse and hold portfolios of company shares, employees in the firm maximize utility, which is a function of income and effort, and so on. In short he should construct the same model he would construct to study corporations in North America or Europe. If it turned out that the model did not explain the data well, auxiliary assumptions could be added until it did.

Alternatively one could begin by going to Japan and studying Japanese corporations, Japanese managers, and the institutional environment in which they operate. One could read some of the autobiographies of Japanese managers, interview those currently at the top, study the organizational charts, procedures for promotion, and implicit contracts within the corporation; study the nexus of markets in which the corporation raises capital, sells its products, hires labor, and buys inputs; study the relative importance of status and wealth in Japanese society, and upon what status depends. Although one should not hope to understand fully the 'inscrutable Japanese', one might be able to acquire sufficient knowledge to construct a good model. Nor would it necessarily have to be a complex, multi-objective model.[16] All models must abstract from some elements of reality. The skilled modeler recognizes the essential elements in the situation he is studying, and includes them with as much economy as possible. But recognizing what is

[16]Hiroyuki Odagiri (1981, 1991) believes that the long run nature of the manager/corporation employment contract is such as to allow one to model many decisions under the assumption that Japanese managers maximize the growth of their company. Aoki (1984) has also attempted to model corporate decisionmaking building on a richer behavioral base.

essential requires some study of the situation being modeled. All essential elements cannot be identified a priori.

Today's economist protests that the latter research strategy would make the model *ad hoc*, i.e., suitable only for the special purpose of explaining the Japanese corporation. I agree. But that was of course what our *EARIE* colleague would have been commissioned to do. Moreover, the first strategy would undoubtedly wind up with a model that was also *ad hoc* in that several auxiliary assumptions will have been added to produce a better fit to the data. Almost all models of complex behavior that try and account for observed patterns of data are *ad hoc*. The difference between the two strategies comes in whether the assumptions needed to get the model to fit the data are added after one has estimated the pristine model and found it sorrily lacking, or whether one begins with a set of assumptions about the motives of the actors and the environment in which they act that one believes captures the elements essential to understand it.

Lest I be misunderstood, let me emphasize that I am *not* arguing against the use of simplifying behavioral assumptions and analytic techniques. Occam's razor remains an important instrument for achieving the economist's pedagogic goals – when used judiciously. Assuming that managers maximize profits, that markets are in equilibrium, etc., may prove to be the best way to analyze and describe *certain* economic questions. A management, which chooses its investment strategy to maximize the growth of the firm, would, for example, optimally set prices to maximize short run profits [Williamson (1966)]. The task faced by the economist is to determine what simplifying assumptions are appropriate for the analysis of a particular question. This determination can only be made after one observes the behavior of individuals in the context of the particular problem being investigated.

Nor do I wish to denigrate the use of sophisticated modeling and econometric hypothesis testing. Instead, I am advocating that these techniques be combined with a richer set of behavioral assumptions about the individuals whose actions are being explained, where such assumptions are appropriate. Modeling corporate managers' decisions regarding investment, mergers and the growth of the firm is an important exampl of where alternative behavioral assumptions will have high payoffs.

I have dwelled on the glass being 9/10 empty. But one might be optimistic that the glass is even 1/10 full, and its level *might* be rising. In recent years psychologists have intruded into the pages of our most prestigious journals with experimental evidence that strikes at the behavioral foundations of our discipline. Psychology has been drawn upon to explain fads in buying and stock market cycles, the distribution of wages in firms, and still other economic phenomena that seem to be poorly accounted for by the standard behavioral assumptions employed in economics. Michael Jensen, who in 1983

would request more 'knowledge of this enormously productive social invention: the corporation' (with Ruback, p. 47), has in 1989, presumably having acquired the requested knowledge, predicted that the inefficiencies of the corporation with ownership and control separated are so significant that it is destined to disappear entirely. There is cause for hope. If the economist is willing to supplement his powerful and highly developed analytical tools with a broader behavioral foundation, and then turn his attention to the corporation, he may yet be able to acquire a sufficient understanding of this complex institution to allow him to recommend policies that will both preserve it and curb its inefficiencies and social costs, or barring this to help craft those institutions that will replace it. A research agenda that should be challenging enough for any young economist.

References

Alchian, Armen, A., 1950, Uncertainty, evolution, and economic theory, Journal of Political Economy 58, 211–221.

Aoki, Masahiko, 1984, The co-operative game theory of the firm (Clarendon Press, Oxford).

Bagwell, Laurie Simon, 1991, Share repurchase and takeover deterrence, The Rand Journal of Economics 22, Spring, 72–88.

Baldwin, William L., 1964, The motives of managers, environmental restraints, and the theory of managerial enterprise, Quarterly Journal of Economics 78, May, 236–258.

Baumol, William J., 1959, Business behavior, value and growth (Macmillan, New York) second edition, 1967.

Baumol, William J., P. Hiem, B.G. Malkiel and R.E. Quandt, 1970, Earnings retention, new capital and the growth of the firm, Review of Economics and Statistics 52, Nov., 345–355.

Becker, Gary S., 1962, Irrational behavior and economic theory, Journal of Political Economy 70, Feb., 1–13.

Berle, Adolf A. and Gardner C. Mearns, 1932, The modern corporation and private property (Commerce Clearing House, New York); rev. ed. (Harcourt, Brace, Jovanovich, New York), 1968.

Bhaskar, V., S. Machin, and G. Reid, 1991, Testing a model of the kinked demand curve, The Journal of Industrial Economics 39, March, 241–254.

Booth, D.L., V. Kanetkar, I. Vertinsky and D. Whistler, 1991, An empirical model of capacity expansion and pricing in an oligopoly with barometric price leadership: A case study of the newsprint industry in North America, The Journal of Industrial Economics 39, Mar., 255–276.

Carlton, Dennis W., 1986, The rigidity of prices, American Economic Review 76, Sept., 637–658.

Caves, Richard E., 1989, Mergers, takeovers and economic efficiency: Foresight vs. hindsight, International Journal of Industrial Organization 7, Mar., 151–174.

Caves, Richard E. and David R. Barton, 1990, Efficiency in U.S. manufacturing industries (MIT Press, Cambridge, MA).

Council of Economic Advisers, 1985, Annual report (Government Printing Office, Washington, DC).

Cyert, Richard M. and James G. March, 1963, A behavioral theory of the firm (Prentice-Hall, Englewood Cliffs, NJ).

Davis, Steve J. and John Haltiwanger, 1991, Wage dispersion between and within U.S. manufacturing plants, 1963–1986, Brookings Papers on Economics, Microeconomics.

Elliot, J.W., 1973, Theories of corporate investment behavior: Revisited, American Economic Review 63, Mar., 195–207.

Estrin, Saul and Virginia Perotin, 1991, Does ownership always matter? International Journal of Industrial Organization 9, Mar., 55–72.

Ferguson, D.G., Kenneth G. Stewart, J.C.H. Jones, and Andre le Dressay, 1991, The pricing of sports events: Do teams maximize profit? The Journal of Industrial Economics 39, Mar., 297–310.

Frank, Robert H., 1985, Choosing the right pond (Oxford University Press, Oxford).

Fraja, Giovanni de, 1991, Efficiency and privatization in imperfectly competitive industries, The Journal of Industrial Economics 39, Mar., 311–321.

Friedman, Milton, 1953, The methodology of positive economics, in his essays in positive economics (University of Chicago Press, Chicago, IL) 3–43.

Galbraith, John K., 1961, The great crash, 1929 (Houghton Mifflin, Boston, MA), third edition, 1972.

Grabowski, Henry G. and Dennis C. Mueller, 1972, Managerial and stockholder welfare models of firm expenditures, Review of Economics and Statistics 54, Feb., 9–24.

Grabowski, Henry G. and Dennis C. Mueller, 1975, Life-cycle effects on corporate returns on retentions, Review of Economics and Statistics 57, Nov., 400–409.

Hall, R.L. and C.J. Hitch, 1939, The theory of business behavior, Oxford Economic Papers, May, 12–45.

Jensen, Michael C., 1986, Agency costs of free cash flow, corporate finance and takeovers, American Economic Review 76, May, 323–329.

Jensen, Michael C., 1989, Eclipse of the public corporation, Harvard Business Review, Sept.–Oct., 61–74.

Jensen, Michael C. and Richard S. Ruback, 1983, The market for corporate control, Journal of Financial Economics 11, Apr., 5–50.

Jensen, Michael C. and Kevin J. Murphy, 1990, Performance pay and top-management incentives, Journal of Political Economy 98, 225–264.

Jensen, Michael C. and Richard S. Ruback, 1983, The market for corporate control, Journal of Financial Economics 11, April, 5–50.

Jorgenson, D.W. and C.D. Siebert, 1968, A comparison of alternative theories of corporate investment behavior, American Economic Review 58, Sept., 681–712.

Journal of Law and Economics, 1983, Special Issue, June.

Kahn, Alfred E., 1959, Pricing objectives in large corporations: Comment, American Economic Review 49, 670–680.

Kaplan, A.D.H., Joel D. Dirlam and Robert F. Lanzillotti, 1958, Pricing in big business (Brookings Institution, Washington, DC).

Kuh, E. and J.R. Meyer, 1957, The investment decision (Harvard University Press, Cambridge, MA).

Lester, Richard A., 1946, Shortcomings of marginal analysis for wage–employment problems, American Economic Review 36, 63–82.

Letwin, William, 1965, Law and economic policy in America (Random House, New York).

Lichtenberg, Frank R. and Donald Siegel, 1987, Productivity and changes in ownership of manufacturing plants, Brookings Papers on Economic Activity, 643–673.

Lichtenberg, Frank R. and Donald Siegel, 1990, The effect of ownership changes the employment and wages of central office and other personnel, Journal of Law and Economics 33, Oct., 383–408.

Machlup, Fritz, 1946, Marginal analysis and empirical research, American Economic Review 36, Sept., 519–554.

Machlup, Fritz, 1947, Rejoinder to an antimarginalist, American Economic Review 37, 148–154.

Machlup, Fritz, 1967, Theories of the firm: Marginalist, behavioral, managerial, American Economic Review 57, Mar., 1–33.

Marris, Robin, 1963, A model of the 'managerial' enterprise, Quarterly Journal of Economics 77, May, 185–209.

Marris, Robin, 1964, The economic theory of managerial capitalism (Free Press, Glencoe).

Marshall, Alfred, 1890, Principles of economics (London); 8th edition (Macmillan, New York) 1920.

Marshall, Alfred, 1923, Industry and trade (Macmillan, London), 4th ed.

Mayer, Thomas, 1972, Permanent income, wealth and consumption (University of California Press, Berkeley, CA).

Means, Gardiner C., 1935, Industrial prices and their relative inflexibility (Secretary of Agriculture, Washington, DC).
Means, Gardiner C., 1972, The administered price thesis reconfirmed, American Economic Review 62, June, 292–306.
Mill, John Stuart, 1885, Principles of political economy, 9th edition (Longmans, Green and Co., London).
Mizuno, Makoto and Hiroyuki Odagiri, 1990, Does advertising mislead consumers to buy low-quality products? International Journal of Industrial Economics 8, 545–558.
Mueller, Dennis C., 1967, The firm decision process: An econometric investigation, Quarterly Journal of Economics 81, Feb., 58–87.
Mueller, Dennis C., 1969, A theory of conglomerate mergers, Quarterly Journal of Economics 83, Nov., 643–659.
Mueller, Dennis C., 1972, A life cycle theory of the firm, Journal of Industrial Economics 20, July, 199–219.
Mueller, Dennis C., 1977, The effects of conglomerate mergers: A survey of the empirical evidence, Journal of Banking and Finance, Dec., 315–347.
Mueller, Dennis C., 1987, The corporation: Growth, diversification and mergers (Harwood Academic Publishers, London).
Mueller, Dennis C. and Elizabeth A. Reardon, 1990, Rates of return on corporate investment, University of Maryland, Working paper, no. 90-10.
Nelson, Richard R. and Sidney G. Winter, 1982, An evolutionary theory of economic change (Harvard University, Cambridge, MA).
North, Douglass C., 1983, Comment on Stigler and Friedland: The literature of economics: The case of Berle and Means, Journal of Law and Economics 26, 269–271.
Odagiri, Hiroyuki, 1981, The theory of growth in a corporate economy: An inquiry into management preference, R&D and economic growth (Cambridge University Press, Cambridge).
Odagiri, Hiroyuki, 1991, Growth through competition, competition through growth: Strategic management and the economy in Japan (Oxford University Press, Oxford).
Papandreou, Andreas G., 1952, Some basic problems in the theory of the firm, in: B.F. Haley, ed., A survey of contemporary economics, Vol. 2 (Irwin, Chicago, IL) 183–222.
Peterson, Shorey, 1965, Corporate control and capitalism, Quarterly Journal of Economics 79, Feb., 1–24.
Primeaux, W.J. Jr. and Mark R. Bomball, 1974, A reexamination of the kinky oligopoly demand curve, Journal of Political Economy 82, 851–862.
Primeaux, W.J. Jr. and Mickey C. Smith, 1976, Pricing patterns and the kinky demand curve, Journal of Law and Economics 19, 189–199.
Ravenscraft, David J. and F.M. Scherer, 1987, Mergers and managerial performance, in: John C. Coffee, Jr., Louis Lowenstein and Susan Rose-Ackerman, eds., Takeovers and contests for corporate control (Oxford University Press, Oxford).
Romano, Roberta, 1987, The political economy of takeover statutes, Virginia Law Review 73, 111–199.
Rose, Nancy L. and Paul L. Joskow, 1990, The diffusion of new technologies: Evidence from the electric utility industry, The Rand Journal of Economics 21, Autumn, 354–373.
Salant, Stephen W. and Eban Goodstein, 1990, Predicting committee behavior in majority rule voting experiments, The Rand Journal of Economics 21, Summer, 293–313.
Scherer, Frederic M., 1930, Industrial market structure and economic performance, second edition (Rand McNally, Chicago, IL).
Simon, Herbert A., 1957, The compensation of executives, Sociometry 20, Mar., 32–35.
Simon, Herbert A., 1959, Theories of decision making in economics and behavioral science, American Economic Review 49, June, 253–283.
Simon, Julian L., 1969, A further test of the kinky oligopoly demand curve, American Economic Review 59, 971–975.
Slusky, Alexander S. and Richard E. Caves, 1991, Synergy, agency, and the determinants of premia paid in mergers, The Journal of Industrial Economics 39, Mar., 277–296.
Smith, Adam, 1937, Wealth of nations (Random House, New York).

24

Stigler, George J., 1947, The kinky oligopoly demand curve and rigid prices, Journal of Political Economy 55, 432–449, reprinted in: G.J. Stigler and K.E. Boulding, eds., 1952, Readings in price theory (Irwin, Chicago, IL) 410–439.

Stigler, George J., 1950, Monopoly and oligopoly by merger, American Economic Review 40, May, 23–34, reprinted in: R.B. Heflebower and G.W. Stocking, eds., Readings in industrial organization and public policy (Irwin, Homewood, IL) 1958, 69–80.

Stigler, George J., 1958, The economies of scale, Journal of Law and Economics 1, 54–71.

Stigler, George J. and James K. Kindahl, 1970, The behavior of industrial prices (Columbia University Press, New York).

Stigler, George J. and James K. Kindahl, 1973, Industrial prices, as administered by Dr. Means, American Economic Review 63, 717–721.

Sweezy, Paul M., 1939, Demand conditions of oligopoly, Journal of Political Economy 47, 568–573.

Tversky, Amos and Daniel Kahneman, 1987, Rational choice and the framing of decisions, in: R.B. Hogarth and M.W. Reder, eds., Rational choice (University of Chicago Press, Chicago) 67–94.

Vancil, Richard F., 1987, Passing the baton (Harvard Business School, Boston, MA).

Weiss, Leonard W., 1977, Stigler, Kindahl, and Means on 'administered prices', American Economic Review 67, Sept., 610–619.

Williamson, John, 1966, Profit, growth and sales maximization, Economica 33, Feb., 1–16.

Williamson, Oliver E., 1963, Management discretion and business behavior, American Economic Review 53, Dec., 1032–1057.

Williamson, Oliver E., 1985, The economic institutions of capitalism (Free Press, New York).