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Source: *Strategic Management Journal*, Winter, 1991, Vol. 12, Special Issue: Fundamental Research Issues in Strategy and Economics (Winter, 1991), pp. 61-74

Published by: Wiley

Stable URL: <https://www.jstor.org/stable/2486434>

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WHY DO FIRMS DIFFER, AND HOW DOES IT MATTER?

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In virtually all economic analyses, differences among firms in the same line of business are repressed, or assumed to reflect differences in the market environments that they face. In contrast, for students of business management and strategy, firm differences are at the heart of their inquiry. This paper explores the reasons behind this stark difference in viewpoint. It argues that economists really ought to recognize firm differences explicitly.

INTRODUCTION

This paper is concerned with the sources and significance of interfirm differences, from the viewpoint of an economist. How might an economist's perspective on this differ, say, from that of a student of business management? I would argue that the most important difference is that economists tend to see firms as players in a multi actor economic game, and their interest is in the game and its outcomes, rather than in the particular play or performance of individual firms. That is, economists are interested in how the automobile industry works, and its performance in various dimensions, and not in General Motors or Toyota *per se*, but only insofar as the particularities of these firms influence the industry more broadly. This perspective is quite different, it seems to me, than that of a student of management who is concerned with the behavior and performance of individual firms in their own right.

My objective in this essay is to make a strong case for the economic significance, in the sense above, of discretionary firm differences. My

position certainly has been influenced by the work of scholars of firm management who have persuasively documented significant differences among firms in an industry in behavior and performance, and proposed that these differences largely reflect different choices made by firms. However, because the interests of those authors have differed from the interests of economists, almost no attention has been paid to the industry or economy wide implications of such different choices. Thus while the management literature provides a start for my argument, there is much that I need to build myself, in cooperation with like thinking friends.

It should be recognized that, in trying to make a case for the economic significance of discretionary firm differences, I and my co-arguers are fighting against a strong tide in economics, particularly in theoretical economics, that downplays or even denies the importance of such differences. The argument in economics is not that firms are all alike; economists recognize that computer firms differ from textile firms, and in both industries, German firms almost certainly differ from Taiwanese firms. Rather, the position is that the differences aren't discretionary, but rather reflect differences in the contexts in which firms operate: computer design and production

Key words: Firms, innovation, evolution, competition

0143-2095/91/100061-14\$07.00

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technology and the computer market differ from the situation in textiles. Factor prices and availabilities and product markets in Germany differ from those in Taiwan. Thus, firms are forced to be different.

The tendency to ignore discretionary firm differences in part reflects that economists are not interested in behavior and performance at the level of firms, but rather in broader aggregates—industry or economy wide performance. It reflects, as well, some strong theoretical views held by most main line economists about what economic activity is all about, and about the role and nature of firms in economic activity. My argument that discretionary firm differences within an industry exist and do matter significantly is part and parcel of my broader argument that neoclassical economic theory is badly limited.

Let me flag here, for future elaboration, what I do and don't mean by the term 'discretionary'. I do mean to imply a certain looseness of constraints, both in the short and long run, that gives room so that firms that differ in certain important respects can be viable in the same economic environment. I do mean that to some extent these differences are the result of different strategies that are used to guide decision making at various levels in firms. On the other hand, I do not mean that what a firm is and does is under the tight control of high level decision makers. And I certainly do not mean that what makes a firm strong or weak at any time is well understood, even within the firms themselves, although there well may be an articulated point of view on this. More on these matters later.

The remainder of this essay is structured as follows. In the following section I shall flesh out my above remarks about the very significant differences in perspective between scholars trained or inclined to see discretionary firm level variables as important, and economists who see firm differences as determined largely by more aggregative economic forces. Then I focus on the basic theoretical preconceptions of neoclassical economic theory that lead to this position, and which make it very difficult to move any distance from it. I follow with an exploration of evolutionary economic theory which provides a very different view of what economic activity is all about and within which firm differences are central, and go on to consider the role of firm differences in the evolution of technology and

modes of organizing economic activity. Finally, a reprise.

THE DIVERGENT LITERATURES ON 'COMPETITIVENESS'

The differences in perspective can be seen clearly in the divergent literatures concerned with what now popularly is called the 'competitiveness' issue—the recent weakness of American firms, particularly *vis-à-vis* Japanese ones, in industries where not so long ago U.S. firms were doing very well. There is a sharp split between studies that focus on the differences between American and Japanese firms, and studies by economists that are focused on more aggregated variables.

Made in America, a publication put out in the summer of 1989 by the MIT Commission on Industrial Productivity, is a good example, and summary, of the former line of research. While the staff of the Commission undertook considerable research on its own, the multifaceted diagnosis it presents is quite consistent with that presented in a number of prior studies concerned with why American firms have been losing out.

American firms are hooked on old style mass production methods, in an era where flexible manufacturing has become a more effective mode of operation. Similarly, our hierarchical mode of organization and custom of specifying job assignments narrowly, while perhaps appropriate in an earlier era, now are sources of weakness. Research and product design and development stand too distant from manufacturing and production engineering; thus it takes American companies much longer than the Japanese to go from conception to production, and our production costs and quality often are inferior. American firms are myopic, both in the sense of their failure to look at world rather than national markets, and in the sense that time horizons are short. The latter partly has to do with the high cost of capital in the United States, but also with the way our managers think and the tools of analysis they are taught in business schools. Compared with the Japanese and Germans, our blue collar work force comes to the work place poorly trained by the public education system. This is compounded by a weakness of in-company training and retraining programs. Together, this puts American firms at a significant disadvantage

regarding labor skills. American firms are less willing to cooperate with each other on matters where cooperation would yield pay-off, partly because of the attitudes of managers, but also partly because government looks on cooperation with suspicion or hostility. More generally, business and government seldom work together and often are at odds.

Others might summarize the central arguments somewhat differently, but I believe the above does represent fairly the kinds of propositions about firm differences made in the report. The arguments are plausible and provocative, and may provide important guidance to American management, and for public policy.

However, there are two important issues one can raise about the conclusions of the study. First, one can question the confidence one should place in the causal connections asserted in studies like *Made in America*. Second, one also can question whether the variables treated there as basic really are so, as contrasted with themselves being determined by broader forces.

At this stage I want only to flag the former issue. However, there really is a big question about just what Japanese firms in the automobile industry, or the semiconductor industry, are doing that lies behind their evident stronger performance, in various dimensions, than American or European firms. Later in this essay I shall focus on this uncertainty, and some of its implications.

For the present I want to focus on the latter question, because it gets sharply into view the contrast between analyses like *Made in America*, and the standard views of economists about the determinants of 'competitiveness'. There is some discussion in *Made in America* of macro or national level variables, like the exchange rate, the cost of capital, or more generally the system of corporate finance, the effectiveness of the public education system, government policies, etc. However, this is not where the focus is. It is firm level variables that receive the top billing, and it is presumed that these are discretionary to a considerable degree. In contrast, the inclination of economists is to focus on macro, or environmental level variables, and to play down or ignore the role of firm discretion.

The same year that *Made in America* was published, three economists, Baumol, Blackman, and Wolff, published their interpretation and

diagnosis of lagging American productivity growth rates, and the convergence of productivity and living standards among the major industrial nations. The focus of *Productivity and American Leadership: The Long View* (1989) is usually at the level of the national economy, and sometimes at the level of the sector or industry. The variables considered are national savings and investment rates, investments in education, processes through which technology flows from creators to followers, and the like. There is scarcely a word about discretionary behavior at the level of firms.

It is strongly tempting, and I think right headed, to propose that each of the studies has described part of the elephant. The argument in the MIT study, that many of the difficulties American firms are having are self inflicted, is quite persuasive. At the same time the economist's proposition, that to a considerable extent firms are molded by the broader economic conditions surrounding them, is compelling. What seems sorely needed is an analysis that sees both of these matters, in a coherent way.

While the authors of *Made in America* never quite got into serious analyses of environmental variables, it does not seem difficult to augment an analysis that starts at the firm level to consider the environments that firms are in. Two new books are exemplary in that they do just this. Both recognize explicitly that national or environmental variables strongly influence firm strategy and structure, and that firms have considerable range of choice about these variables. Chandler's *Scale and Scope* (1990) describes in considerable depth how the different economic conditions, institutions, and cultures of the U.S., Great Britain, and Germany, molded the nature of the modern manufacturing firms that grew up in these different countries in the first decade of the twentieth century, and influenced the industries in which the nations developed special strength. However, there is nothing deterministic about Chandler's description of how the environment shapes firms and influences their performance.

Porter's *The Competitive Advantage of Nations* (1990) presents a similar perspective in which environmental influences matter greatly, but the firms have a considerable range of freedom regarding whether, or just how, they will take advantage of the opportunities the environment

affords. Indeed both authors see the firms as to some extent molding their own environment as, for example, in calling forth significant public investments in education in the U.S. and Germany.

Chandler is an historian by training. Porter's formal training is in economics, but his career has been at a Business School and his research focus has been on management. It should be recognized that the orientation of these authors to 'firms' is quite different than that in most of economics. Indeed it is apparent that for both authors the center of attention is the firms, and the central questions are 'how are they doing' and 'what makes them strong or weak'. They are drawn to wider economic mechanisms and institutions in the search for answers to these questions. Now firm performance clearly is related to broader economic performance, but I have argued above they are not the same thing. Since neither Chandler nor Porter presents a coherent statement of the economy wide problem, their analyses stop considerably short of providing an answer that would satisfy economists to the question of 'why do firms differ and how does it matter?'

FIRMS IN NEOCLASSICAL ECONOMIC THEORY

To get at that question from an economist's perspective, one needs to start with a broad understanding of what economic activity is all about, and what constitutes good economic performance or poor. Neoclassical theory, which provides the current conventional wisdom on these matters for economists, militates against paying attention to firm differences as an important variable affecting economic performance for several reasons.

The first is the perception of what economic activity is all about. Since the formulation of general equilibrium theory almost a century ago, the focus has largely been on how well an economy allocates resources, given preferences and technologies. This position is far from universal. Empirically oriented economists have been interested in things like technical change and, recently, there has been a rash of work on economic institutions and how and why these change over time. Schumpeter some time ago

put forth a strong general theoretical challenge to the effect that innovation ought to be the center of economic analysis. But it is hard to overestimate the degree to which economists continue to see the central economic problem as that of meeting preferences as well as possible, given resources, and prevailing technologies and institutions. This perspective implies a rather limited view of what firms are about.

Second, partly reflecting this general orientation, but not the only possible formulation of firms' decision processes consistent with it, economists became wedded to a theory of firm behavior that posited that firms face given and known choice sets (constrained for example by available technologies) and have no difficulty in choosing the action within those sets that is the best for them, given their objectives (generally assumed to be as much profit as possible). Thus the 'economic problem' is basically about getting private incentives right, not about identifying the best things to be doing, which is assumed to be no problem.

The perspective on the economic problem and the theory of firm behavior described above do not invite a careful inquiry into what goes on in firms. However, the tradition in economics of treating firms as 'black boxes,' was not inevitable either. The fact that until recently at least, this has been the norm deserves recognition in its own right.

The overall result is a view that what firms do is determined by the conditions they face, and (possibly) by certain unique attributes (say a choice location, or a proprietary technology) they possess. Firms facing different markets will behave and perform differently, but if the market conditions were reversed so would be firm behaviors. Where the theory admits product differentiation, different firms will produce different products but, in the theoretical literature, any firm can choose any niche. Thus there are firm differences but there is no essential autonomous quality to them.

The theoretical orientation in economics thus leans strongly against the proposition that discretionary firm differences matter. Of course economists studying empirical or policy questions have a proclivity to wander away from the tethers of theory when the facts of the matter compel them to do so. Thus in doing industry studies, economists often have been forced to recognize,

even highlight, firm differences, and differences that matter. One cannot study the computer industry sensitively without paying attention to the peculiarities of IBM. The recent history of the automobile industry cannot be understood without understanding Toyota and G.M. But as the Baumol, Blackman, and Wolff book testifies, the theoretical preconceptions shared by most economists lead them to ignore firm differences, unless compelled to attend to them.

Several recent developments in theoretical economics would appear to be changing this somewhat. Thus the same summer that *Made in America*, and *Productivity and American Leadership* were published, the long awaited *Handbook of Industrial Organization* (1989) was also. Included in the chapters were several that survey theoretical work that does recognize firm differences.

There are, first of all, the essays by Ordover and Saloner, and by Gilbert, which are expressly concerned with theoretical work that aims to explain firm differences, or at least some consequences of firm differences. In the models reported, there usually is an incumbent in the industry, or in the production of a particular product, who has certain advantages over firms who might think of joining the action. The presence of these advantages, or threats of action should a newcomer try to encroach, is enough to make the advantages durable. Gilbert deals more generally with models where there are costs to firms of changing their market positions. However, with few exceptions the models surveyed in these chapters do not consider in much depth or detail original sources of firm differences.

Reinganum's chapter, which surveys modern neoclassical models of technological innovation, is focused on what certainly is an important source of such differences—industrial R&D and the innovation R&D makes possible. In the models she surveys, a firm's technology may differ from a rival's because of the luck of an R&D draw, with the advantages made durable by patent protection or subsequent learning curve advantages. Given an initial difference, firms may face different incentives and thus find different courses of action most profitable. However, while these models may rationalize the observation that firms possess different technologies, the answers as to why certainly aren't very deep. And one comes away from

them, or at least I do, with very little theoretical insight into why IBM is different, or Toyota, and so what.

There has been a certain amount of recent theoretical work by economists that looks inside of firms, at their structure, and thus seems to give promise of a theoretical window for a deeper look into why firms differ. The chapters by Holmstrom and Tirole, and by Williamson, report on such work. The questions explored in the surveyed work include what determines, through make or buy decisions, the boundaries of a firm, how it is organized, the relative bargaining power of owners, managers, and workers, etc. But, again, the ultimate reason for why firms differ is rather superficial. Implicitly they differ because some chance event, or some initial condition, made different choices profitable.

In my view, recent theoretical developments in neoclassical theory have loosened two of the theoretical constraints making it difficult if not impossible to see firm differences as important. Economists are getting away from the theoretical tethers of static general equilibrium theory and are treating technology as a variable not a given. And they are trying to look inside the black box of the firm. However, for the most part there has been failure to get away from the third tether—taking a firm's choice sets as obvious to it and the best choice similarly clear and obvious. And because of that, the reasons for firm differences, in technology or organization, are ultimately driven back to differences in initial conditions, or to the luck of a draw, which may make choice sets different. Given the same conditions, all firms will do the same thing.

As I indicated above, I certainly do not want to play down the role of environment in constraining and molding what firms do. And I do not want to play down the role of chance in causing large and durable subsequent differences among firms. But in my view the models most economists keep playing with do not effectively come to grips with what lies behind the firm differences highlighted in *Made in America*, or the implications of those differences.

The reason, I want to argue, is that while the surveyed work purports to be concerned with 'innovation', with the introduction of something new to the economy in the form of new technology or a new way of organizing a firm, the models in question completely miss what is involved

in innovation. Thus nowhere in the models Reinganum describes is the fundamental uncertainty, the differences of opinion, the differences in perceptions about the feasible paths, that tend to stand out in any detailed study of technical advance, even recognized, much less analyzed in any detail. Williamson's own work on the determinants of firm organization has been much influenced by Chandler, and he dedicates a certain space in his chapter to a transactions cost interpretation of Chandler's account of the rise of the modern corporation. But nowhere does he recognize explicitly the halting, trial and feedback, often reactive rather than thought-through, process that led to the new ways of organizing that Chandler describes.

Put compactly, the treatment of technological and organizational 'innovation' described in these chapters simply takes the given 'choice set' and 'maximizing over it' presumptions of standard neoclassical theory and applies them to 'innovation.' That is, innovation is treated as basically like any other choice. Investment costs may need to be incurred before the new product or organizational design is ready to be employed, but in neoclassical theory this is true of other capital goods like a bridge or a machine. There may be high risks involved in doing something new, in a formal sense of that term, but this is treated as statistical uncertainty with the correct probability distribution known to all as is standard in micro economic theory. The innovation may yield a new latent or manifest public good, and this raises theoretical problems of 'market failure', but this is no different than investment in, say, public health.

But what if effective treatment of innovation (and perhaps other activities) requires breaking away from the assumptions of clear and obvious choice sets and correct understanding of the consequences of making various choices? Does it really make sense to work with a model that presumes that the transistor, or the M form of organization, were always possible choices out there and known to all relevant parties, and that they simply were chosen and thus came into existence and use when conditions made profitable the relevant investments? Does the assumption that 'actors maximize' help one to analyze situations where some actors are not even aware of a possibility being considered by others?

If one reflects on these issues, one may be

moved to adapt a very different view of the economic problem. Within this view, which I will call evolutionary, firm differences play an essential role.

INNOVATION AND FIRMS IN EVOLUTIONARY THEORY

The models of technological innovation surveyed by Reinganum show economists interested in the theory of the firm struggling to break away from the orientation of general equilibrium theory, which sees the economic problem as allocating resources efficiently, given technologies. So too the new literature on organizational innovation. Here economists seem to be basically interested in how new ways of doing things—technologies, and ways of organizing and governing work—are introduced, winnowed, and where proven useful, spread, as contrasted with how familiar technologies and organizational modes are employed. Many years ago Schumpeter insisted that the focus of general equilibrium theory was on questions that, over the long run, were of minor importance compared with the question of how Capitalist economies develop, screen, and selectively adopt new and better ways of doing things. Many of the writers surveyed by Reinganum call themselves 'neo Schumpeterians'.

However, the dynamic processes Schumpeter described are not captured by the new neoclassical models. As he put it 'in dealing with Capitalism, you are dealing with an evolutionary process'. He clearly had in mind a context in which people, and organizations, had quite different views about what kinds of innovations would be possible, and desirable, and would lay their bets differently. There are winners and losers in Schumpeter's 'process of creative destruction', and these are not determined mainly in ex-ante calculation, but largely in ex-post actual contest.

In his 1911 *Theory of Economic Development*, Schumpeter saw the key innovative actors as 'entrepreneurs'. His 'firms' were basically the vessels used by entrepreneurs, and other decision makers forced to adapt to the changes wrought by entrepreneurial innovators or to go under. By the time (1942) he wrote *Capitalism, Socialism, and Democracy*, Schumpeter's view of the sources of innovation had changed, or rather it might be better to say that there had been a transformation

of the principal sources of innovation from an earlier era, and Schumpeter's views reflected this transformation. Modern firms, equipped with research and development laboratories, became the central innovative actors in Schumpeter's theory. The chapter by Cohen and Levin in the *Handbook* admirably surveys the wide range of empirical research that has been inspired by Schumpeter, particularly the research concerned with the relationships among innovation, firm size and other characteristics, and market structure.

In our book, *An Evolutionary Theory of Economic Change* (1982), Winter and I spent quite a bit of space presenting a 'theory of the firm' which is consistent with, and motivates, a Schumpeterian or evolutionary theoretic view of economic process and economic change. Our formulation drew significantly on Simon (1957), on Cyert and March (1963), and on Penrose (1959), as well as on Schumpeter. With the vision of hindsight, it is clear that our writing then was handicapped by insufficient study of the writings of Chandler, particularly his *Scale and Scope* (1966).

Since the time we wrote, there have been a number of theoretical papers on firm capabilities and behavior that draw both on Chandler and on our early formulation, and which add significantly to the picture. Papers by Teece (1980, 1982), Rumelt (1984), Cohen and Levinthal (1989), Dosi, Teece and Winter (1989), Prahalad and Hamel (1990), Pavitt (1987, 1990), Cantwell (1989, 1990), Kogut (1987), Henderson (1990), Burgelman and Rosenbloom (1989), Langlois (1991), and Lazonick (1990), all present a similar or at least a conformable theoretical view, although with differences in stress. The recent paper by Teece, Pisano, and Shuen (1990) provides an overview of many of these works, and I believe correctly states that the common element is a focus on firm specific dynamic capabilities.

This emerging theory of dynamic firm capabilities can be presented in different ways. Here it is convenient to focus on three different if strongly related features of a firm that must be recognized if one is to describe it adequately: its strategy, its structure, and its core capabilities. While each has a certain malleability, major changes in at least the latter two involve considerable cost. Thus they define a relatively stable firm character.

The concept of strategy in this theory of the firm is basically what business historians and scholars of management mean, as contrasted with game theorists. It connotes a set of broad commitments made by a firm that define and rationalize its objectives and how it intends to pursue them. Some of this may be written down, some may not be but is in the management culture of the firm. Many economists would be wont to propose that the strategy represents a firm's solution of its profit maximization problem, but this seems misconceived to me. In the first place, the commitments contained in a strategy often are as much a matter of faith of top management, and company tradition, as they are of calculation. Second, firm strategies seldom determine the details of firm actions, but usually at most the broad contours. Third, and of vital importance, there is no reason to argue *a priori* that these commitments are in fact optimal or even not self destructive. If it is proposed that competition and selection force surviving strategies to be relatively profitable, this should be a theorem not an assumption.

The concept of firm structure in this literature also is in the spirit of Chandler, as is the presumption that strategy tends to define a desired firm structure in a general way, but not the details. Structure involves how a firm is organized and governed, and how decisions actually are made and carried out, and thus largely determines what it actually does, given the broad strategy. A firm whose strategy calls for being a technological leader that does not have a sizeable R&D operation, or whose R&D director has little input into firm decision making, clearly has a structure out of tune with its strategy. However, the high level strategy may be mute about links between its R&D lab and universities, whether to have a special biotech group, etc.

Change in strategy may require a change in management as well as a change in articulation; indeed for the latter to be serious may require the former. However, within this theory of the firm structure is far more difficult to change effectively than is strategy. While changing formal organization, or at least the organization chart, is easy, and selloffs and buyups are possible, significantly changing the way a firm actually goes about making operating level decisions and carries them out is time consuming and costly to

do. Or rather, while it may not be too difficult to destroy an old structure or its effectiveness, it is a major task to get a new structure in shape and operating smoothly. Thus to the extent that a major change in strategy calls for a major change in structure, effecting the needed changes may take a long time.

The reason for changing structure, of course, is to change, possibly to augment, the things a firm is capable of doing well. Which brings the discussion to the concept of core capabilities. Strategy and structure call forth and mold organizational capabilities, but what an organization can do well has something of a life of its own.

Winter and I have proposed that well working firms can be understood in terms of a hierarchy of practiced organizational routines, which define lower order organizational skills, and how these are coordinated, and higher order decision procedures for choosing what is to be done at lower levels. The notion of a hierarchy of organizational routines is the key building block under our concept of core organizational capabilities. At any time the practiced routines that are built into an organization define a set of things the organization is capable of doing confidently. If the lower order routines are not there for doing various tasks, or if they are but there is no practiced higher order routine for invoking them in the particular combination needed to accomplish a particular job, then the capability to do that job lies outside the organization's extant core capabilities.

The developing theory of dynamic firm capabilities I am discussing here starts from the premise that, in the industries of interest to the authors, firms are in a Schumpeterian or evolutionary context. Simply producing a given set of products with a given set of processes well will not enable a firm to survive for long. To be successful for any length of time a firm must innovate. The capabilities on which this group of scholars focus are capabilities for innovation and to take economic advantage of innovation.

In industries where technological innovation is important, a firm needs a set of core capabilities in R&D. These capabilities will be defined and constrained by the skills, experience, and knowledge of the personnel in the R&D department, the nature of the extant teams and procedures for forming new ones, the character

of the decision making processes, the links between R&D and production and marketing, etc. This means that at any time there will be certain kinds of R&D projects that a firm can carry out with some confidence and success, and a wide range of other projects that, while other firms might be able to do them, this particular firm can not, with any real confidence.

R&D capabilities may be the lead ones in defining the dynamic capabilities of a firm. However, in a well tuned firm, its production, procurement, marketing and legal organizations must have built into them the capabilities to support and complement the new product and process technologies emanating from R&D. In Teece's terms, the firm's capabilities must include control over or access to the complementary assets and activities needed to enable it to profit from innovation. And in an environment of Schumpeterian competition, this means the capability to innovate, and to make that innovation profitable, again and again.

The concept of organizational capabilities, and the theory that Winter and I proposed as to what determines and limits them, does not directly imply any coherency to the set of things a firm can do. However, Dosi *et al.* (1989) argue that, in effective firms, there is a certain coherency. There would appear to be several reasons. The ones stressed by Dosi *et al.* basically are associated with localized learning in a dynamic context, and follow on the arguments that Winter and I made some time ago that, to be under control, a routine needs to be practiced. Firms need to learn to get good at certain kinds of innovation, and at the things needed to take advantage of these, and this requires concentration or at least coherency, rather than random spreading of efforts. Further, in many technologies one innovation points more or less directly to a set of following ones, and the learning and complementary strengths developed in the former effort provide a base for the next round.

But I think it also is the case that to be effective a firm needs a reasonably coherent strategy, that defines and legitimizes, at least loosely, the way the firm is organized and governed, enables it to see organizational gaps or anomalies given the strategy, and sets the ground for bargaining about the resource needs for the core capabilities a firm must have to take its next step forward. Absent a reasonably

coherent and accepted strategy, decision making about rival claims on resources has no legitimate basis. Decisions from above have no supportive rationale, and there is no way to hold back log rolling bargaining among claimants other than arbitrary high level decisions. There is no real guidance regarding the capabilities a firm needs to protect, enhance, or add in order to be effective in the next round of innovative competition.

But I think I simply am restating what Chandler, Lazonick, Williamson, and other scholars of the modern corporation, have been saying for some time. To be successful in a world that requires that firms innovate and change, a firm must have a coherent strategy that enables it to decide what new ventures to go into and what to stay out of. And it needs a structure, in the sense of mode of organization and governance, that guides and supports the building and sustaining of the core capabilities needed to carry out that strategy effectively.

If one thinks within the frame of evolutionary theory, it is nonsense to presume that a firm can calculate an actual 'best' strategy. A basic premise of evolutionary theory is that the world is too complicated for a firm to comprehend, in the sense that a firm understands its world in neoclassical theory. There are certain characteristics of a firm's strategy, and of its associated structure, that management can have confidence will enhance the chances that it will develop the capabilities it needs to succeed. There are other characteristics that seem a prescription for failure. However, there is a lot of room in between, where a firm (or its management) simply has to lay its bets knowing that it does not know how they will turn out.

Thus diversity of firms is just what one would expect under evolutionary theory. It is virtually inevitable that firms will choose somewhat different strategies. These, in turn, will lead to firms having different structures and different core capabilities, including their R&D capabilities. Inevitably firms will pursue somewhat different paths. Some will prove profitable, given what other firms are doing and the way markets evolve, others not. Firms that systematically lose money will have to change their strategy and structure and develop new core capabilities, or operate the ones they have more effectively, or drop out of the contest.

THE EVOLUTION OF TECHNOLOGY

In real capitalist economies, in contrast with the neoclassical models, technical advance proceeds through an evolutionary process, with new products and processes competing with each other and with prevailing technology in real time, rather than solely in ex-ante calculation. Some of the innovations will be winners, other losers. With the vision of hindsight the whole process looks messy and wasteful, and a more coherent planning approach to technological advance appears attractive.

However, it is striking how inefficient and misguided efforts to plan and control significant technical advance have been. Where, for one reason or another, society has been denied the advantages of multiple independent approaches to advance technology, which flows naturally from a basis of independent rivalrous firms, almost always the approach chosen has turned out, after the fact, to have major limitations. And since alternatives had not been developed to a point where they could be tried in comparison, there has been lock in. A number of U.S. military R&D efforts since 1960 are striking examples. Nuclear power programs are another. The fact is that in virtually every field where we have had rapid technical advance that has met a market test or its equivalent, we have had multiple rivalrous sources of new technology.

While Winter and I formally modelled company R&D programs as generating results through a random draw, in fact in the industries that I know well there has tended to be a certain consistency in the R&D efforts of particular companies. This consistency reflects a basically stable company 'strategy', and the core R&D and other dynamic capabilities it has put in place to carry it out. Where company strategies and associated capabilities differ significantly, their patterns of innovation are likely to differ significantly as well.

This has an important consequence often overlooked in the literature on technological imitation. When one firm comes up with a successful innovation, its competitors may differ significantly among themselves in their ability effectively to imitate or develop something comparable. Contrary to many economic models, effective technological imitation very often requires the imitating firm to go through many

of the same design and development activities as did the innovator, and to implement similar production and other supporting activities. Thus firms with similar strategies and core capabilities are in a much better position to imitate or learn and build from each others work than firms with different strategies and capabilities.

Thus to an extent the market is selecting on strategies and companies, as well as new technologies. This suggests that in some circumstances strategic diversity may get extinguished.

There is something to this argument. A number of analysts, some working in the tradition of economic research, some in a business school research tradition, have suggested that there is a natural industry life cycle. When an industry or a broad technology is new, a wide variety of approaches to technological innovation—strategies—is taken by different firms. As experience grows, certain of the approaches begin to look significantly better than others. Firms who have made the right bets do well. Those who have not, need to switch over, or drop out. A number of studies have shown that, as an industry or technology matures, there is a significant reduction in the number of firms, and in some cases the emergence of a ‘dominant design’ with all surviving firms producing some variety of that tuned to the niche they have found.

One fascinating question is what happens in a relatively mature industry when a new and potentially superior technology comes into existence. The evidence suggests that it matters whether the new technology is conformable with the core capabilities of extant firms, or requires very different kinds of capabilities. Tushman and Anderson (1986) call these two kinds of developments ‘competence enhancing’ and ‘competence destroying’. Under the latter circumstances, new firms are likely to be the innovators, and old firms often are unable to respond effectively. Tushman and Anderson note that a change in management, and presumably a major change in strategy, often is necessary if the old firm is to survive in the new environment. But it may not be sufficient. Structure and core capabilities are far more difficult to change than management and articulated strategies.

For a student of business management the question of what enables a firm to change directions effectively, and be a viable competitor in the new regime, is of central interest in its

own right. For an economist what matters is that pharmaceutical R&D take advantage of the new possibilities opened by new biotechnology, and not whether the old pharmaceutical firms do it, or whether they fail, so long as new ones take up the torch.

However, the fact that the leading edge companies in a field often change is a fascinating matter. It is consistent with the theory of focused and constrained core capabilities presented above. And it is a central reason why, for an economist interested in technological advance, firm differences matter importantly.

THE EVOLUTION OF FIRM ORGANIZATION

There has been far more study of the way technology advances than there has been of the way firm organization changes. By organization I mean what I think Chandler (1966) means by strategy and structure, those aspects of a firm that are wider and more durable than the particular technologies and other routines it employs at any moment, or even its extent core capabilities, and which in effect guide the internal evolution of these. It is apparent that change in organization in this broad sense, as well as advance in technology, has been an essential feature of the enormous economic progress that has been experienced over the last century and a half.

Some writers clearly would like to give organizational change separate and equal billing with technical advance as a source of economic progress. I would like to argue here, however, that one needs to understand organizational change as usually a handmaiden to technological advance, and not a separate force behind economic progress.

If I understand him correctly, this would be Chandler’s position. The new technology of the railroads required, for its effective implementation, the development of organizational capabilities far beyond that possessed by traditional owner managed firms. Line and staff organizational form, along with the development of the position of hired manager, enabled the railroads to be effectively ‘governed’, to use Williamson’s term. Later, new technologies which promised

economies of scale and scope in manufacturing called for large firms operating in several different product fields, or market areas. The M form of managerial structure evolved to govern effectively this kind of business operation.

Over the long run what has mattered most has been organizational changes needed to enhance dynamic innovative capabilities. Reich (1985), Hounshell and Smith (1988), and other writers have described how the organizational device of the industrial research and development laboratory came into existence, to permit firms to shield a portion of their scientific and technical personnel from the pressures of day by day problem solving so that they could work on the development of new products and processes. This development was preconditioned by the rise of a new 'technology' for product and process development, one employing the understandings and techniques of the sciences and engineering disciplines in a systematic way. One can read Chandler's and Lazonick's account of the rise of other aspects of the modern corporation in terms of Teece's arguments about needed complementary assets or capabilities.

As I read the case study evidence, devising and learning to use effectively a significantly new organizational form involves much the same kind of uncertainty, experimental groping, and learning by making mistakes and correcting them, that marks technological invention and innovation. New modes of organization aren't simply 'chosen' when circumstances make them appropriate. They, like technologies, evolve in a manner that is foreseen only dimly. And even when a firm makes a conscious decision to change organization, it may take a long time before it is comfortable and effective in its new suit of clothes.

I want to return here to a point I made at the start of this paper. I suspect that the uncertainties about new organization are even greater than those surrounding technological innovations. This is especially so regarding organization which molds effective dynamic innovative capabilities and the abilities to profit from innovation. At the present time there is little in the way of tested and proved theory (let me use the less pretentious word—knowledge) that enables confident prediction of the best way of organizing a particular activity, or what will be the consequences of adopting a different mode of organiz-

ation. If the 'rationally choosing' view of technological advance is misguided, the 'rationally choosing' view of organizational change is even more so.

Just as important, it is common, not infrequent, for a particular mode of organization put in place for one reason to turn out to have advantages, or disadvantages, in arenas that were not considered at the time the original move was contemplated and made. It also is common, not infrequent, that there be considerable dispute about just what features of a firm's organization are responsible for certain successes or failures.

Thus, as I understand it, large Japanese firms adapted 'life time employment' for their skilled workers in the early post war era to try to deal with a problem of skill shortages and labor unrest. It is quite unclear how many Japanese managers foresaw advantages associated with worker loyalty, and ability of a firm to do in-house training without fear of losing the investment through worker defection. Just in time was, I understand, largely a response to scarce space, high inventory costs, and input shortages. It is not clear how many saw that it would facilitate quality control.

American companies looking at their Japanese competitors often have been uncertain about just why the Japanese are better in some respects, and just what they can effectively transplant. They only will be able to learn by trying some things, seeing what happens, and having the good luck to see it right.

The evidence is very limited, but there is reason to believe that firms have greater ability to replicate themselves in another setting in a way that preserves their strength, than to comprehend and adopt what gives their rivals strength. Thus as Womack, Jones, and Roos (1991) and Clark and Fujimoto (1991) document convincingly, American automobile manufacturers still are struggling to catch up with the Japanese in terms of productivity and quality of production. Where they are coming close it seems to be in cases where the Japanese are serving as partners. This does not look accidental. Florida and Kenney (1991) report that Japanese owned automobile assembly plants in the United States have rather quickly been able to establish practices—strategies and structures—similar to their home operations, and with comparable outcomes.

I want to put forth the argument that it is organizational differences, especially differences in abilities to generate and gain from innovation, rather than differences in command over particular technologies, that are the source of durable, not easily imitable, differences among firms. Particular technologies are much easier to understand, and imitate, than broader firm dynamic capabilities.

From one point of view it is technological advance that has been the key force that has driven economic growth over the past two centuries, with organizational change a handmaiden. But from another perspective, we would not have got that technological advance without development of new ways of organization that can guide and support R&D and enable firms to profit from these investments.

I have been concentrating on firm organization. However, it is clear that the organizational changes that have enabled nations to support the modern R&D system and the technological advance it generates go far beyond those of firm organization. Universities had to change. New scientific disciplines and societies had to come into being. In many cases new bodies of law were needed. Some technologies required major new public infrastructure for their effective development.

The coevolution of technology and institutions is a fascinating subject. Chandler, and a few other scholars such as Hughes (1983) and Freeman (1989), have begun to address it. There clearly have been major national differences in how the institutions needed to support particular evolving technologies themselves evolved. Perhaps in the study of the coevolution of technology and institutions we will begin to develop a serious theory of how national comparative advantage comes into being, or is lost. But I now am far beyond the scope of this paper.

REPRISE

Students of firm management, in particular those working in the strategy field, treat discretionary firm differences as their bread and butter. Economists have tended to play down these differences, or to argue that they are the result not the cause of general economic differences. In good part the difference in viewpoints is due

to differences in basic interests—the student of firm management concerned with the fate of individual firms, and the economist interested in general economic performance of an industry or nation. But I have argued that the lack of interest by economists in discretionary firm differences stems as well from a particular theoretical view of economic activity and the role and behavior of firms.

If one takes an evolutionary rather than a neoclassical view of what economic activity is about, then firm differences matter importantly regarding issues that traditionally have been the central concern of economists. Competition can be seen as not merely about incentives and pressures to keep prices in line with minimal feasible costs, and to keep firms operating at low costs, but, much more important, about exploring new potentially better ways of doing things. Long ago Schumpeter remarked that the former function was trivial compared with the latter, if the measure was contribution to the economic well-being of humankind.

From the perspective of evolutionary theory, firm diversity is an essential aspect of the processes that create economic progress. Monopoly, or tight oligopoly with strong barriers to entry, can be seen as a serious economic problem, not so much because such structures permit a large gap between price and cost, but because they are unlikely to generate the variety of new routines, and the attendant shifts in resource allocation on which economic progress depends. One is suspicious of arguments to ‘rationalize’ production and innovation for the same reasons, particularly when the winds of change are blowing from uncertain angles.

Thus, the ‘dynamic capabilities’ view of firms being developed by scholars in the strategy field can be seen to be important not only as a guide to management, but also as the basis for a serious theory of the firm in economics. It, when embedded in an evolutionary theory of economic change, instructs us regarding ‘Why do Firms Differ, and How Does it Matter?’

ACKNOWLEDGEMENTS

The author is indebted to the Sloan Foundation, through its funding of the Consortium on Competitiveness and Cooperation, and to the

Conference on Fundamental Issues and Strategy for whom this essay was written, for support in its undertaking. Many people have commented helpfully on an earlier draft. I especially want to thank Alfred Chandler, Wesley Cohen, Rebecca Henderson, Richard Langlois, William Lazonick, Richard Rosenbloom, Donald Sexton, David Teece, and Michael Tushman.

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