

I

OVERVIEW AND MOTIVATION

I

Introduction

IN THIS VOLUME we develop an evolutionary theory of the capabilities and behavior of business firms operating in a market environment, and construct and analyze a number of models consistent with that theory. We propose that the broad perspective provided by an evolutionary theory is useful in analyzing a wide range of phenomena associated with economic change stemming either from shifts in product demand or factor supply conditions, or from innovation on the part of firms. The specific models we build focus in turn on different aspects of economic change—the response of firms and the industry to changed market conditions, economic growth, and competition through innovation. We draw out the normative as well as the positive implications of an evolutionary theory.

The first premise of our undertaking should be noncontroversial: it is simply that economic change is important and interesting. Among the major intellectual tasks of the field of economic history, for example, certainly none is more worthy of attention than that of understanding the great complex of cumulative change in technology and economic organization that has transformed the human situation in the course of the past few centuries. Among policy issues regarding the world economy today, none present a more critical mix of promise and danger than those that reflect the wide disparities in present levels of economic development and the strains that afflict societies struggling to catch up. In the advanced economies, meanwhile, successful modernization has brought forth new concerns about the long-term ecological viability of advanced industrial soci-

ety and renewed questions about the relation between material success and more fundamental human values. Among the focal concerns of theoretical economics in recent years have been the roles of information, the formation of expectations by economic actors, detailed analysis of markets functioning given the presence of various “imperfections,” and new versions of old questions about the efficiency of market systems. Much of this work seeks to comprehend, in stylized theoretical settings, the unfolding of economic events over time. Thus, any significant advance in understanding of the processes of economic change would cast new light on a range of intellectually challenging questions that are of great social consequence.

We expect, however, that many of our economist colleagues will be reluctant to accept the second premise of our work—that a major reconstruction of the theoretical foundations of our discipline is a precondition for significant growth in our understanding of economic change. The broad theory that we develop in this book, and the specific models, incorporate basic assumptions that are at variance with those of the prevailing orthodox theory of firm and industry behavior. The firms in our evolutionary theory will be treated as motivated by profit and engaged in search for ways to improve their profits, but their actions will not be assumed to be profit maximizing over well-defined and exogenously given choice sets. Our theory emphasizes the tendency for the most profitable firms to drive the less profitable ones out of business; however, we do not focus our analysis on hypothetical states of “industry equilibrium,” in which all the unprofitable firms no longer are in the industry and the profitable ones are at their desired size. Relatedly, the modeling approach that we employ does not use the familiar maximization calculus to derive equations characterizing the behavior of firms. Rather, our firms are modeled as simply having, at any given time, certain capabilities and decision rules. Over time these capabilities and rules are modified as a result of both deliberate problem-solving efforts and random events. And over time, the economic analogue of natural selection operates as the market determines which firms are profitable and which are unprofitable, and tends to winnow out the latter.

A number of our fellow economists do share with us a sense of general malaise afflicting contemporary microeconomic theory.¹ It is

1. It is noteworthy that since 1970 several of the presidential addresses given annually before the American Economic Association have lamented the state of economic theory. Leontief's address (1971) is explicitly concerned with the inability of microeconomic theory to come to grips with empirical realities. Tobin's address (1972), and Solow's (1980), are focused on macroeconomics, but are substantially concerned also

widely sensed that the discipline has not yet located a path that will lead to a coherent and sustained advance beyond the intellectual territory claimed by modern general equilibrium theory. The discovery of such a path will, it is believed, require a theoretical accommodation with one or more of the major aspects of economic reality that are repressed in general equilibrium theory. Much of the most interesting theoretical work of the past two decades may be interpreted as exploratory probing guided by a variety of different guesses as to which of the possible accommodations are the most important ones to make. Considerable attention has been given to imperfections of information and of competition, to transaction costs, indivisibilities, and increasing returns, and to some of the relations among these. It has been recognized that general equilibrium theory's austere description of the institutions of capitalism becomes woefully inadequate as soon as any of these accommodations to reality are made—and, on the other hand, that the actual institutional devices employed in real market systems constitute a complex and challenging object for theoretical study. The fruits of these exploratory efforts include a good deal of work that is intellectually impressive when taken on its own terms, much that is directly useful in understanding certain portions of economic reality, and some that seems likely to be of lasting value regardless of the future course that economics may take. But the great majority of these exploratory probes have carried along (or at least intended to carry along) almost all of the basic conceptual structure that orthodoxy provides for the interpretation of economic behavior.

We regard that structure as excess baggage that will seriously encumber theoretical progress in the long run, however much its familiarity and advanced state of development may facilitate such progress in the short run. Here, obviously, our appraisal of the situation is more radical than anything that can be associated with the "general malaise" referred to above. What we offer in this book is,

with the adequacy of the theoretical foundations that orthodox microeconomics provides for macroeconomics. Similar themes have been sounded in addresses to other professional organizations; see, for example, Hahn (1970), Phelps Brown (1972) and Worswick (1972). The sense of malaise is also reflected in a number of the review articles in the *Journal of Economic Literature*. Shubik (1970), Cyert and Hedrick (1972), Morgenstern (1972), Preston (1975), Leibenstein (1979), Marris and Mueller (1980), and Williamson (1981) all complain explicitly about the inability of the prevailing theory to come to grips with uncertainty, or bounded rationality, or the presence of large corporations, or institutional complexity, or the dynamics of actual adjustment processes. We do not aim in this footnote, or in the book as a whole, to identify all the souls that are kindred at least in their surface diagnosis of the problem, if not in their deeper diagnoses or prescriptions. We know that in this respect we are part of a crowd.

we believe, a plausible promise that fundamental reconstruction along the lines we advocate would set the stage for a major advance in understanding of economic change—and, at the same time, make it possible to consolidate and preserve most of the discipline's significant achievements to date. To make full delivery on such a promise is not a task for two authors, or for a single book.

1. THE TERMS OF THE DISCUSSION: "ORTHODOX" AND "EVOLUTIONARY"

We have above made the first of many references to something called "orthodox" economic theory. Throughout this book, we distinguish our own stance on various issues from the "orthodox" position. Some such usage is inevitable in any work that, like the present one, argues the need for a major shift of theoretical perspective on a wide range of issues. However, there may be some who would deny that any "orthodoxy" exists in economics, apart from a widely shared commitment to the norms and values of scientific inquiry in general. Others would agree that an orthodoxy exists in the descriptive sense that there are obvious commonalities of intellectual perspective and scientific approach that unite large numbers of economists. But they would strenuously deny there is an orthodox position providing a narrow set of criteria that are conventionally used as a cheap and simple test for whether an expressed point of view on certain economic questions is worthy of respect; or, if there is such an orthodoxy, that it is in any way enforced. Our own thought and experience leave us thoroughly persuaded that an orthodoxy exists in this last sense, and that it is quite widely enforced. We do concede that contemporary orthodoxy is flexible and ever-changing, and that its limits are not easily defined. It therefore seems important to attempt, if not an actual definition, at least a clarification of our use of the term.

We should note, first of all, that the orthodoxy referred to represents a modern formalization and interpretation of the broader tradition of Western economic thought whose line of intellectual descent can be traced from Smith and Ricardo through Mill, Marshall, and Walras. Further, it is a *theoretical* orthodoxy, concerned directly with the methods of economic analysis and only indirectly with any specific questions of substance. It is centered in microeconomics, although its influence is pervasive in the discipline.

To characterize the actual content of contemporary orthodoxy is a substantial undertaking, with which we will concern ourselves recurrently in this book. Here we address the question of how one

might check our claims that particular views and approaches are “orthodox”—or, alternatively, the question of how we would defend ourselves against a claim that we are attacking a straw man or an obsolete, primitive form of economic theory. The first recourse should be to the leading textbooks used in the relatively standardized undergraduate courses in intermediate microeconomics. These texts and courses expound the theoretical foundations of the discipline at a simplified level. They are generally viewed as providing important background for understanding applied work in economics—often, in fact, as providing *essential* background for applied work done at a respectable intellectual level. The best of the texts are notably insistent on the scientific value of abstract concepts and formal theorizing, and offer few apologies for the strong simplifications and stark abstractions they employ. Neither do they devote much space to caveats concerning the theory’s predictive reliability in various circumstances. In these respects and others, they prefigure the treatment of the same issues in advanced texts and courses in theory. Indeed, it often appears that doctoral-level courses in economic theory are distinguished from intermediate-level courses primarily by the mathematical tools employed, at least so far as the core topics are concerned.

There is, admittedly, a degree of caricature involved when texts aimed at college sophomores and juniors are nominated to represent modern economic theory. Many of the strong simplifying assumptions commonly employed—perfect information, two commodities, static equilibrium, and so on—are emphasized in such texts for reasons having to do with the perceived limitations of the students, and not because the discipline has nothing better to offer. And if the conclusions of the analysis are sometimes put forward without due emphasis on the qualifications to which they are subject, it is not necessarily because the importance of those qualifications is not recognized by the author. It is more likely because the students are seen as deserving a reward for their struggles with the logic of the argument, and as positively demanding clear-cut answers to put in the exam book. In many respects, orthodoxy is more subtle and flexible than the image of it presented in the intermediate texts.

There are, however, some very important respects in which the portrait is drawn true. First of all, the logical structure of the intermediate texts underlies much of the informal discussion of economic events and policies engaged in by economists and others with substantial economics background. This is particularly the case with views concerning the efficiency properties of market systems: there seems to be a remarkable tendency for discussion of this question to throw off the encumbrances of advanced learning and revert to a

more primitive and vigorous form. In this sense, the conclusions of intermediate analysis seem much more indicative of "where the discipline stands" than do appraisals that are theoretically more sophisticated, but also more difficult and less familiar to nontheorists. Second, the strong simplifying assumptions of the intermediate texts often have close analogues in advanced work, right out to the theoretical frontiers. It is a caricature to associate orthodoxy with the analysis of static equilibria, but it is no caricature to remark that continued reliance on equilibrium analysis, even in its more flexible forms, still leaves the discipline largely blind to phenomena associated with historical change. Similarly, defenders of orthodoxy may justifiably disdain to reply to criticisms of perfect-information assumptions, but they have something at risk if the criticism focuses instead on the assumption that all possible contingencies can be foreseen and their consequences weighed. Thus, although it is not literally appropriate to stigmatize orthodoxy as concerned only with hypothetical situations of perfect information and static equilibrium, the prevalence of analogous restrictions in advanced work lends a metaphorical validity to the complaint.

Last, there is one key assumption in the structure of orthodox thought that does not get significantly relaxed or qualified as one passes from intermediate to advanced theory; on the contrary, it becomes stronger to support a greater weight. This is the assumption that economic actors are rational in the sense that they optimize. In elementary instruction or in popular exposition, this assumption of economic rationality may be presented as a conceptual expedient justified by the realistic observation that people have objectives which they pursue with a certain amount of consistency, skill, and forethought. At the intermediate level, the assumption takes on a stark appearance that strains credulity, but then intermediate theory is pretty stark overall. In advanced forms of orthodoxy, while recognition of informational and other "imperfections" softens the general theoretical picture regarding what the actor knows, no such compromise with reality affects the treatment of economic rationality. As theoretical representations of the problems faced by economic actors increase in realistic complexity and recognition of uncertainty regarding values of the variables, there is a matching increase in the feats of anticipation and calculation and in the clarity of the stakes imputed to those actors. Never is such a theoretical actor confused about the situation or distracted by petty concerns; never is he trapped in a systematically erroneous view of the problem; never is a plain old mistake made. It is a central tenet of orthodoxy that this is the *only* sound way to proceed; recognition of greater complexity in the problem *obligates* the theorist to impute a subtler rationality to

the actors. Thus, with regard to rationality assumptions, to allow orthodox theory to be championed by its elementary and intermediate versions is to waive a set of objections that become particularly telling at the advanced level.

The foregoing discussion should make clear the sources of a problem that will arise repeatedly in the analysis that follows. Theoretical orthodoxy is manifested at a variety of levels, and displays a variable mix of strengths and shortcomings. Some of the shortcomings of elementary versions are corrected in advanced treatments; others are merely papered over. Sometimes a deficiency undergoes mutation to a new but analogous form, and some deep problems get exacerbated as the theory gets "better." We attempt to cope with this complex situation by modifying our references to orthodoxy with clarifying phrases—"textbook" or "simple" orthodoxy versus "advanced" or "recent developments," and so forth. We also distinguish between "formal" orthodoxy, displayed in logically structured theorizing, and the "appreciative" version which is more intuitive and modified by judgment and common sense. (This distinction is discussed further in the following chapter.) These devices are not entirely adequate to the task, but it does not seem reasonable to interrupt our discussion repeatedly for the sake of clarifying and documenting each criticism of orthodoxy. We hope that we have here provided an adequate guide, at least for those familiar with economic theory, to the way in which such detailed indictments might be developed.

Our use of the term "evolutionary theory" to describe our alternative to orthodoxy also requires some discussion. It is above all a signal that we have borrowed basic ideas from biology, thus exercising an option to which economists are entitled in perpetuity by virtue of the stimulus our predecessor Malthus provided to Darwin's thinking. We have already referred to one borrowed idea that is central in our scheme—the idea of economic "natural selection." Market environments provide a definition of success for business firms, and that definition is very closely related to their ability to survive and grow. Patterns of differential survival and growth in a population of firms can produce change in economic aggregates characterizing that population, even if the corresponding characteristics of individual firms are constant. Supporting our analytical emphasis on this sort of evolution by natural selection is a view of "organizational genetics"—the processes by which traits of organizations, including those traits underlying the ability to produce output and make profits, are transmitted through time. We think of organizations as being typically much better at the tasks of self-maintenance in a constant environment than they are at major change, and much better at

changing in the direction of “more of the same” than they are at any other kind of change. This appraisal of organizational functioning as relatively rigid obviously enhances interest in the question of how much aggregate change can be brought about by selection forces alone.

The broader connotations of “evolutionary” include a concern with processes of long-term and progressive change. The regularities observable in present reality are interpreted not as a solution to a static problem, but as the result that understandable dynamic processes have produced from known or plausibly conjectured conditions in the past—and also as features of the stage from which a quite different future will emerge by those same dynamic processes. In this sense, all of the natural sciences are today evolutionary in fundamental respects. Perhaps the most dramatic illustration of this point is the increasing acceptance of the cosmological theory of the Big Bang, a conception that regards all of known reality as the continuously evolving consequence of one great antecedent event. At a less cosmic level, science has come to see the continents as shifting with sporadic violence beneath our feet, the changing behavior of the Sun as a possible factor in human history, and the world’s climate as threatened with major and perhaps irreversible change as a consequence of industrialization. Against this intellectual background, much of contemporary economic theory appears faintly anachronistic, its harmonious equilibria a reminder of an age that was at least more optimistic, if not actually more tranquil. It is as if economics has never really transcended the experiences of its childhood, when Newtonian physics was the only science worth imitating and celestial mechanics its most notable achievement.²

There are other connotations that have at most a qualified relevance to our own evolutionary approach. For example, there is the idea of gradual development, often invoked by an opposition between “evolutionary” and “revolutionary.” Although we stress the importance of certain elements of continuity in the economic process, we do not deny (nor does contemporary biology deny) that change is sometimes very rapid. Also, some people who are particularly alert to teleological fallacies in the interpretation of biological evolution seem to insist on a sharp distinction between explanations that feature the processes of “blind” evolution and those that feature “deliberate” goal-seeking. Whatever the merit of this distinction in

2. In his *Dynamic Economics* (1977) Burton Klein discusses at some length this failure of economics to recognize the profound changes in the view of “what science is” that have occurred in the natural sciences, principally physics. His perceptions of the problems with contemporary orthodox economics are consonant with ours in many respects.

the context of the theory of biological evolution, it is unhelpful and distracting in the context of our theory of the business firm. It is neither difficult nor implausible to develop models of firm behavior that interweave “blind” and “deliberate” processes. Indeed, in human problem solving itself, both elements are involved and difficult to disentangle. Relatedly, our theory is unabashedly Lamarckian: it contemplates both the “inheritance” of acquired characteristics and the timely appearance of variation under the stimulus of adversity.

We emphatically disavow any intention to pursue biological analogies for their own sake, or even for the sake of progress toward an abstract, higher-level evolutionary theory that would incorporate a range of existing theories. We are pleased to exploit any idea from biology that seems helpful in the understanding of economic problems, but we are equally prepared to pass over anything that seems awkward, or to modify accepted biological theories radically in the interest of getting better *economic* theory (witness our espousal of Lamarckianism). We also make no effort to base our theory on a view of human nature as the product of biological evolution, although we consider recent work in that direction to be a promising departure from the traditional conception of Economic Man.

2. EVOLUTIONARY MODELING

It is not an easy matter to state precisely what orthodox theory entails. Our evolutionary theory, as we shall develop it in this volume, is similarly flexible and will take on diverse forms depending on the purpose of the particular inquiry. There is, nevertheless, a characteristic modeling style associated with each theory, a style that is defined by the features that diverse models have in common. The principal purpose of this section is to describe the general style of evolutionary modeling. Before proceeding to that task, we briefly set forth an analogous characterization of orthodox modeling, for the sake of the contrast provided.

The Structure of Orthodox Models

There are some readily identifiable building blocks and analytic tools employed in virtually all models within contemporary orthodox theory of the behavior of firms and industries.³ These same struc-

3. We are concerned here only with *describing* in general terms the structure of orthodox models; in the next two chapters we discuss the adequacy of orthodox modeling of economic change and offer a critique of the basic orthodox concepts.

tures are visible in models spanning a very diverse set of specific inquiries. While our discussion of the orthodox art form will be quite general, it might be useful for the reader to keep in mind the central and best-known example of orthodox modeling of firm and industry behavior: the standard textbook model of the determination of firm and industry inputs and outputs, and prices.

In orthodox theory, firms are viewed as operating according to a set of decision rules that determine what they do as a function of external (market) and internal (such as available capital stock) conditions. The theory contains a sharp answer to the question “Why are the rules the way they are?”—an answer that also yields predictions about the scope or characteristics of the rules. The rules reflect *maximizing* behavior on the part of firms. This is one structural pillar of orthodox models.

A maximization model of firm behavior usually contains three separable components. First, there is a specification of what it is the firms in the industry are seeking to maximize—usually profit or present value, but in some cases the objective is something different or more complex. Second, there is a specification of a set of things that the firms know how to do. Where the focus is on production in a traditional sense, these things might be specified as activities or techniques, assumptions made about the characteristics of activities and their mixability and about the properties of the “production set” thus determined. But in models concerned with other questions, the set of things a firm knows how to do might comprise advertising policies or financial asset portfolios. The third component of a maximizing model is the presumption that a firm’s action can be viewed as the result of choice of the action that maximizes the degree to which its objective is achieved, given its set of known alternative actions, market constraints, and perhaps other internal constraints (like the available quantities of factors that are fixed in the short run). In some models, the representation of maximizing behavior takes into account information imperfections, costs, and constraints.

The maximization approach permits the deduction of a decision rule or set of rules employed by a firm—a rule or rules that specify a firm’s actions as a function of market conditions, given its capabilities and objectives. It attempts a theoretical *explanation* of firm decision rules in the sense that it traces their origin and accounts for their characteristics by reference to these underlying considerations, together with the maximization procedure. The decision rules themselves are the operational part of the theory. In some cases a maximization model generates predictions about the form of the decision rules. For example, if the production set is strictly convex and firms treat prices as parameters, the “output supply rule” relating produc-

tion to product price is continuous and a price increase never decreases the output supplied. More generally, the maximization hypothesis leads analysts to try to figure out why a firm is doing something, or what it would do differently under different conditions, on the basis of an assessment of its objectives and its choice set.

The other major structural pillar of orthodox models is the concept of *equilibrium*. This is an extremely powerful and flexible concept; a full equilibrium in an orthodox model may be an equilibrium in two or three distinguishable senses relating to a number of different components or variables within the model's overall structure. The role and result of all these equilibrium conditions is to generate within the logic of the model conclusions about economic behavior itself—as distinguished from the conclusions about the *rules* of behavior that are generated by the maximization analysis. In the most basic example, the supply and demand curves in a market are simply aggregations of behavioral rules of individual sellers and buyers, which for each actor describe the transaction quantity that would be most desirable at each possible value of the market price. The *actual* value of the price—and hence the actual behavior of the actors—is determined by the supply-demand equilibrium condition, which picks out the specific price for which the aggregate desired purchase quantity precisely equals the aggregate quantity sellers wish to sell. Although the details may be different and much more complex, the spirit of equilibrium analysis in economics is almost always the same as in this basic example: to impose an equilibrium condition is to add an equation to the mathematical system characterizing the model and thus to provide for the determination, within the model, of the value of another variable.

Formal models embodying the central orthodox concepts of maximization and equilibrium have been built with a variety of mathematical tools. Indeed, the range and rate of change of the set of mathematical devices employed to explore an essentially constant set of theoretical concepts is such as to make one suspect that the key mechanisms in the process involve the levels of mathematical sophistication attained by researchers and their audiences, and not any deep affinities between the mathematical tools and the subject matter. Calculus techniques are, however, increasingly central in the intermediate and advanced pedagogy of the subject, and they have long been an important research tool. They do seem to provide a natural and efficient way of expressing some of the key ideas of orthodoxy, particularly those relating to maximizing behavior. Given some ancillary assumptions about the shape and smoothness of the frontiers of the choice set and other constraints, maximizing choices can be deduced by setting the appropriate derivatives equal to zero.

Lagrangian multipliers associated with the constraints have a natural connection to theoretical understanding of pricing. Equilibrium of the set of firms in question implies that the equations characterizing their maximizing behavior must be simultaneously satisfied. These mathematical ideas seem to fit the subject matter extremely well; undoubtedly, that is at least partly because they have significantly influenced the development of thinking about the subject matter.

The Structure of Evolutionary Models

The decision rules employed by firms form a basic operational concept of our proposed evolutionary theory as well as contemporary orthodoxy. However, we reject the notion of maximizing behavior as an explanation of why decision rules are what they are; indeed, we dispense with all three components of the maximization model—the global objective function, the well-defined choice set, and the maximizing choice rationalization of firms' actions. And we see "decision rules" as very close conceptual relatives of production "techniques," whereas orthodoxy sees these things as very different.

Our general term for all regular and predictable behavioral patterns of firms is "routine." We use this term to include characteristics of firms that range from well-specified technical routines for producing things, through procedures for hiring and firing, ordering new inventory, or stepping up production of items in high demand, to policies regarding investment, research and development (R&D), or advertising, and business strategies about product diversification and overseas investment. In our evolutionary theory, these routines play the role that genes play in biological evolutionary theory. They are a persistent feature of the organism and determine its possible behavior (though *actual* behavior is determined also by the environment); they are heritable in the sense that tomorrow's organisms generated from today's (for example, by building a new plant) have many of the same characteristics, and they are selectable in the sense that organisms with certain routines may do better than others, and, if so, their relative importance in the population (industry) is augmented over time.

Undoubtedly, there is a great deal of business behavior that is not, within the ordinary meaning of the term, "routine." Equally clearly, much of the business decision making that is of the highest importance, both from the point of view of the individual firm and from that of society, is nonroutine. High-level business executives do not, in the modern world, spend humdrum days at the office applying the same solutions to the same problems that they were dealing with five years before. We do not intend to imply any denial of these

propositions in building our theory of business behavior on the notion of routine. For the purposes of economic theorizing, the key point is somewhat different. It is that most of what is *regular and predictable* about business behavior is plausibly subsumed under the heading "routine," especially if we understand that term to include the relatively constant dispositions and strategic heuristics that shape the approach of a firm to the nonroutine problems it faces. The fact that not all business behavior follows regular and predictable patterns is accommodated in evolutionary theory by recognizing that there are stochastic elements both in the determination of decisions and of decision outcomes. From the point of view of a participant in business decision making, these stochastic elements may reflect the result of tumultuous meetings or of confrontations with complex problems under crisis conditions; but from the viewpoint of an external observer seeking to understand the dynamics of the larger system, the significant point about these phenomena is that they are hard to predict. Conversely, if they were *not* hard to predict, the observer would be inclined to interpret the tumult and the sense of crisis as some sort of organizational ritual—a part of the routine.

Our use of several different terms for different types of routines is meant to convey our appreciation that, for some purposes, it is important to distinguish between a production technique whose operation is tightly constrained by machinery or chemistry and procedures for choosing what technique to employ at a certain time, and also between a relatively low-order procedure or decision rule (for example, the way a new order is handled or an inventory decline recognized and responded to) and a higher-order decision rule or policy (for example, a rule to switch from use of oil to natural gas as fuel when the relative price ratio hits a certain level, or the custom of keeping advertising expenditures roughly in proportion to sales). But, as the use of the common term "routine" indicates, we believe that these distinctions are subtle and continuous, not clear and sharp. Orthodox theory makes a sharp distinction between the choice set and choosing—between what is involved in operating a particular technique and what is involved in deciding what technique to use. In our evolutionary theory we see strong similarities in these. In mixing up batches of raw materials, decisions have to be made as to whether the composition and temperature are right or not, and, if not, what to do. If there is a rationale for orthodoxy's policy of denying theoretical recognition to this element of choice in firm behavior by including it in the description of technique, it presumably has to do with the fact that the choices are made in a routinized manner, and perhaps also that they are not an important source of variability in the firm's profits. But empirical studies of pricing

behavior, inventory management, and even advertising policies reveal a similar "by-the-rule" character of firm decision making in these arenas. In some cases, though not in all, routinization holds sway in particular decision-making arenas because the important action is elsewhere—perhaps in finance, R&D policy, or coping with regulation.⁴ Thus, orthodoxy's unwillingness to give parallel treatment to the similar forms of routinized behavior involved in "doing" and "choosing" remains a puzzle and will be a recurring theme in this book.

In any case, evolutionary modeling highlights the similarities among different sorts of routines. At any time, a firm's routines define a list of functions that determine (perhaps stochastically) what a firm does as a function of various external variables (principally market conditions) and internal state variables (for example, the firm's prevailing stock of machinery, or the average profit rate it has earned in recent periods). Among the functions thus defined might be one that relates inputs required to output produced (reflecting the firm's technique), one that relates the output produced by a firm to market conditions (the supply curve of orthodox theory), and one that relates variable input proportions to their prices and other variables. But whereas in orthodox theory the available techniques are a constant datum, and decision rules are assumed to be the consequence of maximization, in evolutionary theory they are treated as simply reflecting at any moment of time the historically given routines governing the actions of a business firm.

Although the routines that govern behavior at any particular time are, at that time, given data, the characteristics of prevailing routines may be understood by reference to the evolutionary process that has molded them. For the purposes of analyzing that process, we find it convenient to distinguish among three classes of routines.

One of these relates to what a firm does at any time, given its prevailing stock of plant, equipment, and other factors of production that are not readily augmented in the short run. (In effect here we are defining the basic unit "period" in our evolutionary modeling, as a counterpart to Marshall's "short run.") These routines that govern short-run behavior may be called "operating characteristics."

A second set of routines determine the period-by-period augmentation or diminution of the firm's capital stock (those factors of pro-

4. A major theme of R. A. Gordon's classic study of corporate decision making (Gordon, 1945) is that many of the decisions with which economic theory is concerned (such as price and output determination) are made by routinized procedures, while corporate executives actually spend their time on matters of greater importance—which also happen to be matters that resist orthodox modeling.

duction that are fixed in the short run). The extent to which actual investment behavior follows predictable patterns probably varies a good deal from one situation to another. In some cases the decision making surrounding the question of whether to build a new plant may not be much different in kind from the decision making regarding whether or not to continue to run a particular machine that has been operating roughly, or to stop it and call in the maintenance crew. In other cases, the new plant decision may be more like a decision to undertake a major R&D program on a recently opened technological frontier, a problem without real precedent that is dealt with through improvised procedures. Which of the two patterns obtains probably depends importantly on the size of the investment project relative to the existing activity of the firm. As suggested above, this spectrum of realistic possibilities corresponds in evolutionary theory to a range of differing roles for stochastic elements in the representation of investment decision making. In the particular models we shall develop later in this volume, the investment rule used by firms will be keyed to the firm's profitability, and perhaps to other variables. Thus, profitable firms will grow and unprofitable ones will contract, and the operating characteristics of the more profitable firms therefore will account for a growing share of the industry's activity.

The selection mechanism here clearly is analogous to the natural selection of genotypes with differential net reproduction rates in biological evolutionary theory. And, as in biological theory, in our economic evolutionary theory the sensitivity of a firm's growth rate to prosperity or adversity is itself a reflection of its "genes."

Finally, we view firms as possessing routines which operate to modify over time various aspects of their operating characteristics. In a sense, the model firms of evolutionary theory can be thought of as possessing market analysis departments, operations research shops, and research and development laboratories. Or there may be none of these organizational devices built into a firm, but at least from time to time some people within the firm may engage in scrutiny of what the firm is doing and why it is doing it, with the thought of revision or even radical change. We propose that these processes, like other ones, are "rule guided." That is, we assume a hierarchy of decision rules with higher-order procedures (for example, scrutiny of the currently employed production technique, or the undertaking of a study of a range of possible modifications in advertising policy) which act occasionally to modify lower-order ones (the techniques used to make a particular part, or the procedure determining the mix of raw materials employed, or current decision rules regarding advertising expenditure). And there may even be procedures of a still

higher order, such as occasional deliberations regarding the adequacy of present research and development policy, or of the methodological soundness of the marketing studies being used to guide advertising policy.⁵

These routine-guided, routine-changing processes are modeled as "searches" in the following sense. There will be a characterization of a population of routine modifications or new routines that can be found by search. A firm's search policy will be characterized as determining the probability distribution of what will be found through search, as a function of the number of variables—for example, a firm's R&D spending, which in turn may be a function of its size. Firms will be regarded as having certain criteria by which to evaluate proposed changes in routines: in virtually all our models the criterion will be anticipated profit. The particular model we shall employ for search will depend on the question we are probing.

Our concept of search obviously is the counterpart of that of mutation in biological evolutionary theory. And our treatment of search as partly determined by the routines of the firm parallels the treatment in biological theory of mutation as being determined in part by the genetic makeup of the organism.

As in orthodoxy, the characterization of individual firms in evolutionary theory is primarily a step toward analyzing the behavior of industries or other large-scale units of economic organization. The models in this book are of "industries"—that is, situations in which a number of broadly similar firms interact with one another in a market context characterized by product demand and input supply curves. In modeling these situations we often find it convenient to assume that "temporary equilibrium" is achieved—to abstract from such short-run dynamic processes as those that establish a single price in the market in a single period. However, we emphatically do not assume that our model industries are in long-run equilibrium, or focus undue attention upon the characteristics of long-run equilibria.

The core concern of evolutionary theory is with the dynamic process by which firm behavior patterns and market outcomes are jointly determined over time. The typical logic of these evolutionary processes is as follows. At each point of time, the current operating characteristics of firms, and the magnitudes of their capital stocks and other state variables, determine input and output levels. Together with market supply and demand conditions that are ex-

5. This image of a hierarchical structure of rules, with higher-level rules governing the modification of lower-level ones, is essentially that presented by Cyert and March (1963, ch. 6).

ogenous to the firms in question, these firm decisions determine market prices of inputs and outputs.⁶ The profitability of each individual firm is thus determined. Profitability operates, through firm investment rules, as one major determinant of rates of expansion and contraction of individual firms. With firm sizes thus altered, the same operating characteristics would yield different input and output levels, hence different prices and profitability signals, and so on. By this selection process, clearly, aggregate input and output and price levels for the industry would undergo dynamic change even if individual firm operating characteristics were constant. But operating characteristics, too, are subject to change, through the workings of the search rules of firms. Search and selection are simultaneous, interacting aspects of the evolutionary process: the same prices that provide selection feedback also influence the directions of search. Through the joint action of search and selection, the firms evolve over time, with the condition of the industry in each period bearing the seeds of its condition in the following period.

Just as some orthodox ideas seem to find their most natural mathematical expression in the calculus, the foregoing verbal account of economic evolution seems to translate naturally into a description of a Markov process—though one in a rather complicated state space. The key idea is in the final sentence of the preceding paragraph: the condition of the industry in each time period bears the seeds of its condition in the following period. It is precisely in the characterization of the transition from one period to the next that the main theoretical commitments of evolutionary theory have direct application. However, those commitments include the idea that the process is not deterministic; search outcomes, in particular, are partly stochastic. Thus, what the industry condition of a particular period really *determines* is the probability distribution of its condition in the following period. If we add the important proviso that the condition of the industry in periods prior to period t has no influence on the transition probabilities between t and $t + 1$, we have assumed precisely that the variation over time of the industry's condition—or "state"—is a Markov process.

Of course, a vast array of particular models can be constructed within the broad limits of the theoretical schema just defined. Each particular model defines a particular Markov process, which may be analyzed with the aid of the mathematical propositions relating to Markov processes in general. For such analysis to reach conclusions of economic interest, however, there must be a lot of specific eco-

6. Alternatively, firm decisions and market prices may be jointly determined in each time period.

conomic content in the model. General theorems about Markov processes are not themselves of economic interest; they are just tools that are useful in attempting to extract the conclusions that have been introduced into the model through its specific assumptions. For example, it may be possible to show that the industry approaches a “long-run equilibrium,” which may be either a static condition or a probability distribution of the industry state that applies (approximately) to all dates in the remote future. And if an approach to such an equilibrium is in fact implied in the model’s assumptions, it will ordinarily be possible to describe some properties of such an equilibrium—for example, to describe the operating characteristics of firms that survive.

An important determinant of the success of efforts to extract such conclusions is the complexity of the model. This brings us to an important point regarding the scope of evolutionary theory and, more particularly, of the class of Markov models of industry evolution. At an abstract level, this modeling schema has enormous generality. We may think of a “firm state” as comprising descriptions of the firm’s physical state (plant and equipment), information state (contents of file drawers and human memories), operating characteristics, investment rules (affecting transitions of physical state), recording rules (affecting transitions of information states), and search rules (affecting transitions of operating characteristics, recording rules, and search rules). All of these descriptions could in principle be highly detailed. We can think of an “industry state” description as involving the list of all firm state descriptions, for all firms in being and also for potential or deceased firms, together with a list of environmental variables that may be determined as given functions of time and/or as functions of the firm states. The transition rules for this complex industry state description are largely implicit in the description itself. Operating characteristics map physical and information states into current actions. Current actions and the date determine the environmental variables. Firm by firm, the current firm state and values of environmental variables are mapped into a new firm state by application of investment, recording, and search rules. And the process continues.

There is nothing wrong with the foregoing as an abstract conceptualization. However, the point of a modeling effort is not just to describe a system, but to describe it in such a way that its behavior may in some degree be understood. It is for this reason that the models that appear later in this book are very simple examples within the abstract scheme just described. Like most of our orthodox colleagues, we distinguish sharply between the power and general-

ity of the theoretical ideas we employ and the much more limited results that our specific modeling efforts have yielded thus far.

3. PLAN OF THE VOLUME

In the following chapter, we examine and diagnose some key deficiencies of orthodox theory. Our own response to those deficiencies is placed against the backdrop provided by past criticisms of orthodoxy and by the broader tradition of economic thought.

At the end of Part I, we offer an option. Most readers, we hope, will be interested in our attempt to offer a plausible cure for certain deep-seated inadequacies of economic theory. These inadequacies involve, of course, the flagrant distortion of reality represented by economists' basic assumptions about individual and organizational capabilities and behavior. Part II sets forth this attempt. It contains no formal models itself, but rather develops the image of individual and organizational functioning that underlies and guides the subsequent modeling. We first scrutinize with some care the conceptual foundations of orthodoxy's treatment of these topics. We then set forth an alternative view focused on sequences of coordinated behavior—individual skills and organizational routines. Among other things, this analysis makes clear that there is no sharp line separating the performance of actions from the choosing of actions. Most important, Chapter 5 seeks to establish that the formal models that appear later in the volume are well founded in a realistic account of organizational capabilities and behavior generally, and of the sources of continuity therein in particular. The assumptions of the formal models seek to capture some of the main tendencies that emerge from the detailed mechanisms described in Part II.

Some readers will be interested above all in the style of formal theorizing that characterizes evolutionary theory, in the answers that evolutionary models give to standard analytical questions, and in the new lines of attack developed for the more recalcitrant problems of economic analysis. We suggest that these readers skip Part II and proceed to Part III, in which we deal with two of the central questions explored in the positive theory of firm and industry behavior: the characteristics of industry equilibrium and the response of firms and the entire industry to changed market conditions. By exploring these traditional questions with the concepts and tools of evolutionary theory, we develop the basis for comparisons with orthodoxy both in terms of methods and of results. It becomes clear that a

number of familiar theoretical conclusions can survive a shift to new foundations—but new interpretations and caveats surround them.

Part IV is concerned with developing and exploring several evolutionary models of long-run economic growth. It will be argued that the treatment of innovation within an evolutionary model provides a far better basis for modeling economic growth fueled by technical advance than does the neoclassical model amended by the introduction of variables that represent technical advance. In particular, we shall develop the point that an evolutionary theory of growth offers a framework that is far more capable of integrating micro and macro aspects of technical advance than is the more orthodox, formal approach.

In Part V, we turn to a problem that has resisted effective attack with conventional theoretical tools: analysis of the processes of competition through innovation described by Joseph Schumpeter (1934, 1950). We develop models capable of exploring and identifying strands of the rich web of relationships between market structure and innovation that such processes involve. One of the aspects explored will be the line of causation that connects successful innovation to firm growth to change in market structure. But we shall consider, as well, the more traditionally conceived Schumpeterian “tradeoffs” and some of the associated policy issues.

The analysis reverts to a less formal style in Part VI, where we discuss normative economics from the perspective provided by the evolutionary view of positive economics. Many of the traditional questions of normative theory will be dismissed as too artificial to be helpful surrogates for real issues, others will receive somewhat different answers, and a number of policy questions that are not brought into view with orthodox lenses will be observed and considered. In particular, the issue of the strengths and weaknesses of free enterprise as a means of organizing supply will be seen in a light quite different from that provided by contemporary welfare economics theory.

A final chapter reviews the progress made and points to the much larger agenda of tasks not yet undertaken.

2

The Need for an Evolutionary Theory

IT IS INCUMBENT upon those who propose a major shift of theoretical orientation to point out in some detail the deficiencies of the prevailing theory or the advantages of the prevailing alternative—or preferably both. Our case for the advantages of an evolutionary theory is presented throughout this volume in the course of our development and illustrative application of the theory itself. In this chapter we introduce our critique of orthodoxy and attempt to place it in the context of other work that has broken with the orthodox tradition.

There are numerous respects in which orthodox theory seems to us erroneous or inadequate. Accordingly, a critique might plausibly be initiated from any of a number of different perspectives. One possible emphasis would be methodological, since highly disputable questions of scientific methodology are raised by the defensive devices that shield orthodoxy from the facts of individual and organizational behavior. A survey of some of the more salient of those facts would provide an alternative perspective. Yet another might emphasize a critical appraisal of the sort of evidence that is typically put forward in *support* of the orthodox explanatory scheme. All of these approaches will be taken at one point or another in this book. But it seems appropriate to begin with an examination of orthodoxy's difficulties in the analysis of various facets of economic change—the same important theoretical tasks with which our evolutionary alternative is principally concerned.

1. THE AWKWARD TREATMENT OF ECONOMIC CHANGE BY ORTHODOX THEORY

Much of economic analysis is concerned with predicting, explaining, evaluating, or prescribing change. Presumably, then, the adequacy of a theory of firm and industry behavior should be assessed in good part in terms of the light it sheds on such phenomena as the response of firms and the industry as a whole to exogenous change in market conditions, or how it illuminates the sources and consequences of innovation. We are not the first to point out that orthodox theory tends to deal in an *ad hoc* way with the first problem, and ignores or deals mechanically with the second.

The theory of firm and industry behavior put forth in contemporary textbooks and certain advanced treatises certainly appears to address the first problem directly; indeed, this is what positive theory seems to be mostly about. Formal orthodox theory purports to explain the determination of equilibrium prices, inputs, and outputs under various underlying product demand and factor supply conditions. In the context of partial equilibrium industry analysis, for example, the heart of the theoretical exercise involves the derivation of output supply functions (firm and industry output as a function of factor and product prices), functions relating input proportions of firms to relative factor prices (presuming movements along isoquants), and so on. But, despite appearances to the contrary, the theory does not directly address the question: What happens if the demand for the product of the industry increases, or if the price of a particular factor of production rises? That is, it does not address the question unless one assumes both that behavioral adjustments are instantaneous and that these changes in market conditions and the resulting equilibrium prices are perfectly forecast in advance by everybody. More realistically, firms must be understood as making time-consuming responses to changed market conditions they had not anticipated on the basis of incomplete information as to how the market will settle down.¹

On this plausible interpretation, firm behavior in the immediate aftermath of a change in market conditions cannot be understood as "maximizing," in the simple sense of that term embraced by the theory in question, and the industry must be understood as being

1. In his *Foundations* (1947) Samuelson clearly articulates the "out of equilibrium" character of actual firm and industry responses to shocks. Since that time the profession has grown somewhat casual about the problem, in the context of partial equilibrium analysis. See, for example, the treatment of dynamics, introduced almost as an afterthought, in Henderson and Quandt (1980, pp. 159–169).

out of equilibrium at least for a time after the shock. Absent the perfect-foresight assumption or something very close, one must admit that changes in market conditions may come as a surprise to at least some firms in the industry. Once the unanticipated change comes, firms' prevailing policies, keyed to incorrect expectations, are not profit maximizing in the actual regime. Explicit models that recognize the problem tend to incorporate the assumption that, faced with a shock that makes old policies suboptimal, firms adapt to the changed conditions by changing their policies in an appropriate direction.² Seldom do these models assume that the changes are made instantly or once and for all. Positing adaptive (rather than maximizing) responses to unforeseen shocks is partially an implicit or explicit concession to the existence of some adjustment costs or "friction" in economic adjustment; friction, however, is a phenomenon that is not generally considered in the textbook accounts of optimizing behavior.

Some recent papers have recognized explicitly the adjustment cost/friction phenomenon, and have attempted to deal with it by treating the time path of response to an unforeseen shock as optimal, given adjustment costs.³ But such an approach founders if it is admitted that the response of firms in the industry to the initial set of disequilibrium prices will likely change those prices in ways that cannot be foreseen in advance, unless one goes back to the initial perfect-forecasting assumption. Indeed, it is a rather delicate and complicated theoretical matter even to define an optimum adjustment strategy in a context where there are many firms, unless some very stringent assumptions are made.

Thus, contrary to the apparent impressions of many economists, the operative theory (if one can call it that) of firm and industry response to changed market conditions is not derivable from the textbook formalism about profit maximizing and equilibrium constellations. Rather, the theory actually applied in the interpretation of real economic events is one that posits adaptive change (specified in any of several plausible ways) and typically involves two key pre-

2. In particular, notions of adaptive behavior have often been the implicit or explicit rationale for the use of distributed lags in applied econometrics. For discussions emphasizing that this sort of statistical specification is incompatible with strict orthodox theoretical principles, see Griliches (1967) and Nerlove (1972).

3. Formal analysis of the effects of various forms of economic friction has been undertaken in a number of advanced theoretical papers dealing with investment behavior and market functioning. See, for example, Gould (1968), Lucas (1967b), Treadway (1970), and a number of the papers in the volume by Phelps et al. (1970). For an empirical approach that emphasizes continuing optimal adjustment to changing market conditions, see Nadiri and Rosen (1973).

sumptions. One of these is that the *direction* of adaptive response is the same as the direction of the change in profit maximization constellations. The second is that the adaptive processes ultimately *converge* to the new equilibrium constellation.

At best this theory is an *ad hoc* mix of maximizing and adaptive models of behavior, and is not at all consistent with orthodoxy's rhetorical emphasis on the unique validity of the maximizing approach. At worst, there are some serious analytic stumbling points along the road. If decisions are taken at discrete time intervals, adaptive adjustment in "the right direction" may overshoot the goal—the well-known cobweb problem. Even in the absence of discreteness, differences in the presumed nature of adaptive response (for example, whether output increases or price increases in response to excess demand) can affect the stability conditions. Adaptive models may or may not generate time paths that converge to equilibrium. And whether they do or do not in a particular case, if the adaptive behavior model is accepted as characterizing how firms respond to unanticipated events, it should be recognized that its account of the process is not the formal model expounded in most textbooks and treatises. Verbal descriptions of adjustment, especially in elementary texts, do carry an adaptive flavor. This sort of discrepancy is not uncommon in theoretical discussion.

In general equilibrium theory, the same basic problem appears in another form. The objectives of the analysis are, of course, less pragmatic and applied, and more concerned with the functioning of highly idealized systems. F. H. Hahn (1970), in his presidential address to the Econometric Society, surveyed the accomplishments of the mathematical theory of general equilibrium, and called attention to the fact that economists had made little progress in modeling plausible processes of disequilibrium adjustment that converge to general competitive equilibrium. He noted that the institutional assumptions on which most of the extant stability theorems depend (Walrasian tâtonnement) are plainly artificial, while models slightly closer to reality fail to yield the desired result in realistic cases. He concluded that, absent understanding of dynamic adjustment processes out of equilibrium, "the study of equilibrium alone is of no help in positive economics. Yet it is no exaggeration to say that the technically best work in the last twenty years has been precisely that. It is good to have it, but perhaps the time has now come to see whether it can serve in an analysis of how economies behave. The most intellectually exciting question on our subject remains: Is it true that the pursuit of private interests produces not chaos but coherence, and if so, how is it done?" (Hahn, 1970, pp. 11–12).

In spite of Hahn's suggestion that "the time has now come," the

years that have passed since he wrote have yielded no significant progress on the problems he identified. The reason is simply that thoroughgoing commitment to maximization and equilibrium analysis puts fundamental obstacles in the way of any realistic modeling of economic adjustment. Either the commitment to maximization is qualified in the attempt to explain how equilibrium arises from disequilibrium, or else the theoretical possibility of disequilibrium behavior is dispatched by some extreme affront to realism. Applied work has tended to take the former path, and more abstract theoretical work the latter.

Much the same strains have distorted orthodox attempts to analyze innovation and technical change. To begin with, it is noteworthy that such analyses constitute a specialized literature, ignored not only in most of the theory textbooks, but also in the rest of the research literature. This segregation certainly does not reflect any corresponding isolation of technical change and innovation from other phenomena of economic reality. Rather, it is implicit testimony that the orthodox theoretical engines operate more smoothly in (hypothetical) environments from which these change phenomena are absent. The task of coping with the complications they introduce has been faced up to only when the particular characteristics of a specific subject matter have plainly left no other choice open—and sometimes not even then.

Technical advance is now acknowledged by economists to be a central force behind a wide variety of economic phenomena: productivity growth, competition among firms in industries like electronics and pharmaceuticals, patterns of international trade in manufactured goods, and many more. But recognition of its importance in these contexts long predated the attempts to represent its role in formal modeling. Such attempts have often reflected a grudging recognition that the data would continue to rebuff any theoretical structure from which technical advance is excluded. And the resulting models have typically grafted variables relating to technical advance onto orthodox theory in ways that aim to preserve as much as possible of the standard theoretical structure. In our view, these responses have been inadequate.

This intellectual syndrome surely marks the post-World War II theorizing about long-run economic growth. Empirical studies in the 1950s established that the historical growth of gross national product (GNP) per worker in the United States could not be accounted for by increases in complementary inputs per worker: there was a large unexplained residual. When models appeared that “predicted” the appearance of such a residual as a result of something called “technical advance,” they preserved most other aspects of orthodox static

theory. In particular, they maintained the basic assumptions that the firms in the economy maximize profit faultlessly and that the system as a whole is in (moving) equilibrium.⁴

It is, however, an institutional fact of life that in the Western market economies—the economies that growth theory purports to model—much technical advance results from profit-oriented investment on the part of business firms. The profits from successful innovation are *disequilibrium* phenomena, at least by the standard of equilibrium proposed in the models in question. They stem largely from the lead over competitors that innovation affords. And it is also a fact of life that the success of innovation is very hard to predict in any detail: different decision makers and firms make different bets even while under the same broad economic influences, and *ex post* some prove right and others wrong. Given these facts, the retention in growth theory of a static conception of profit maximization tended to hinder understanding of economic growth rather than facilitate it. Paradoxically, it had this effect because it underemphasized and obscured the part that the pursuit of profit plays in the growth process. For the sake of a formal adherence to the orthodox canon, growth theory abstracted from the uncertainty, the transient gains and losses, the uneven, groping character of technical advance, and the diversity of firm characteristics and strategies—that is, from the key features of the capitalist dynamic.

In principle, these features could have been much better accommodated in a more sophisticated theory embodying subtler applications of orthodox theoretical principles. Indeed, the fact that such a theory does not exist today must be attributed largely to the difficulty of constructing it rather than to a failure to appreciate the desirability of doing so. But while the difficulties imposed by the complexity of the subject matter are certainly substantial, it is important to note that orthodox theorists operate under additional severe constraints that are self-imposed. When properly invoked (by orthodox standards), the notions of maximization and equilibrium that are required to model uncertainty, diversity, and change are delicate and intricate intellectual devices. Extremely stringent criteria of consistency must be satisfied in models properly built around these notions—so stringent that their effect is to make situations that have been simplified and stylized to the point of absurdity blossom into challenging puzzles.⁵ There is no gainsaying the intellectual achieve-

4. We discuss these issues in considerable detail in Part IV.

5. The general theoretical approach identified with the term “rational expectations” is supremely orthodox in the sense that the consistency requirements associated with a rational expectations equilibrium are supremely stringent. What is note-

ment represented by the solution of such puzzles, but the achievement would be more interesting if only there were some reason to think that reality actually displays the consistency that the orthodox theorist struggles so valiantly to represent.

It is not surprising that growth theorists generally chose to rely upon simple conceptions of maximization and equilibrium, rather than attempting to carry the weight of the combined difficulties (inevitable and self-imposed) that the phenomena of growth present to orthodox theorizing. What is significant is that there was so little willingness to compromise further, that maximization and equilibrium retained the honored place in the theory while the key substantive phenomena were ejected.

A different response to the same problem has dominated the literature concerned with the nature of competition in industries marked by high rates of innovation. Schumpeter's basic contributions have been widely invoked by economists in their verbal accounts of behavior in these industries, but have received only a few attempts at formalization. Economic theorists, working with ideas of profit maximization and equilibrium, have known in their bones that it would be extremely difficult to build a model of Schumpeterian competition out of such components. As a result, until recently at least, economists whose motivation is to describe and explain economic phenomena as they see them, rather than to test or calibrate a particular body of theory against data, have had to work with verbal theoretical statements for which there is no established formal counterpart. Sometimes, in obeisance to the canons of acceptable economic argument associated with prevailing formal theory, these economists point to profit-seeking behavior and call it profit maximization, and to tendencies of dynamic competition to wipe out quasi-rents generated by past innovative success and call this equilibrium. However, it should be recognized that these conceptions of profit maximization and equilibrium are a far cry from those of contemporary formal theory, whether at textbook or advanced levels. Moreover, the intellectual coherence and power of thinking about Schumpeterian competition have been quite low, as one would expect in the absence of a well-articulated theoretical structure to guide and connect research.

There have been a number of attempts in recent years to model Schumpeterian competition. Most of these have employed the ortho-

worthy about this approach, aside from its indifference to descriptive accuracy at the individual actor level, is that its total dedication to the consistency aesthetic often forces the use of the most extreme simplifying assumptions in the statement of the model.

dox building blocks of maximization and equilibrium. Several have been quite ingenious. They have managed to call attention to certain phenomena that might obtain in the real world of Schumpeterian competition, and to provide at least pieces of plausible explanation for these. However, invariably they have two limitations. First, the requirement that the model adhere rigorously to the concepts of maximization and equilibrium has forced the theorists to greatly simplify and stylize the processes of R&D, industrial structure, the institutional environment, and so forth. Second, the simplifying assumptions employed obscure what seems to us to be essential aspects of Schumpeterian competition—the diversity of firm characteristics and experience and the cumulative interaction of that diversity with industry structure.

2. DIAGNOSIS AND PRESCRIPTION

Many of our criticisms of orthodox analysis are familiar enough, at least within the individual theoretical contexts to which they refer. Less familiar, and more controversial, is our suggestion that the difficulties of such analysis are largely a reflection of fundamental limitations arising from orthodoxy's canonical assumptions of profit maximization and equilibrium. If this suggestion is correct, the problems are not fully inherent in the subject matter, but on the other hand there is no reason to think that orthodox theorizing will ultimately overcome them. They will persist, though perhaps in altered form, until theoretical tools of quite different design are directed at them.

In economic theory, as in other spheres, novel designs are never innovative in all respects; they borrow heavily from what has gone before. This is certainly the case with our own proposal. Following is a concise statement of our key differences with orthodoxy—and also of the main points of agreement.

First, we believe it is a powerful theoretical hypothesis that economic actors—particularly business firms—have objectives that they pursue. Profit is an important one of these objectives. Indeed, in the specific models we present in this volume, profit is the only business objective explicitly recognized. And this assumed objective operates in our models of business behavior in the standard way—that is, as a criterion for choice among contemplated alternative courses of action. If this much were all that “profit maximization” implied, our models would be models of profit-maximizing behavior.

The profit maximization assumption of formal orthodox theory is,

however, much stronger than the view with which we have expressed agreement. It involves very definite commitments on the nature of the alternatives compared and the comparison process. We explore these commitments in detail in Chapter 3. Here we make the point concisely and a bit too starkly: the orthodox assumption is that there is a global, faultless, once-and-for-all optimization over a given choice set comprising all objectively available alternatives.⁶ This clearly conflicts with, for example, an assumption that the firm operates at all times with a status quo policy, the profitability of which it inexactly compares, from time to time, with individual alternatives that present themselves by processes not entirely under its control—changing policies when the comparison favors the presented alternative over the current status quo. This latter assumption is more in the spirit of evolutionary theory: it is an assumption of “profit seeking” or “profit-motivated striving,” but certainly not of profit maximization.

In a sufficiently calm and repetitive decision context, the distinction between striving for profit and profit maximization may be of little moment, but in a context of substantial change it matters a great deal. Strict adherence to optimization notions either requires or strongly encourages the disregard of essential features of change—the prevalence of Knightian uncertainty (Knight, 1921), the diversities of viewpoint, the difficulties of the decision process itself, the importance of highly sequential “groping” and of diffuse alertness for acquiring relevant information, the value of problem-solving heuristics, the likely scale and scope of actions recognized *ex post* as mistaken, and so forth. Many years ago Schumpeter remarked: “While in the accustomed circular flow every individual can act promptly and rationally because he is sure of his ground and is supported by the conduct, as adjusted to the circular flow, of all other individuals, who in turn expect the accustomed activity from him, he cannot simply do this when he is confronted by a new task . . . Carrying out a new plan and acting according to a customary

6. Although this characterization is stark, it is not erroneous. Some orthodox theoretical models appear superficially to fall outside its scope—for example, models of optimal search and other models of sequential decision making appear not to involve a once-and-for-all optimization. But close scrutiny discloses that what is modeled is indeed a once-and-for-all choice of an optimal *strategy* of response to the unfolding situation; indeed, the fact that this reduction to once-and-for-all choice is made possible is the essence of the analytical power of the notion of a strategy. This means that the actors in sophisticated orthodox models, like those in simpler ones, are conceived of as incapable of response to truly unanticipated information. Either they are essentially right about the problem from the start, or they can only deal with an unanticipated environment by responding, “Does not compute.”

one are things as different as making a road and walking along it" (Schumpeter, 1934, pp. 79, 85). In a similar vein, Baumol more recently said: "In all these [maximizing models] automaton maximizers the businessmen are and automaton maximizers they remain. And this shows why our body of theory, as it has developed, offers us no promise of being able to deal effectively with the description and analysis of the entrepreneurial function. For maximization and minimization have constituted the foundation of our theory, as a result of this very fact the theory is deprived of the ability to provide an analysis of entrepreneurship" (Baumol, 1968, p. 68). Change, in short, presents distinctive problems that automaton maximizers are ill-equipped to solve, and that theories incorporating automaton maximizers are ill-equipped to analyze.

We are similarly in partial accord with orthodoxy (with similarly important qualifications) on concepts of competition and equilibrium. Competitive stimuli and pressures are, we agree, an important part of the environment for the decision making that goes on in each of the firms in an industry. Competitive forces not only shape voluntary business decisions—they help to set involuntary, survival-related constraints on business decisions. And it is certainly useful, in attempting to understand the overall tendencies of a model constellation of competitive forces, to ask where the whole dynamic process is likely to wind up—that is, to look for a stable equilibrium configuration in which those particular forces would no longer be producing change.

Again, orthodoxy goes much further. In the most typical formulation, notions of competition and equilibrium are employed in tandem at an early stage of the modeling logic, and produce a drastic narrowing of the range of possibilities contemplated. Such models do not explicate the competitive struggle itself, but only the structure of relations among the efficient survivors. Obviously, they cannot address such questions as the duration of the struggle or the durability of the mistakes made in the course of it.

This theoretical neglect of competitive *process* constitutes a sort of logical incompleteness, noted in the discussion of the preceding section. It is only in equilibrium that the model of optimizing behavior by many individual actors really works. Disequilibrium behavior is not fully specified (unless it is by *ad hoc* assumptions). But this means that there is no well-defined dynamic process of which the "equilibrium" is a stationary point: consistency relations, and not zero rates of change, define equilibrium. The question of how equilibrium comes about cannot be posed in fully orthodox theoretical terms (without *ad hoc* assumptions), and thus necessarily cannot be answered.

We propose, in short, that orthodoxy's basic intuitions about economic reality are potentially much more helpful in understanding economic change than are the modern formalizations of those intuitions. While purpose and cogitation are fruitful assumptions to make in modeling the behavior of firms, strict profit maximization is not. Similarly, although it is legitimate and fruitful to model the processes by which actions taken by individual firms impinge on the others and in turn cause them to modify their actions, it is not fruitful to view that process as being always at or near equilibrium.

Why does the orthodox approach ultimately prove to be so crippling? It is because of the combined force of two shortcomings, neither of which would be fatal in itself. The first is the oft-noted lack of descriptive realism in the characterization of behavior and events. By adhering tenaciously to its extreme abstractions, orthodoxy forces economics into increasing isolation from sources of information and insight that could be of great value to it—from management theory and practice, psychology, organization theory, and business history, for example. The severe abstractions and the isolation they entail might be a justifiable cost if they adequately performed their function of facilitating analysis of complex systems. But it is only at the textbook level that the abstractions truly have a simplifying effect. This is orthodoxy's second critical shortcoming: in advanced theoretical work, and in many applied contexts, its apparatus is cumbersome. Faced with the facts of uncertainty and change, it attributes great explanatory force to elaborate hypothetical structures of preference and subjective probability. In gross disregard of Occam's Razor, it multiplies these entities far beyond the empirical necessities imposed by any reasonable prospect of endowing them with operational content.

If the foundations were empirically secure, the attention lavished on the ornate logical superstructure would be understandable. If the superstructure were austere and of immediate practical use, expedient commitments to shaky foundations might be justified. Increasingly, orthodoxy builds a rococo logical palace on loose empirical sand.

3. ALLIES AND ANTECEDENTS OF EVOLUTIONARY THEORY

In intellectual evolution, as in other sorts, the accidents and incidents along the way play an important role in the transformation of relatively simple and amorphous beginnings into the complex structures of later times. Thus, while traits of economic theory today be-

tray both its classical origins and its present scientific utility, it would be a mistake to suppose that these considerations, either separately or in combination, fully account for the form that this theory takes today. Adam Smith might have had other and more robust intellectual descendants than contemporary orthodoxy—and more adequate interpretations.

A distinctive feature of intellectual evolution is that successive generations of the contending "species" often leave to posterity their own interpretations of the evolutionary struggle itself—though without, of course, the benefit of full foresight as to its future course. The choices and accidents, the refinements and extensions that molded present orthodoxy have been discussed and disputed both as they occurred and retrospectively. Many of the theoretical issues with which we are concerned in this volume have a long, complex, and sometimes tedious history in the literature of the discipline. They are treated in the work of economists now considered in the "mainstream," but more particularly in the writings of others now classified primarily as critics and heretics. There are broad themes around which the individual issues may be organized—the nature and behavior of the firm and of market processes and structures, the character of capitalist social institutions more generally, and a range of questions concerning methodology, philosophy, and value. These themes interweave, however, and the historical dimension of the pattern contributes further complexity.

In the preceding section we have laid out our central agreements and disagreements with contemporary orthodoxy; here we do the same for a number of the critics and for earlier mainstream authors. This survey is, necessarily, neither exhaustive nor detailed, but it should suffice to suggest the main patterns of contrast, complementarity, and intellectual indebtedness that define the place of our work in the literature.

Managerialism and Behavioralism

We begin by considering two heterodox approaches to analysis of the business firm that have been developed in recent decades and that are marked by a comparatively strong commitment to some type of formal theorizing.

"Managerialist" thinking diagnoses the problem of orthodox theory as a failure to represent correctly the motives that directly operate on business decisions. Contrary to the tenets of orthodoxy, the objectives pursued by firms include more than merely profits. Baumol (1959), who proposed to replace profits with another simple objective—revenue (subject to a profit rate constraint)—and Wil-

liamson (1964), who proposed a more general model of managerial utility maximization, are two important examples of the class. Some authors have paid particular attention to the processes and means by which stockholders or the capital market as an institution imperfectly constrains the pursuit of managerial objectives. Under this heading one can place Marris (1964), Williamson (1970), Jensen and Meckling (1976), and Grossman and Hart (1980). As the last two examples illustrate, and as we further argue in Chapter 3, the gap between managerialist and orthodox analysis is sometimes small.

In our view, these proposals yield useful insights into questions of managerial behavior and performance that obviously cannot be addressed within the strict orthodox framework (since in that framework management is just another input). However, the particular problems with traditional theory that we have discussed above, and to which our analytic proposals are addressed, are not stressed by the "managerial motivation" theorists. Baumol, Williamson (in this guise), and other creators of managerial models generally have assumed that managers maximize whatever it is they seek to achieve, with full cognizance of all possible actions they might take and the consequences of choosing any. Our central concern is with the maximization postulate as a characterization of how managers make decisions given their objectives. And that concern is relevant whether the objective is profit or something different or more general.

Distinct from the managerialist view, but consistent with many elements of it, is the "behavioralist" position. Behavioralists, taking their lead from the work of Herbert Simon (1955a, 1959, 1965), stress some or all of the following elements. Man's rationality is "bounded": real-life decision problems are too complex to comprehend and therefore firms cannot maximize over the set of all conceivable alternatives. Relatively simple decision rules and procedures are used to guide action; because of the bounded rationality problem, these rules and procedures cannot be too complicated and cannot be characterized as "optimal" in the sense that they reflect the results of global calculation taking into account information and decision costs; however, they may be quite satisfactory for the purposes of the firm given the problems the firm faces. Firms satisfice; a firm is unlikely to possess a well-articulated global objective function in part because individuals have not thought through all of their utility tradeoffs and in part because firms are coalitions of decision makers with different interests that are unlikely to be fully accommodated in an intrafirm social welfare function.⁷

We accept and absorb into our analysis many of the ideas of the

7. The basic reference is, of course, Cyert and March (1963).

behavioral theorists. Our basic critique of orthodoxy is connected with the bounded rationality problem. We base our modeling on the proposition that in the short and medium run the behavior of firms can be explained in terms of relatively simple decision rules and procedures. Much more than the behavioralists, however, our concern has been with economic change. Therefore, we have put much more stress than they on processes that link changes in firm decision rules and procedures (including productive techniques) to a changing economic environment.

We are in sympathy with the behavioralist position that firms should not be viewed as having stable, finely graded yardsticks for the comparison of alternatives, and in some of our models we have included a variant of the "satisficing" idea put forth by Simon (1955a, 1959), and Cyert and March (1963). Leibenstein (1966) has made use of a similar idea, calling it "inert areas." However, in other models we have employed the profit yardstick in a relatively conventional way. We remain pragmatic about this issue. Finally, we follow the behavioralists in regarding computer simulation as a legitimate approach to the formal representation of theoretical schemes that, for one reason or another, do not lend themselves to analytical treatment. There are, however, some differences of philosophy and emphasis that distinguish our uses of simulation techniques from those illustrated in, for example, the work of Cyert and March.

We diverge from the behavioral theorists in our interest in building an explicit theory of industry behavior, as contrasted with individual firm behavior. This means on the one hand that our characterizations of individual firms are much simpler and more stylized than those employed by the behavioral theorists, and on the other hand that our models contain a considerable amount of apparatus linking together the behavior of collections of firms. Perhaps in the future it will become possible to build and comprehend models of industry evolution that are based on detailed and realistic models of individual firm behavior. If so, our work will at that point reconverge with the behavioralist tradition.

Analysts of Firm Organization and Strategy

A considerable literature has developed on the relationships linking the growth and profitability of a firm to its organizational structure, capabilities, and behavior. Several different but largely complementary strands are involved. Penrose (1959) provided the elements of an analysis linking firm growth, structure, and the nature of the management function. Though she was apparently unaware of Coase's (1937) transaction cost approach to the nature of the firm, her analysis

is largely consistent with it. More recently, Williamson in a number of works has woven the transaction cost theme together with other conceptual strands in a series of highly insightful analyses of firm scope, organizational structure, and related policy issues (1970, 1975, 1979, 1981).

A line of work centered in the Harvard Business School has explored a concept of business strategy in its relation to the organization of the firm; Chandler's (1962, 1977) historical analysis from this point of view has been particularly influential. The strategy concept involved in this tradition is distinctive. Implicitly, at least, it involves acceptance of the basic premise of bounded rationality—that the economic world is far too complicated for a firm to understand perfectly; therefore the attempts of firms to do well must be understood as being conditioned by their subjective models or interpretations of economic reality. These interpretations tend to be associated with strategies that firms consciously devise to guide their actions. Such strategies differ from firm to firm, in part because of different interpretations of economic opportunities and constraints and in part because different firms are good at different things. In turn, the capabilities of a firm are embedded in its organizational structure, which is better adapted to certain strategies than to others. Thus, strategies at any time are constrained by organization. But also a significant change in a firm's strategy is likely to call for a significant change in its organizational structure.⁸

As should be obvious by now, we have considerable sympathy for these lines of analysis. Our treatment of firm behavior, in Part II, draws on the work of Williamson and others, as well as on that of the behavioralists. In some of our models, the higher-order decision rules or policies with which we endow our firms may metaphorically be interpreted as their strategies. In these models firms have different strategies, and a central analytic concern is the viability or profitability of firms with different strategies. And although in the models described in this book we do not permit firms to change their strategies, such changes are quite admissible within the logic of our theory. Indeed, within an evolutionary theory, change in strategy or policy can be treated in exactly the same way as change in technique.

We also are strongly sympathetic with the proposition that firm

8. Caves and Porter (1977) and Caves (1980) offer interpretations of the business strategy literature and establish the relevance and usefulness of its concepts in the context of industrial organization economics. The gap between the concerns of that literature and those of orthodox microeconomic theory has been narrowed by the theoretical contributions of several economists, particularly Spence (1979, 1981; see also Porter and Spence, 1982).

organization is an important variable for analysis in its own right. There are strong connections both between a firm's strategy and its appropriate organizational structure, and between the techniques commanded by a firm and its organization. Largely in the interests of establishing an understandable linkage between individual firm behavior and industry behavior, our formal models in this book suppress considerations of internal structure and organizational change. But in principle, an evolutionary theory can treat organizational innovation just as it treats technical innovation. The problems of business strategy, like the issues explored by the behavioralists, clearly call for a rich and detailed modeling of individual organizations; the long-run challenge is to discover modeling techniques and analytical methods that will make a rich treatment of the individual firm compatible with tractability in the analysis of larger systems.

One feature that distinguishes our analysis from most of the work under the present heading is the explicitness of our rejection of the orthodox view of firms as optimizing actors—a view that tends to be presumed in the strategy literature. To our eyes, the situation here parallels that noted above in our discussion of Schumpeterian competition. The sort of "maximizing" imputed to firms in these informal analyses is so remote from the concept employed in orthodox formal models as to make its invocation plainly ritualistic. And indulgence in the ritual merely tends to postpone the day when formal theory might actually have substantial and fruitful application in these areas.

Views of the Activist Firm

Several prominent critics have focused their attention on the passive nature of the firms depicted by orthodox theory. They have proposed that in the most dynamic industries firms try to modify the demand for their products and engage in the development of new technologies, rather than merely reacting to market conditions by choosing the most appropriate technology for those conditions. Economists like J. M. Clark (1955), Galbraith (1967), and, of course, Schumpeter have stressed that typical market structures are not perfectly competitive and that firms employ advertising and research and development as central competitive weapons. A corollary to this emphasis has been a tendency to downplay the importance of price competition, particularly of the idealized form represented by standard competitive models, and to view large firms and relatively concentrated market structures as the typical case in the "interesting" part of the economy, if not in the economy as a whole. These perspectives converge in an assessment of the large corporation as a crit-

ical feature of the institutional dynamics of modern capitalism, as a relatively autonomous chooser of society's means and to some extent of its effective ends, and as the stimulus for the development of new social institutions for its control and accommodation.

Of this bundle of concerns, it is really only the role of the large firm in technological change that we address seriously in this book. Even in that arena, our formal models are restricted, in the interests of simplicity, to the case of "disembodied" process innovation in an industry in which firms produce a homogeneous product. We do not analyze advertising or, indeed, do anything about reforming consumer theory: the theory implicit in our models is orthodox. And we touch only briefly on the implications of our theory for the complex institutional design problems in which the role of the large corporation is central. All of these limitations and lacunae simply reflect our inability to address all the important problems at once, and are not intrinsic features of the evolutionary approach. They remain, at the end of the book, on the long agenda of important unfinished business.

Where our proposals for theoretical revision diverge from those of the most prominent critics of the sort just mentioned is in our concern with developing a formal theoretical structure with analytical power. Many of those economists who have criticized economic theory because of its static nature seem to be content with stressing that valid point and positing some generalities about Schumpeterian competition at a verbal level, but appear to have no particular interest in developing a formal theory of Schumpeterian competition. We are centrally concerned with the development of formal theory.

Schumpeter

The influence of Joseph Schumpeter is so pervasive in our work that it requires particular mention here. Indeed, the term "neo-Schumpeterian" would be as appropriate a designation for our entire approach as "evolutionary." More precisely, it could reasonably be said that we are evolutionary theorists *for the sake* of being neo-Schumpeterians—that is, because evolutionary ideas provide a workable approach to the problem of elaborating and formalizing the Schumpeterian view of capitalism as an engine of progressive change. Although Schumpeter had some harsh words for loose invocations of evolutionary ideas in the analysis of economic development (1934, pp. 57–58), we believe that he would have accepted our evolutionary models as an appropriate vehicle for the explication of his ideas.

There are, of course, numerous points of varying importance on

which our perspectives and conclusions differ from those of Schumpeter. Their number, and the fact that many of them are subtle, make it impractical to attempt a survey here. It does seem appropriate to remark on the extent to which the influence of the Schumpeterian vision has been limited over the years for want of adequate development (particularly *formal* theoretical development) of constitutive or complementary ideas. For example, Schumpeter's credentials as a theorist of bounded rationality could hardly be more incisively established than in the following passage from *The Theory of Economic Development*:

The assumption that conduct is prompt and rational is in all cases a fiction. But it proves to be sufficiently near to reality, if things have time to hammer logic into men. Where this has happened, and within the limits in which it has happened, one may rest content with this fiction and build theories upon it Outside of these limits our fiction loses its closeness to reality. To cling to it there also, as the traditional theory does, is to hide an essential thing and to ignore a fact which, in contrast with other deviations of our assumptions from reality, is theoretically important and the source of the explanation of phenomena which would not exist without it. (Schumpeter, 1934, p. 80)

Because Simon and others have taught us much about what behavior is like when it is *not* "prompt and rational," we are in much better a position to challenge the "traditional theory" from this point of view than was Schumpeter himself. On this issue and others, our position on the shoulders of the giant gives us a somewhat different perspective.

We are not alone in this regard. While the mainstream of economic analysis of technical change has repressed the bounded rationality problem, many scholars of technical change have recognized it, if sometimes implicitly. Our formal theoretical view is consonant, we believe, with the writings on technical change of such economic historians as Rosenberg (1969, 1974, 1976) and David (1974), industrial organization economists like Peck (1962) and Phillips (1971), scholars of contemporary industrial technical change and of public policy issues like Mansfield (1968, 1971, 1977), Pavitt (1971), Freeman (1974), and Klein (1967, 1977). With few exceptions these scholars have not tried to formalize their implicit theory about what is going on. Gunnar Eliasson's work (1977) is an exception, as is Carl Futia's (1980), and our theoretical structure has much in common with theirs in being both formal and explicitly evolutionary.

Frank Knight and the Modern Austrians

Schumpeter stressed innovation as deviation from routine behavior, and argued that innovation continually upsets equilibrium. Other scholars also have stressed the importance of breaking from routine, but have placed less emphasis on innovation—at least if that term connotes major novelty. Both Knight (1921) and Hayek (1945) have argued that the economic world is continually throwing up new situations that constitute opportunities to make a profit if the situation can be comprehended and seized appropriately. Perhaps a freeze destroys the citrus crop in Florida, or a new fad about Pandas develops, or an oil field is discovered under Cape Cod. What profitable business opportunities are thereby opened up, or foreclosed? Hayek has stressed that the hard economic problem is to respond appropriately to such changes. Knight argued that a key characteristic of such changes is that it is impossible to calculate the right thing to do; what is appropriate and what is not will be revealed only by events.

In recent years, Kirzner (1979) has drawn on and developed these ideas, articulating what he has called a (neo-) Austrian approach to analysis of market behavior. He has argued that the focus of theoretical attention ought to be on market processes, rather than on equilibrium conditions. We certainly are in accord. Littlechild and Owen (1980) have explored the neo-Austrian approach mathematically. We apply evolutionary theory to analyze the effect of autonomous changes in market conditions, as well as change induced by endogenous innovation. Our theory is a theory about market processes.

Evolutionary Theorists

The general idea that market competition is analogous to biological competition and that business firms must pass a survival test imposed by the market has been part of economic thought for a long time. Systematic development of the idea is, however, much rarer in the literature. For the most part, it has been briefly invoked for broad rhetorical purposes or as an auxiliary defense for the assumption of profit maximization. We briefly survey its use in the latter connection in Chapter 6.

Among the contributions that have taken the evolutionary point of view more seriously, Alchian's 1950 article "Uncertainty, Evolution and Economic Theory" stands out as a direct intellectual antecedent of the present work. In that article, Alchian noted the diffi-

culties in extending standard microeconomic theory to the case of uncertainty, and particularly emphasized the importance of examining the role of uncertainty from the *ex post* viewpoint, when some actions are seen to be successful and others mistaken. He proposed that evolutionary mechanisms would tend to bring about responses to changed market conditions on the part of populations of firms that were in accord with the predictions of orthodox theory. And he suggested that such a line of argument might provide a sounder guide and rationale for the use of the standard tools of economic analysis—but did not emphasize that quite different tools might turn out to be appropriate if such a shift of foundations were to occur.

Alchian offered only a few sketchy suggestions for specific models reflecting his approach. Winter (1964) investigated some differential equation models of selection processes as part of a general examination of the economic natural selection argument. The models served to stress in particular the distinction (and relationship) between a behavioral routine or rule and a particular action: what matters to survival is the actions taken in environments that occur repeatedly, not those taken very infrequently or those that exist only as the potential response a rule would yield to environmental states that never occur. Farrell (1970) explored a simple evolutionary model of speculative behavior with a quite different mathematical tool—the theory of branching processes. Dunn (1971) presented a view of economic and social development similar in many ways to ours. However, he did not develop his analysis formally.

In her 1952 critique of the use of biological analogies in economics, Penrose raised, among other questions, the problem of whether there exists an economic counterpart of genetic inheritance. To some extent, this problem had been anticipated by Alchian (1950, pp. 215–216), who emphasized the “reproduction” via imitation of rules of behavior. Winter (1971) made the connection to the work of the behavioralists, proposing that the observed role of simple decision rules as immediate determinants of behavior, and operation of the satisficing principle in the search process for new rules, provided the required genetic mechanism.

There has recently developed a flurry of intellectual exchange activity across the interdisciplinary frontiers where biology meets economics, other social sciences, and law. Evolutionary theorists in biology have directly borrowed concepts from modern formal economic theory (later we shall remark upon some of the awkwardness that is introduced to biological theory by taking the maximization and equilibrium notions too seriously). In turn, a number of economists have participated in the interdisciplinary literature on socio-

biology that has burgeoned since the publication of E. O. Wilson's book (1975).⁹ Hirshleifer (1977a), in particular, has emphasized both the unifying and synthesizing value of sociobiological ideas in the social sciences and the range of specific insights that sociobiology and economics can draw from each other. The sociobiological literature, or that part of it which applies evolutionary theory to human social behavior, links analysis of biological selection mechanisms to a long-standing tradition of study of sociocultural evolution. Campbell (1969) provided an excellent survey of that broad field and argued for the merits of a variation and cultural selection-retention theory of sociocultural evolution. Our own work may be viewed as a specialized branch of such a theory, as may the work of economists and lawyers exploring the evolution of the common law and the efforts of organization theorists who have taken the evolutionary tack.¹⁰ Indeed, a great web of intellectual connections links all the work cited in this paragraph (and much more): the shared ideas relate sometimes to specific substance, often to analytical concepts and formalisms, and always to a common evolutionary philosophy.

Classical, Marxian, and Neoclassical Antecedents

Although our theoretical views are clearly at odds with much of present orthodoxy, they are quite consonant with the tradition of microeconomic theorizing as it existed from the time of Adam Smith up until around World War II. What today's orthodoxy represents is, above all, a particular (and not inevitable) refinement and elaboration of the core ideas from that broader tradition relating to market functioning and self-interested behavior. The price paid for the refinement has been a considerable narrowing of focus and a tendency to segregate from the main corpus of theory the questions and phenomena for which the refined theory is ill-suited.

The title of Book I of *The Wealth of Nations* is "Of the causes of improvement in the productive powers of labor and of the order according to which its produce is naturally distributed among the different ranks of the people." The book commences with a discussion of what today would be called the sources and consequences of technical advance. John Stuart Mill, like Smith, provides a rich historical discussion of the evolution of both productive techniques and eco-

9. See, for example, Becker (1976) and the exchange that followed among Hirshleifer (1977b), Tullock (1977), and Becker (1977).

10. On the evolution of the common law, see Cooter and Kornhauser (1980) and references cited therein. The evolutionary, ecological approach to organizational analysis is set forth in Hannan and Freeman (1977); see also Kaufman (1975).

economic institutions to set the context for the narrower economic analysis, and his economic theory is to a considerable extent dynamic, not static.

Much of Marxian economic theory is evolutionary. Many of the recent attempts to formalize Marx, both by economists sympathetic to Marx and by those of more orthodox leanings, have, we think, been tightly bound by the analytical tools of contemporary orthodoxy. As a result, they have failed to do justice to his ideas about the laws of economic change. Some of our own ideas are quite compatible with those of Marx, in that we stress both that capitalist organization of production defines a dynamic evolutionary system and that the distribution of firm sizes and profits also must be understood in terms of an evolutionary system. However, while in some of our models the share of labor and capital is endogenous, we have not followed Marx and his contemporary sympathizers to the extent of focusing our analysis on the determinants of the profits-wages split. Nor does the play of political power have much of a role in the formal evolutionary models developed in this book, although in our discussion of normative economics from an evolutionary viewpoint, we do present some initial outlines of an endogenous theory of the evolution of government policies. Where a Marxian would most likely fault our discussion is in our failure to employ the ideas of contradictions and of class in our positive evolutionary modeling and our normative analysis. We have not found these concepts particularly useful.

Marshall is now generally regarded as a precursor or source of today's formal neoclassical economics. So he was, in the sense that he introduced to economics a portion of its present technical apparatus and stressed in particular that market analysis must consider both the supply and the demand side. But it is explicit in the *Principles* that his real interest was in economic dynamics:

The Mecca of economics lies in economic biology rather than economic mechanics. But biological conceptions are more complex than those in mechanics; a volume on foundations must therefore give a relatively large place to mechanical analogies; and frequent use is made of the term equilibrium, which suggests something of a statical analogy. This fact, combined with the predominant attention paid in the present volume to the normal conditions of life in the modern age, has suggested the notion that its central idea is "statical" rather than "dynamical." In fact it is concerned throughout with the forces that cause movement; and its key note is that of dynamics rather than statics. (Marshall, 1948, p. xiv)

Also, it is widely recognized that Marshall's writings reveal a somewhat agonized effort to balance the demands of rigorous theorizing with those of descriptive accuracy in the analysis of an evolving

system (see Koopmans [1957], and Samuelson [1967]). A striking example of the effect of these tensions is Marshall's imperfectly drawn distinction between static increasing returns to scale and what we would today call induced scale-augmenting technical change. Contemporary commentary on this tends to rebuke Marshall for his affront to the logic of purely static analysis; the fact that he quite correctly emphasized the role of (informational) increasing returns as an economic mechanism of irreversible change receives less attention. On this question and many others, our evolutionary theory is closer to the original Marshallian doctrine than is contemporary orthodoxy.

Similarly, although Pigou (1957; parts first published as *Wealth and Welfare*, 1912) is widely regarded as the source of contemporary welfare economics, he followed his teacher Marshall in attempting to analyze an economic world in continuing flux. Indeed, for Pigou economic change and the slowness of economic institutions in responding effectively to change were prime reasons for the problems recounted in his *Economics of Welfare*. This is the position we ourselves shall adopt in our treatment of the normative issues illuminated by an evolutionary theory.

Thus, while we break with contemporary orthodoxy on a number of issues that have concerned other critics before us, it is also true that our theory is compatible with, or even a natural extension of, a line of economic thought that goes back through Marshall to the classics. This appraisal raises two related questions. First, why did economic theory take the "wrong road"? Second, why have contemporary critics of orthodoxy had so little success in getting the error corrected? These and some wider questions about the intellectual forces operating in the development of the discipline are examined in the following section.

4. THE NATURE OF FRUITFUL THEORIZING IN ECONOMICS

The answer to the first question can be located in Marshall's own ambivalence. It has already been suggested that there was a strong tension in Marshall between having a theory that captured what he saw as the key structural aspects of the economic system and of economic processes, and having an abstract theory that was analytically tractable and logically complete. Given the mathematical tools at his disposal, he could not reconcile these two objectives. He recognized the great importance of the latter to the progress of economics as a science. That the discipline responded to his leadership in formal theory construction rather than to his richer insights into economic

reality probably reflects what the pursuit of "science" was thought to entail.

More generally, a reading of the economic literature and reflection upon the role of economic theory in economic analysis suggest that theory is used in two distinguishable ways. These two modes are sufficiently different so that one may reasonably think of two different kinds of theory as being involved. When economists are doing or teaching theory *per se* or reporting the results of empirical work designed to test a particular aspect of theory, the theoretical style is stark, logical, formalized. In contrast, when economists are undertaking applied work that is of interest for policy reasons or are explaining, to an audience interested in that question *per se*, why certain economic events happened, theoretical ideas tend to be used less formally and more as a means of organizing analysis. These two different styles of theorizing we shall call *formal* and *appreciative*. Although they are quite different, both kinds of theorizing are necessary for economic understanding to progress satisfactorily, and there are strong if subtle connections between them.

The adherents of a broad theoretical structure share a way of looking at phenomena, a framework of appreciation. A theory defines the economic variables and the relationships that are important to understand, gives a language for discussing these, and provides a mode of acceptable explanation. Implicitly, therefore, a theory classifies some phenomena as peripheral, unimportant, and theoretically uninteresting; also it implicitly characterizes certain ways of talking about economic phenomena and certain kinds of explanations as ill-informed and unsophisticated.

In its role of providing a framework for appreciation, a theory is a tool of inquiry, and in skillful applied research that tool is used flexibly, bent to fit the problem, and complemented by any other tools that happen to be available and that appear to be useful. The focus is on the endeavor in which the theoretical tools are applied. In contrast, when economists or other scientists are pursuing the formal development of a theory, or undertaking empirical work as a specific check on theory, the focus is on improving or extending or corroborating the tool itself: they are exploring possible logical connections that have not been seen before, seeking implications of certain sets of assumptions, developing abstract parables that display possible causal mechanisms for particular phenomena, and trying to understand at an intuitive level the implications that seem to flow from deductive theorizing. In these activities, as contrasted with use of a theory as a framework of appreciation, the premium is on analytical tractability and power.

Formal and appreciative theory are linked in a number of ways.

Formal theory is an important source of the ideas invoked in appreciative theory. The formal theoretical enterprise extends and sharpens the tools used by the more empirically or policy-oriented members of the discipline. But in a well-working scientific discipline, the flow of influence is not only from formal to appreciative theorizing, but in the reverse direction as well. Phenomena identified in applied work that resist analysis with familiar models, and rather casual if perceptive explanations for these, become the grist for the formal theoretical mill. Formal theoretical structures are augmented so that the previously uninterpretable phenomena now have an interpretation. Somewhat informal explanations in the style of appreciative theory are abstracted, sharpened, and made more rigorous. These linkages also can be seen as constraints. In particular, if certain mathematical limitations prohibit formal theorizing from proceeding fruitfully in certain directions, appreciative theory tends to respond to the blockage too, and to be pulled where formal theory does proceed fruitfully.

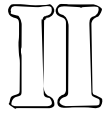
Marshall clearly recognized the distinction between these two different forms of theorizing and the desirability of close connections. So, albeit implicitly, has the economics profession at large. What probably was a binding constraint in Marshall's time on the range of analytically tractable styles of formal theorizing has played an extremely powerful role in determining how formal theory in economics has evolved, and has thereby shaped appreciative theory as well. But since Marshall's time, that constraint has been considerably relaxed. A wider range of mathematical knowledge has become available, including in particular the modern mathematical theory of stochastic processes. The stock of mathematical competence in the discipline is vastly larger than it was. The advent of the computer has made available the computer program as a type of formal theoretical statement, and simulation as a technique of theoretical exploration. These developments now make possible what Marshall obviously wanted but could not reasonably attempt with the mathematical tools he had then—the development of a formal evolutionary theory.

Our answer to the first question—why theory evolved along the lines it did—provides the basis for our answer to the second question—why the contemporary heterodox tradition in economics has had so little impact on thinking within the profession. In the appendix to *The New Industrial State*, Galbraith (1967) proposes his own answer to the question: the hostile reaction to heterodox ideas should be attributed to parochialism and (intellectual) vested interests. There certainly are parochialism and vested interests in the sense that the profession as a whole has an enormous stake in a coherent theoretical structure, that the prevailing structure provides a power-

ful if particular way of looking at things, and that it is hard to shift focus. But one could argue as well that the failure of the heterodox tradition to influence the profession stems from its lack of appreciation of the importance and nature of theory in economics. Heterodox critics also tend not to understand the varied and extremely flexible nature of prevailing theory.

Indeed, a major reason for heterodoxy's lack of influence is that many complaints or proposals can be accommodated by slight changes of meaning, treated and accommodated as special case models, or absorbed by broadening the theory somewhat, all with very few ripples. The fact that prevailing theory itself defines what are reasonable and sophisticated objections to prevailing theory and what distinguishes appropriate from inappropriate proposals for amendment or reform is another defense. It is employed primarily when the complaint seems uninteresting and unimportant, but tends to be used also in cases where the complaint is potentially important but not easily treated by marginal modifications of the theory. Thus, proposals that firms are interested in objectives other than profits are readily absorbed in special models and held at the periphery of orthodoxy. More general complaints that the theory of the firm does not adequately recognize the market-shaping activities of large corporations are absorbed into appreciative theory but not formal modeling, and the tension between appreciative and formal theory is ignored. But the proposal that such firms are governed by shifting coalitions and that therefore their objectives are not readily expressed in maximizing language is dismissed as ill-informed or atheoretical at the level of appreciative theory as well as formal theory.

If the contemporary critics of orthodox theory can be accused of not appreciating the importance of a coherent theoretical structure and of underestimating the resiliency and absorptive capacity of prevailing orthodox theory, the defenders of orthodoxy can be accused of trying to deny the importance of phenomena with which orthodox theory deals inadequately and at the same time overestimating the potential ability of models within the orthodox framework somehow to encompass these phenomena. Perhaps economists should be less pessimistic about the prospects of developing a broad-gauge economic theory that encompasses much of what contemporary orthodoxy does but is not subject to its basic difficulties.



ORGANIZATION-THEORETIC
FOUNDATIONS OF ECONOMIC
EVOLUTIONARY THEORY

3

The Foundations of Contemporary Orthodoxy

SYSTEMATIC UNDERSTANDING of the events that take place within individual business firms never has been a high-priority objective on most economists' research agendas. Rather, attention has been focused on the behavior of larger systems—industries, sectors, the national or global economy. To facilitate the task of addressing important questions about these larger systems, the individual organization has been treated in highly stylized terms that are dictated almost entirely by the functional role of the organization in the analysis at hand at the moment. Thus, the theoretical firm is not merely a "black box"—it is a black box whose input and output channels may be modified by assumption at the convenience of the investigator. Without apology, the individual economist may, in a series of inquiries, treat "firms" as choosers from very different sets of possible actions—for example, productive input combinations, price policies, and securities issues. That there are real organizations that actually do all these things more or less simultaneously is a fact that recedes into the background until it virtually disappears from view.

Our approach in this book is in many ways similar. The emphasis is on the analysis of the larger systems, not on the individual actors. And because the theoretical treatment of the latter is essentially instrumental to the investigation of other matters, that treatment is flexible and opportunistic in the traditional style. For the sake of logical precision in the analysis of a particular question about a larger system, we make strong simplifying assumptions in building a model addressed to that question; then, upon taking up a different

question, we may make quite different assumptions about the same matters. The justification for this apparently inconsistent approach is strictly pragmatic. It simply is not possible to keep any substantial number of the causal links of reality in sharp logical focus simultaneously. We can make such sharpness compatible with adequate scope only by attending to different parts separately and with different foci. The temporary narrowings of our field of vision are a price we must pay, given our unwillingness to abandon entirely the quest for logical precision.

It is our strongly held belief, however, that modeling at an industry- or an economy-wide level ought to be guided and constrained by a plausible theory of firm capabilities and behavior that is consistent with the microcosmic evidence. We argue in the present chapter that orthodox theory is inadequate in this respect, and in the following two chapters develop the view of events at the individual firm level that underlies our evolutionary theory. Although many of the considerations brought to light in this discussion will receive no explicit attention later in the book, we regard our specific modeling efforts as summarizing the main implications of our view of the micro level. They do so in a variety of different ways, each of which is appropriate to the task of understanding some particular class of events at a more aggregative level. We hope at least to persuade the reader that *if* the underlying realities correspond reasonably closely to the image here set forth, then the models presented in later chapters are useful ones to develop and explore.

Our first task is to get the issues out in the open. To this end, we undertake in the present chapter a critical survey of the conceptual foundations of orthodox economic theory. We identified in the first chapter a number of basic differences in underlying assumptions between orthodox theory and our proposed evolutionary one. Here it is useful to highlight the differences in presumptions made about the nature of the "know-how" possessed by business firms. Orthodox theory treats "knowing how to do" and "knowing how to choose" as very different things; we treat them as very similar. Orthodoxy assumes that somehow "knowledge of how to do" forms a clear set of possibilities bounded by sharp constraints, and that "knowledge of how to choose" somehow is sufficient so that choosing is done optimally; our position is that the range of things a firm can do at any time is always somewhat uncertain prior to the effort to exercise that capability, and that capabilities to make good choices in a particular situation may also be of uncertain effectiveness. The issues here involve the internal structure of the productive organization: What is really involved when an organization is "capable" of something? How does an organization remember its capabilities? What is in-

volved in “choosing” to do one thing rather than another? What kinds of capabilities are involved in choosing?

In Chapter 1 we also described the three basic building blocks of orthodox models of the firm: objectives, a set of things a firm knows how to do, and optimizing choice given those objectives and capabilities and other internal and external constraints. As the above questions suggest, our principal concern in this chapter will be with the latter two building blocks—in particular with the conceptions of human capabilities and behavior that seem to underlie them. We will set the stage in our discussion by considering a topic that has received more discussion in the economic literature: the sense in which business firms might be regarded as having objectives, and the question of where these objectives come from.

1. THE OBJECTIVES OF BUSINESS FIRMS

In the simplest orthodox model of business firms the objective is simply profit, or market value, and the more the better. But many scholars have qualified or questioned this simple specification. There have been efforts to shore up the standard formulation by detailing the linkage between owner interests and management actions. Objectives other than profit value have been proposed by some authors, while others have questioned whether firms have consistent objectives at all, in the sense of choice criteria representable by a scalar-valued function. The criticisms range from the highly heretical (such as Cyert and March on organizational goals) to the obviously orthodox (such as the recent literature on “stockholder unanimity”). Because of the scope and thoroughness of existing discussions in the literature, it is both impossible and unnecessary to review all the issues here; we attempt only to identify the major themes. There are, however, some aspects of orthodoxy’s treatment of the motivational sources of firm behavior that relate importantly to our concerns with the modeling of capabilities and that have received only limited attention in the literature. To these we will devote more attention.

The amount of effort that has been devoted to the problem of the objectives of the business firm can be regarded as indicative of the severity of the intellectual strain produced by two opposed considerations. On one side is the institutional fact of the large business organization—the sheer number of individuals involved, the diversity of their roles and the complexity of their relationships, the relative permanence of the organization and its concerns compared to the typical terms for which individuals serve as employees, stockholders, or even as chief executive officers. On the other side is the

individualist utilitarian philosophy underlying neoclassical economic theory, together with such specific manifestations thereof as the optimality theorems of modern welfare economics. In this philosophical framework, economic organization in its entirety is appraised for its effectiveness in satisfying the wants of individuals. *A fortiori*, the business firm is viewed as *in some sense* an instrumentality of individuals, rather than as an autonomous entity. If the business firm in question is Miller's Mill, there is no real problem in accommodating this need of the normative framework by assuming that the operations of the mill directly reflect the interests of Miller. If it is General Mills, a similar linkage between the actions of the firm and the interests of its owners remains "natural" for orthodox normative theory, but is of doubtful credibility for descriptive purposes. The strain becomes severe.

A variety of developments in contemporary orthodoxy are responsive, in one way or another, to the need to replace the "Miller's Mill" approach with something more plausible. All seem to involve heavy reliance on the categories and conclusions of *market* analysis to shore up the theory of the firm. In the general equilibrium theory and portfolio theory branches of the discussion, maximization of the market value of the firm is unambiguously the objective of the firm. The reason is that in the austere environment of complete and perfectly competitive markets, there is no alternative desideratum left against which the value of the firm might be traded off.

In another line of argument, with a slightly more plausible institutional façade, the "market" for the control of the firm is the one whose effective functioning keeps the firm in line. It is to the external discipline provided by the takeover raider, rather than the internal discipline imposed by Miller, that society looks for the effective functioning of the mill.¹ There have also been some tentative moves toward a view that is distinctive at once for its intellectual boldness and for its faithfulness to the individualist tradition—the view that the firm *is* a market, a particular pattern of voluntary exchange relations, and not a unitary actor at all. Whereas before it seemed that the mill was essentially one of the economic roles of Miller, now it is seen to be essentially an organized market in the nexus wheat, flour, grinding services, labor time, and so forth. In this perspective, relations between superior and subordinate within an organization appear indistinguishable from market-mediated relationships: "Telling an employee to type this letter rather than to file that document is like

1. For a discussion of this argument, see Williamson (1970, ch. 6). A recent formal treatment is O. D. Hart (1977), whose conclusions are for the most part negative with respect to the efficacy of the takeover discipline.

my telling a grocer to sell me this brand of tuna rather than that brand of bread" (Alchian and Demsetz, 1972, p. 777).

More radical suggestions for resolving the basic tensions in the theory of the firm have been put forward by a number of authors. These alternative approaches are distinguished, and marked as unorthodox, by a greater concern for "descriptive realism" in the treatment of the objectives of the large business firm and by a corresponding willingness to sacrifice contact with the normative branch of contemporary orthodoxy. One major camp, briefly discussed in Chapter 2, is that of the managerialists—those who argue that orthodox theory errs primarily by identifying the firm's interests with those of a constituency that is frequently quite passive (stockholders) rather than those of an obviously and necessarily active constituency (managers). Although managerialists have not fully agreed on an answer to the follow-on question—What, then, are the interests of the manager?—there is substantial consensus that some measure of the size or growth of the firm provides at least a partial *operational* answer to this question, and corresponds to one major area of possible divergence between the interests of stockholders and managers.² It has not escaped notice, however, that the pursuit of firm size as a long-run objective entails concern for profitability in the short run. Because of this linkage, and because managerialist analysis is typically conducted with analytical tools made familiar by orthodoxy, managerialism is in some ways a rather mild heresy. Perhaps it will be reassimilated to the main faith in some future ecumenical movement. It may come to be regarded as a refinement of rather than an alternative to the orthodox theory—a refinement that may become well established in certain rather narrow application areas, such as models of managerial consumption-on-the-job and certain problems of corporate finance.

Another heterodox approach, less sharply delineated than the managerialist school, denies that firm behavior can be interpreted as pursuit of the interest of a single dominant constituency. Rather, it sees behavior as the consequence of a bargaining process structured by shifting patterns of coalition formation. This view was put forward, in particular, by Cyert and March (1963). For them the "goals" or "objectives" of the firm cannot be characterized by an objective function of a grand optimization that imposes a coherent structure on all the firm's actions. In their view, the question of the firm's objective, in that sense, can never be resolved because it would involve too much time-consuming bargaining over too many hypothetical

2. See Marris (1964) and Baumol (1962), among others. Heal and Silberston (1972) present a simple analysis of alternative growth objectives.

choices. Rather, the firm persists in a state of "quasi-resolution of conflict," and the firm's goals may be conceived as akin to the terms of a treaty among the participants, according to which they will jointly seek to deal with their common environment. As in the case of treaties among nations, a shift in that environment may render the treaty obsolete, in which case a period of renewed negotiation or overt conflict may ensue.³

Even if shared interests and effective bargaining among top managers suffice to produce agreement on high-level objectives, divergent interests regarding implementation may still be a major factor in the concrete behavior of the firm. Objectives like profit, market share, or growth do not serve to guide action in the absence of specific understanding as to how they are to be achieved. Unless this understanding is obvious, shared by all those who are involved in decision making, even the deepest commitments to a common ultimate objective will not serve to focus attention and coordinate action. To serve this purpose, objectives must be articulated in such a way that they are relevant to the decisions at hand. The person responsible for deciding whether or not to repair a machine is afforded little help by his acquiescence in a general profit goal for the firm; he must have an objective defined in terms of the predictable consequences of his own actions. Put another way, objectives to guide action must be proximate, and specialized to the decisions in question. This suggests, on the one hand, that *choice* of operational objectives is an important arena of managerial *decision*. On the other hand, it prompts recognition of the abundant opportunities for conflict that inhere in the task of dividing operational responsibilities among middle managers, and in the elaboration of systems of control and incentive that are required to align the actions of low-level employees with high-level objectives.

In fact, the discussion in Cyert and March about quasi-resolution of conflict and the literature on divergence of interests between stockholders and managers represent only a small segment of a seriously neglected problem: the shaping role of intraorganizational conflict. Williamson, in his analysis of "opportunism" in the employment relation, has traced the outlines of a more substantial piece (1975, ch. 4). Doeringer and Piore (1971) have called the attention of economists to the role of internal labor markets in partially reconciling worker and manager interests. Economists have yet to concern

3. Although the business press frequently reports the internal policy struggles of large firms in a manner that clearly involves informal use of a coalition model, there is little scholarly literature in economics that takes this perspective. The proposals of March (1962) and Cyert and March (1963) have been largely ignored.

themselves with such things as managerial career systems and their possible implications for the time horizons affecting managerial choice, or for the willingness to cut losses when an undertaking or policy commitment is threatening to fail.

These considerations lead us to concur fully with Cyert and March on one major point: possession of a complete, clearly defined objective function is not a necessary condition for business operation in the real world; all that is required is a procedure for determining the action to be taken. While criteria for choosing form an important part of many such procedures, the criteria need not be derived from some global objective function. And it seems to us, as it did to them, that this proposition has an important corollary: the imputation of such an objective function to the firm is not a *sine qua non* of effective theory construction. Presumably, if the firms in the world can get along without being entirely clear about their goals, so can the firms in a theoretical model. The concern that orthodoxy has lavished on the question of objectives is a reflection of the logical imperatives of its own normative structure—and also, as we have suggested, of its aspiration to reach broad normative conclusions on the efficacy of market mechanisms. To discard that normative baggage is to greatly expand the available options for dealing with motivational issues in the theory of the firm.

Most of these options seem to fall under one or the other of two broad theoretical strategies. The first would restore, at the level of the individual organization member or subunit, the assumption of definite objectives that has been discarded at the level of the firm as a whole. It would then seek to understand the behavior of the firm as a whole in terms of the divergent interests of various constituencies and the specific procedures by which those interests interact to produce the actions of the firm as such. Some orthodox theorists, willing to grant the implausibility of treating large firms as unitary actors, might well concur with behavioralists on the general appropriateness of this reductionist strategy. They would differ sharply, of course, in the modeling of the procedures by which divergent interests interact: orthodoxy would favor some noncooperative game framework, while behavioralism would draw more heavily on insights from organization theory and studies of “bureaucratic politics.”⁴ In empirical application, both approaches suffer under limitations of access to data on the nature of constituent interests and on the structure of the internal political process—and also, when such

4. Allison's study of the Cuban missile crisis (Allison, 1971) includes a fascinating application of the “bureaucratic politics” approach to the explanation of a series of important decisions by the U.S. government.

access is possible, on the complexity of the phenomena and their relative remoteness from the crude and aggregative measures of overt firm behavior with which the economist typically wants to deal.

The second strategy is the one we adopt in our own modeling efforts, and in some ways lies closer to textbook orthodoxy. It seeks to capture with a few simple assumptions the most consistently operating and powerful motivational forces tending to shape the behavior of the firm as a whole. Recognizing that the real causal sources of firm actions do involve divergent interests and complex internal political processes, it nonetheless emphasizes the utility of a simple and tractable approximation that relates directly to the questions of interest, compared with a more elaborate and realistic treatment that risks inconclusiveness on those questions. However—and at this point we diverge from orthodoxy—this approach to business motivation does not warrant a great effort to assure that behavior is represented as being “perfectly prompt and rational.” On the contrary, in view of the nature of the deliberate approximation to the complex underlying reality, it is more natural to represent large-scale motivational forces as a kind of persistent pressure on decisions, a pressure to which the response is sluggish, halting, and sometimes inconsistent. And it may be noted that this is the same view of dominant motivational forces to which one is led if one regards them not as the result of an intellectual quest for perfect consistency, but as the outcome of an imprecise and unsubtle evolutionary purging of motives that diverge excessively from survival requirements. For problems that demand a more refined and exact treatment of business objectives, the appropriate tack is not to polish up the rationality with which the model firm pursues its imputed simple objective of profit or growth, but rather to recall that firms as such do not actually *have* objectives—that is, to revert to strategy one.

Most economists would, we suspect, readily concede the inadequacy of the conceptualization of the firm as a rational actor when the task is to explain particular decisions by particular large firms. The concession only underscores the question of why, in general theory construction, the objective function approach is so deeply entrenched. There are many other ways to represent motivational influences in a theoretical model; our own models illustrate only a few of the possibilities. In particular—as our own models illustrate—the plausible assumption that making money (in some sense) is a dominant business motivation need not be represented as profit or present value or market value maximization. The choice of those specific representations is easily understood as a response to demands for definiteness, precision, and internal consistency. But the source of those demands is not to be found in the realities of business

behavior. They are demands that economic theorists impose upon themselves, perhaps in the mistaken belief that the achievement of definiteness, precision, and internal consistency in the *theory* requires the imputation of the same traits to the subject matter.

2. PRODUCTION SETS AND ORGANIZATIONAL CAPABILITIES

Although, as indicated above, there has been extensive discussion in the economic literature about the motivational aspect of the theory of the firm, there has been startlingly little examination of the implicit theory of the capabilities of business firms that is employed as a key building block in orthodox theory.

The orthodox mode of formal representation of what an organization can do rests on the concept of a production set. The elements of the set are vectors of input and output quantities; to say that a vector is in the production set is to say that it corresponds to a productive transformation that the organization can accomplish. Or, as Debreu put it, "A given production y may be technically possible or technically impossible for the j th producer. The set Y of all production possible for the j th producer is called the production set" (Debreu, 1959, p. 38). Depending on the purpose of the inquiry, the fact that production processes take time may or may not receive explicit attention in the formal representation. Also, the basic formalism can, but need not, be elaborated to include detailed representation of the internal structure of the production process—for example, by including intermediate products in the list of commodities and by identifying production "activities" with particular stages in the production process.

The production set idea is very general, but traditionally, at least, the capabilities so described related to production of goods in the everyday sense of that term. A long tradition in economic writing suggests that "production" is the sort of thing that happens either on a farm (corn) or in an establishment in the metalworking branch of manufacturing (pins, widgets). In recent years, however, the range of capabilities to which economists have applied the production set idea has increased greatly. While it may be "obvious" that concepts introduced for corn and widget production are readily and appropriately transferable to furniture storage, haircuts, and vending machine services, it does seem that some anxiety might be justified concerning the extendability of the same apparatus to, for example, the services produced by attorneys, educators, psychiatrists, and parents. We shall attempt to articulate this anxiety later on. But for

the most part in this volume we adhere to tradition: when we speak of production capabilities, we have manufacturing prominently in mind.

What determines a firm's production set? Why is it what it is? On the surface, at least, orthodoxy is relatively clear about this. It is a state of knowledge that the production set is supposed to characterize—not, for example, the ultimate limits imposed by physical law, or the limits imposed by the actual conditions of input availability. Arrow and Hahn are quite explicit: "The production possibility set is a description of the state of the firm's knowledge about the possibilities of transforming commodities" (1971, p. 53).

What is the nature of this knowledge? Here the orthodox position is less plain. Considering the weight that this conceptualization of productive knowledge must bear in the overall structure of economic theory, the literature contains surprisingly little discussion intended to motivate and defend the approach. However, the connotation clearly is of knowledge "of a way of doing something" or "technological knowledge." Technological knowledge often is identified with a "book of blueprints" or with the knowledge of engineers and scientists. The latter is at least consistent with the view that specific operational knowledge exists in the context of theoretical understanding, while the "blueprints" metaphor suggests that knowledge is unitized, organized in packages labeled "all you need to know about X." Implicit in both metaphors, and in other discussions, is the view that technological knowledge is both articulable and articulated: you can look it up. At least, you could if you had the appropriate training.

Consistent with the notion of a book with a finite number of blueprints, in some treatments the production set is viewed as being generated by a finite number of activities or techniques that a firm knows how to operate. In the formal statement of models of this kind, certain assumptions generally are made about the characteristics of these individual activities—fixed input coefficients, constant returns to scale, and independence of other activities. The firm's production set then is defined as the input-output combinations achievable with all possible levels and mixes of the activities known to the firm. In other treatments economists simply assert certain characteristics of the set—for example, that the frontier of the set is described by a Cobb–Douglas production function. From either perspective, one important feature of the production set concept as it is employed is that, using our terms, a producer either has a capability or he does not. He knows how to run an activity or he does not; he has the blueprints or he does not. There are no fuzzy edges to the set, in fact or in mind.

The identification of a firm's production set with a "state of knowledge" could be interpreted as inviting consideration of a range of further questions. Why is the state of knowledge what it is? How does it change over time? Is it the same for all firms at a given time? For the most part, orthodoxy has declined to examine these issues.

In the standard treatment, the production set is simply taken as given. Issues of its change over time are not considered. The question of whether different firms have different production sets is not treated in a uniform way in orthodox models, but neither is it much discussed. In general, it appears that the most natural assumption within the orthodox framework is that all firms' production sets are identical—the blueprint file is a matter of public information. To make the sets different is implicitly to postulate positive costs of information transfer from firm to firm—a plausible view. But to make them different and *immutable*, as orthodoxy does when it takes this path, is implicitly to postulate that such costs are indefinitely large—an assumption that is clearly not in the spirit of the usual orthodox treatment of information.

The specialized literature on technical change forms, of course, a major exception to the proposition that production sets are viewed as constant over time. There, the typical model views the technological knowledge underlying the production set as changing over time as a result of "technological progress." In turn, technological progress may be viewed as exogenous, or as the consequence of a costly activity called "research and development." In effect R&D expenditure is treated as if it were purchases of an infinitely durable, indivisible fixed input ("knowledge") whose presence enhances the productivity of other inputs. Such formulations typically assume a total separability of R&D from actual production, in the sense that the expansion of the production set could take place even if production itself did not. This, of course, is consistent with the interpretation, noted above, that technological knowledge is articulated knowledge. It is the sort of thing that can be recorded, stored at negligible cost, and referred to when needed. The small group of "learning by doing" models depart from this tradition, but they remain an unconnected and unexplored annex to orthodox doctrine about production capabilities.

To the extent that different firms do different R&D and to the extent that there exist secure patent rights, or industrial secrecy, models that assume endogenous technological advance logically ought to admit that firms almost surely will differ in terms of their production sets. Strangely enough, however, virtually no extant model makes such an admission.

Consideration of the production set concept, as it is employed,

seems to us to raise three critical questions. If “technological knowledge” is what defines a firm’s capabilities, where in the firm does that knowledge reside? What rationale can be given for the presumption that there is a sharp boundary line between what a firm can and cannot do? How does the knowledge possessed by one firm relate to that possessed by others, and to the “state of knowledge” in the society generally? We consider these questions in turn.

Where does the knowledge reside? As we have already noted, two metaphors dominate the meager discussions in orthodox literature that seek to explicate the basic idea of technological knowledge possessed by a firm. One is the “symbolic records” metaphor—for example, the notion that the knowledge is stored in a blueprint file. The other is the “knowledge specialist” metaphor—for example, the idea that there is a “chief engineer” to whom the “entrepreneur” looks for a succinct account of the economically relevant aspects of the array of technical possibilities. Although both of these metaphors are suggestive of aspects of the real phenomenon of possession of capabilities by a firm, it seems clear that they are *merely* suggestive and fall far short of being an adequate account of the matter.

Engineering blueprints, and symbolic design records more generally, do not contain an exhaustive account of the methods involved in the actual exercise of a productive capability. As a matter of fact, blueprints often are quite gross descriptions of what to do, and seldom define a detailed job breakdown, much less provide “how to do it” instructions at the job level. As a matter of logical principle, it seems clear that a symbolic record could not provide an exhaustive account of the methods required for its own interpretation; rather, the use of such records presumes the availability of intelligent interpreters drawing on knowledge not contained in the records themselves. And as a matter of economics, cost considerations clearly limit the extent to which organizations maintain records of their methods and activities, and the records actually maintained are much less complete than they logically might be.

Similarly, the “chief engineer” metaphor is not viable. It seems inescapable that, in the typical and significant cases, the “knowledge” possessed by a firm is not possessed by any single individual within the firm. In the case of a manufacturing establishment of some size and sophistication, it would certainly be unusual if any single individual knew how to perform each and every task in the entire process. This is true even if the “tasks” involved are productive tasks in a narrow sense, and becomes more emphatically so if the tasks include control functions, maintenance, purchasing and marketing, and so on. Furthermore, the notion of a collection of describable “tasks” obviously falls far short of characterizing what the firm

as a functioning entity "knows." What it "knows" includes the system of coordinating relations among the tasks—the relations that combine the tasks into a productive performance.

Thus, the possession of technical "knowledge" is an attribute of the firm as a whole, as an organized entity, and is not reducible to what any single individual knows, or even to any simple aggregation of the various competencies and capabilities of all the various individuals, equipment, and installations of the firm. This observation conforms to the accounts in orthodox textbooks, which rarely mention the "chief engineer" or any other approach to the issues considered here. The usual textbook treatment ascribes the ability effectively to combine inputs to the firm itself, as an actor, and characterizes that ability by the production set. But this approach goes implausibly far: it abstracts the possession of capability *entirely* from the inputs. It postulates a latent capacity to organize that, being totally disembodied from that which is organized, resides in nothing. It would have us believe that there is such a thing as an automobile firm that owns no plant, hires no workers, and produces no automobiles, yet retains the capability to produce automobiles and is ready to do so at the whim of the market.⁵ To provide a plausible account of the relations between the capabilities of an organization and the capabilities of individual organization members, giving both the "reductionist" and the "holistic" viewpoints their due, is a major conceptual undertaking—and one that orthodoxy has not yet seriously attempted.

What real considerations could produce a sharp boundary between "technically possible" and "technically impossible" production activities? Certainly, there is no problem with saying that there are some things a firm can do and some it cannot. As an example of the former, we could point to something that the firm is actually doing, and for an example of the latter we could refer to some hypothetical process whose characteristics violate physical law. However, as we have noted, standard usage of the production set concept contemplates a set of intermediate size, a set including (in most cases) more than what is actually done, and (certainly) less than the full range of the physically possible. The boundary is the boundary of knowledge.

Whatever "knowledge" means in the organizational context, the

5. It is interesting that J. de V. Graaf, a thoughtful commentator on the interpretation of welfare economics, responded to this difficulty by rejecting the standard approach in favor of the view that "the ultimate repositories of technological knowledge in any society are the men comprising it." His attempt to reconstruct the theory on this basis was, we think, unconvincing, but the intellectual discomfort that motivated it was fully justified. See Graaf, *Theoretical Welfare Economics* (1957, p. 16).

state of knowledge is certainly subject to change. It is subject to change by deliberate choice, as when effort is exerted to discover the answer to a specific question, and it is subject to change by unchosen and unwelcome processes, as when an explosion or breakdown signals the infeasibility of an attempted course of action. It is subject to increase, as when production workers learn "by doing" to do their jobs more efficiently, and to decrease as workers forget the details of tasks they have not recently performed. It may be increased by means trivially cheap, such as a look at the Yellow Pages, or by expensive research and development, as in the design of a new computer system. It may be expanded by drawing on what others already know, as by reading reports or directly observing others' practice, or there may be an expansion of the limits of what is perceived to be physically possible. An attempt to improve it may be a matter of looking up the answer in a source known to contain the answer, or an extended search for a problem solution that may not exist.

Where, in all of these dimensions, are the discontinuities that could plausibly give rise to production sets with sharp boundaries? The production set approach seems to rest, albeit implicitly, on a claim that such discontinuities exist. Only on that assumption is it legitimate to consider the firm's position at the "knowledge margin" fixed while exploring the way changing conditions affect its adjustment at other margins. Only on that assumption does the logic of the firm's choice among known techniques, on which so much effort has been expended, relate to a real subject matter.

How does the knowledge possessed by one firm relate to that of others, and to the knowledge environment generally? As we have noted, the standard orthodox response to this question is simply to ignore it, and to take each firm's production set as "given." This position constitutes a powerful labor-saving device built into the structure of orthodox theory. In standard competitive models, it leaves market prices as the sole channel of causal influence linking the actions of different firms. It thus makes possible the decomposition of the problem of price and output determination into an optimization exercise at the firm level, with prices given, followed by an equilibrium analysis at the market level, with firm supply and demand schedules given. To recognize that nonprice information flows among firms are an important phenomenon is to forgo the intellectual economies afforded by this decomposable structure. But it is also to face reality.

The discussion above of the indefinite boundaries of a firm's knowledge touched briefly on some obvious ways in which firms can augment their own knowledge by reaching out into the environment—into their industry or into society more broadly. Information about the activities and methods of other firms can be ob-

tained by a variety of means—by buying and studying their products; by hiring away their technically expert employees; by reading accounts of their activities in trade journals, reports of securities analysts, and their mandatory filings with government agencies; by hiring consultants who work with the other firms of the industry as well; by reading copies of their patents or the publications of their research scientists; by overt purchase or exchange; or by covert schemes of industrial espionage. None of these methods are so cheap and effective as to make it plausible to assume that anything known to one firm is known to all. None are so expensive or ineffective as to justify an assumption that each individual firm is an island of technological knowledge, complete unto itself. And all of these methods are actually used.

Similarly, the firm can reach out through its R&D activity and otherwise, to the knowledge resources of the society at large. Its research scientists can read the publications of academic and government scientists, as well as those of other industrial researchers. It can learn from its suppliers and its customers. Performing R&D under government contract may provide an opportunity to learn things useful in its market-oriented activities. Acquisition of or merger with another firm can bring whole packages of capability under unified control. And again, these options vary widely in cost and effectiveness, and none are neglected.

Presumably there is no room for dispute concerning the existence of these phenomena, and little room for disputing their importance. Yet in orthodox economic modeling, they are either absent entirely, or, in discussions that admit technological change, treated in an awkward and inhibited fashion. We argued in Chapter 1 that the source of the inhibition is largely to be found in the orthodox commitments to optimization and equilibrium, but perhaps it derives also from an understandable reluctance to confront the complexities of a dynamically evolving, imperfectly defined state of knowledge that changes in response to the behavior of actors throughout the society. Our own efforts in this direction are set forth in Parts IV and V.

3. BEHAVIOR AS MAXIMIZING CHOICE

Given capabilities and objectives, the orthodox explanation of behavior—what firms do, given constraints—runs in terms of maximizing choice. The postulate that firm behavior results from maximizing choice leads the theorist to analyze an optimizing decision rule for the firm, a rule that maps from market conditions and other

variables external to the firm to the feasible action that scores highest on the firm's objective function. Both of the terms "maximizing" and "choice" warrant some scrutiny.

Simple textbook treatments generally presume that the actions taken by firms are truly *maximizing* in the sense that, given the circumstances, there are no better actions. However, we stressed earlier that recent sophisticated versions of the theory back off from that presumption. Lags between decision and effective action are recognized, along with the possibility that predictions of what the market will be are not perfect: maximization becomes maximization of expectation. That all potentially available information may not be fully exploited at decision time also is recognized. Maximization must be understood as recognizing information costs as well as other costs.

It is not clear whether the new most complex models of decision making with limited and costly information are intended to capture, as well, the fact of limited information-processing capacity, or the possibility that firms may be wrong in their understanding of the decision problems they face. Some economists seem to believe that models of maximizing behavior under limited information do adequately capture these more general implications of bounded rationality.

We think this is a misconception, and a serious one. In orthodox decision theory, the capacity to *process* information is invariably treated as costless and unlimited in amount; as Marschak and Radner explain, economic man is a perfect mathematician (Marschak and Radner, 1972, p. 315). Among other and more consequential implications, this says that the actors represented in economic theory already know all the theorems ("mere" logical truths) about their behavior that theorists struggle to prove. This affront to realism is not innocuous. It opens the door to full reliance on the notion of a fully preplanned behavior, even in contexts where the level of complexity involved is such as to overwhelm the aggregate capacity of Earth's computers. At the same time, it shuts the door on the study of devices that individuals and organizations actually employ to cope with their severe information-processing constraints—devices that often have a key influence on the actions taken. And it suppresses the role of the firm's own internal organization as a determinant of the effective level of uncertainty to which the firm's actions are subject.

Perhaps the most extensive evidence on this point comes not from the realm of economic activity, but from the history of intelligence failures in international relations. A consistent theme in retrospective studies is that failure occurs not because the intelligence system failed to acquire warning signals but because it failed to process, relate, and interpret those signals into a message relevant to available

choices.⁶ Difficult conceptual issues are involved in judging the extent to which such failures may be explained by "mistakes," "derections of duty," or "irrational behavior." But nothing could be more plainly relevant to their explanation than the fact that intelligence analysts and decision makers have only a limited amount of time each day, limited communication channels to connect their systems, and limited assistance in the task of organizing, analyzing, and *thinking about* the available information. Sometimes, highly "obvious" and emphatic signals get lost in the noise as a result of these limitations. We see no reason to think that economic decision making is any different in this regard.

There is similarly a fundamental difference between a situation in which a decision maker is uncertain about the state of *X* and a situation in which the decision maker has not given any thought to whether *X* matters or not, between a situation in which a prethought event judged of low probability occurs and a situation in which something occurs that never has been thought about, between judging an action unlikely to succeed and never thinking about an action. The latter situations in each pair are not adequately modeled in terms of low probabilities. Rather, they are not in the decision maker's considerations at all. To treat them calls for a theory of attention, not a theory that assumes that everything always is attended to but that some things are given little weight (for objective reasons).

In short, the most complex models of maximizing choice do not come to grips with the problem of bounded rationality. Only metaphorically can a "limited information" model be regarded as a model of decision with limited cognitive capacities. It is inadequate in many contexts because it does not explain or predict how a decision maker actually will behave: the metaphor is then nearly devoid of content. In fact, in most formal theorizing, the simple unsophisticated version of maximization is employed, perhaps augmented by partial recognition of limits on predictive capacities. The firm is visualized as truly optimizing its choices, given constraints and uncertainty.

We now turn our attention to the presumption that behavior is the result of *choice*. Contemporary appreciative theory is comfortably vague about what "choice" means, and the vagueness signals a problem with the concept. Sometimes "choice" refers to a process involving deliberation. But sometimes choice is understood to be involved in the following of a preassigned decision rule without deliberation, the decision rule itself in this usage presumed to be the result of ancestral deliberation. And in some of the more careful de-

6. In particular, this is a major theme in Roberta Wohlstetter's excellent study of the Pearl Harbor attack (Wohlstetter, 1962).

fenses of the theoretical use of optimization assumptions, there even is an admission that the firms may *never* go through any explicit calculating deliberation.

It seems useful to distinguish between processes for taking action that do involve a considerable amount of deliberation, and processes that involve more or less mechanical following of a decision rule. One might question whether the latter processes involve much real choosing using the everyday sense of that term. But, more important, if one knew that a certain class of action was the result of individuals following a prescribed decision rule, this would seem to be an interesting fact in itself, regardless of the provenance of the rule. Such information might lead the analyst to study, and perhaps model, the decision rule being employed. Indeed, if it is not assumed that the decision rule is truly a maximizing one, or one that is maximizing within the particular model of the firm being employed by an economist, this would seem the only way to proceed. The analyst might go on to analyze why the decision rule is what it is, the analysis involving some theory of decision rule creation and change. And, from this perspective, it would be interesting to go on to analyze the adequacy of prevailing decision rules and rule-change processes in terms of how well they enable the firm to cope with the circumstances it faces. That is, the decision rules employed by a firm ought to be regarded as an important part of its overall capabilities, in the same sense as the production activities in its production set. In our reading, this is not the perspective that orthodox theory takes regarding, for example, the pricing policies or advertising policies of firms.

As we shall elaborate in the next two chapters, a considerable portion of what is treated as "choice" in traditional theory indeed largely involves following prescribed decision rules. But this is not to deny that in many cases there is a certain—perhaps considerable—amount of deliberation involved. Again, if this is known, it is useful information. It is not useful as evidence in support of a theory that presumes that firms truly maximize something; the difficulty with this theory is the fact that, even if firms explicitly *try* to maximize, they cannot *truly* maximize. Rather, it is useful because it calls attention to the processes of deliberation. An analyst aiming to explain or predict action that is known to have come from processes involving considerable deliberation might want to exploit known aspects of deliberation processes in organizations. It is useful to list several of these.⁷

7. For a series of case studies that bring out a number of the points made in the following paragraphs, see March and Olsen (1976).

First, deliberative choice reflects a lack of complete preplanning adequate to the state of affairs. One deliberates about a choice because one has not thought through in advance what one would do under such circumstances, or, if such predeliberation has gone on, because for some reason the particular context has made the preplanning incomplete or inadequate for the present purposes. Deliberation signals problems or opportunities of the present status quo that were at least partly unanticipated.

Second, deliberative choice is contingent: its outcome depends on the special circumstances of the situation in which choices are made. In general, it is particular unanticipated problems or opportunities that trigger deliberation, and the deliberation is focused at least initially on these. But deliberative choice is likely to be influenced also by a broader set of particular circumstances.

Third, deliberative choice is fragmented. The temporal aspect of its fragmentation has already been noted, but in large organizations it is likely to be fragmented as well along lines of organizational authority and responsibility. A variety of differing information bases and organizational interests impinge on different aspects of the same interrelated decision problem. Commitments to a course of action may be made in one group or set of meetings, while crucial information on the risks or costs of that action resides, untapped, in another. The timing or compatibility of intended complementary actions may be deficient because responsibility is divided along functional or input-category lines, and within each such area of responsibility there are competing concerns that pull attention and effort away from the joint task. Warnings of unfavorable developments may suffer delay or distortion in communication to higher authority, because they may seem to reflect adversely on the performance of those charged with responsibility in the area in which the problems arise. These and similar categories of difficulties are the classic manifestations of the fragmentation of choice in large organizations, described by organization theorists from a variety of disciplinary perspectives. Theorists of *optimal* organization have made some progress in modeling informational fragmentation, less progress in modeling intraorganizational conflict, negligible progress in representing the realities of personal power and reputation—and have done nothing that departs from the basic assumption of the choice monad: the simultaneous confrontation of all constraints.

Finally, the occasions of choice are often opportunities for the clarification and elaboration of goals. Questions of “what we are trying to accomplish here” often come in for active consideration, not in the mode of logical deduction from premises accepted in the past, but rather in a mode that recognizes the specifics of the choice situation

as posing issues of general direction, balance, and tradeoff that had not hitherto been confronted. Since issues of this kind are raised and partially resolved in a sequential, contingent process of choice, there is a sense in which the objectives of an organization are a "path-dependent" historical phenomenon. Even if the underlying motivational picture is constant and starkly drawn—such as "We are in business to make money"—the delineation of objectives in terms sufficiently precise to inform choice is ordinarily deferred to an actual choice situation.

All of these facets indicate that deliberation is a form of economic activity in its own right, constrained by the scarcity of inputs and by the existing state of the "technology" of deliberation. Although the new sophisticated interpretation of maximizing behavior recognizes information costs, it remains committed to a sharp distinction between having and operating an activity or capability, and choosing an action. This fact accounts for some strikingly paradoxical features of orthodoxy's perspective on economic organization and economic change. An improvement in information-processing techniques that is linked to a metal-shaping device—for example, a numerically controlled machine tool—clearly falls under the "technological change" rubric and is quite typical of the sort of thing economists have in mind when they seek to measure technological change. By contrast, an information-processing improvement that is linked to a deliberative process—such as an econometric model of the firm's output market, or a linear programming procedure to help decide which factories should ship to which warehouses—is theoretically invisible to orthodoxy because it is part of the choice process. Similarly, orthodoxy seems incapable of recognizing that different firms may have different ways of making choices. These differences in the processes of deliberating ought to be a central part of the explanation of why firms make different choices.

Similarly, there is a process of implementation that follows real choice and is also a form of economic behavior in its own right, shaped by input scarcity and the state of implementation technology. For example, the choice of a price policy or pricing rule does not actually suffice to get the proper prices into the catalogs, onto the goods, and into the billing system. Sometimes, implementation costs may constitute a major factor in the choice of the price policy itself. The exercise of an organizational capability is involved in implementing a newly decided pricing policy for goods, just as much as in producing them. Similarly, specific capabilities are exercised in the actual carrying out of market transactions, in the processes of internal control, in record keeping, and so on. That these aspects of business behavior go virtually unnoticed in theoretical economics is

certainly not attributable to inhibitions about broadening the scope of the production concept: applications of that concept made in the analysis of health, education, and child rearing testify to the weakness of those inhibitions. Neither, certainly, is it the case that the issues involved are so trivial as to make explicit attention by managers or theorists unnecessary—consider, for example, the complexities of the problem of preventing embezzlement by computer. Rather, the reason the production-like aspects of implementation remain virtually hidden from orthodox eyes is that implementation, like deliberation, is so intimately related to choice—and choice is simply something done optimally.

The above discussion suggests that ability to deliberate and implement are elements of a firm's capabilities, just as is its command over a particular technical production process. But if this is so, the sharp separation in orthodox theory between capabilities and choosing becomes suspect. The processes of economic choice, like technical capabilities in a narrower sense, can undergo technological progress or regress. And the questions we have raised about the knowledge that underlies capabilities are as relevant to capabilities for choosing as they are to capabilities for producing. In particular, the proposition that the limits of a firm's capabilities are not sharply defined is relevant to both. A firm may be uncertain of its judgmental and deliberative competence in a given area of activity just as it may be uncertain about its technical competence, and a variety of ways of improving its capabilities are open to it.