

**Developing Evolutionary Theory
for Economics and Management**

by

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1. Introduction

In the spring of 1959, chance events led me to read a 1950 paper by Armen Alchian, entitled “Uncertainty, Evolution and Economic Theory” (Alchian 1950). At the time, I was trying to do a dissertation featuring an empirical analysis of the determinants of corporate spending on research and development. R&D had become quite a hot topic in applied economics after the mid-1950s. The theoretical framework that I had planned to use in this investigation was a model based on the familiar concept of the profit-maximizing firm, a core theoretical commitment of mainstream economics then and now. But, at the time of the fortuitous encounter with the Alchian paper, I had become concerned that my model of profit-maximizing R&D spending related to a decision situation that did not actually exist, at least not in any form resembling the context-free one that the model addressed.

Reading Alchian, I saw that an evolutionary approach on the theoretical front might offer a promising way to address satisfactorily a set of otherwise bothersome facts: 1) business discourse on R&D intensity seemed to be anchored on some notion of an appropriate R&D-to-sales ratio; 2) firm R&D decisions of any particular year were strongly shaped and constrained by decisions and their consequences from previous years; 3) incremental changes in policy nevertheless occurred, and had in fact accumulated over time into a pattern of significant and persistent *inter-industry* differences in R&D intensity; and 4) sustained pressures from the

economic and technological environment seemed to play a shaping role in the emergence of those inter-industry differences. Such was the starting point of my long odyssey with evolutionary thinking.

That personal journey is now half way through its fifth decade. More than three decades have passed since Richard Nelson and I published our first collaborative papers on evolutionary economics, and more than two since we presented a major statement of our theory in *An Evolutionary Theory of Economic Change* (Nelson and Winter 1982). Needless to say, there have been a number of significant twists and turns along the way. In particular, the opportunity to present this essay in a volume devoted to management theory reflects developments that certainly were not anticipated in the early stages. From its original status as a possible solution to my specific problem with R&D spending, the evolutionary approach quickly became the basis of an attempt at major reform in *economic* theory. That it remained, though the scope became even broader, as the collaboration with Nelson began. A contribution to *management* theory was not on the program.

Nevertheless, the logic of the connection to management is clear enough. As my subsequent discussion here explains, one of the key advantages of the evolutionary approach is that it offers liberation from overly stylized theoretical accounts of business behavior. Alternatively, one might say that the evolutionary approach embraces the realities of business decision making rather than shrinking defensively from them (exactly the choice posed in my encounter with the question of R&D spending). It thereby makes room for managers in the economic account of business behavior, and at the same time offers a style of economic thinking that is more interesting and potentially helpful to managers. In both directions of that traffic, the words “technology,” “organization,” and “change” are prominent, along with “management” and

“evolution.” A considerable portion of this promise has been realized, thanks in great part to the number of other scholars who have shared this vision, or pieces of it, and sought to bring it to realization. Major opportunities still lie before us.

In the remainder of this essay, I continue the story from the beginnings just described. My chosen structure is quasi-chronological, addressing major substantive areas in roughly the historical order in which they presented themselves to me. Since things didn’t actually develop in such discrete stages, there is a good deal of chronological disorder in the resulting picture.

The basic issues raised at the very beginning are as alive as they ever were. In particular, the realism-scorning methodological position that Milton Friedman (Friedman 1953, chapter 1) staked out remains, in practice, a core commitment of the economics discipline today, even if the actual citations to that essay are less commonly encountered than they once were. That commitment is, in turn, a major source of frustration to anyone who looks to mainstream economics in the reasonable hope that it might offer substantial help with the task of understanding how firm behavior shapes the economic system, or how managers shape firm behavior, or how technology and economic growth are shaping the future of the planet. The following section explores these basic issues, as consequential substantively as they are methodologically fundamental, which were focal in the early stages of my own work on the evolutionary approach. I turn next to the links between the evolutionary theory and the direct study of business behavior – as specifically represented by work in the Carnegie School tradition of Herbert Simon, James March, and Richard Cyert. Section 4 then introduces the connections to technical change, and hence to economic growth and development. That inquiry points to still broader issues about how one thinks of the role of knowledge in productive activity, addressed in Section 5. The penultimate section discusses some of the key empirical issues –

those that are in dispute with mainstream economics or with other prominent schools of thought, and those that are key simply because of their central place in the evolutionary argument. The final section argues that the evolutionary approach offers a style for doing economic theory that has a good fit to the needs of the management discipline.

2. “Realism,” Maximization and the Theory of the Firm

The Friedman paper mentioned above soon supplanted the Alchian paper as the main focus of my early thinking about economic evolution, but Alchian’s work remained a fundamental guide in one key respect. Alchian had proposed a *reconstruction* of economic theory on evolutionary principles, and plausibly sketched some key elements of such a program. That idea appealed to me, but it certainly was not what Friedman was up to.¹

Friedman’s essay, “The Methodology of Positive Economics,” appeared as the first chapter of his *Essays in Positive Economics* (Friedman 1953). In large part, it was Friedman’s response to a lively scholarly controversy about the profit maximization assumption that had emerged in the 1940s. The critics complained that the assumption was not realistic, and some of them cited evidence from close-in observation of business behavior to back their claims.² Friedman argued that the critics suffered from a simplistic understanding of what “realism” meant in science. He also put forward arguments about why profit maximization might be a “fruitful hypothesis” in spite of apparent conflicts with direct observation – scorning the latter with the comment “A fundamental hypothesis of science is that appearances are deceptive”

¹ An argument that Friedman’s evolutionary insights *should* imply reconstruction was actually made by Tjalling Koopmans, a much-admired mathematical economist who was a professor of mine at Yale (Koopmans 1957), pp. 140-41. I do not recall reading that passage in Koopmans before I read Alchian – but I might not have reacted, even if I did.

² A good example is (Gordon 1948), which cites a lot of the other relevant work.

(p. 33). One of his supportive arguments for profit maximization as a scientific hypothesis was an evolutionary “natural selection” argument that concluded with these words:

“The process of ‘natural selection’ thus helps to validate the hypothesis – or rather, given natural selection, acceptance of the hypothesis can be based largely on the judgment that it summarizes appropriately the conditions for survival.” (p. 22)

The critical assessment of this proposition – which I have come to call “the Friedman conjecture” -- became the central theme of my dissertation research, at a rather late stage in the year that I was supposedly devoting to the dissertation. The study of corporate R&D spending was never completed; the theoretical puzzle it presented was recast as an example of a much larger puzzle about the general representation of business behavior in economic theory, and about profit maximization in particular. The topics of R&D and technological change were set aside, but the early concern with these issues was a portent of things to come in the development of evolutionary economics.

As Friedman’s essay explained quite well, every science faces the challenge of finding ways to make its theoretical concepts operational, thus building a bridge from a theory to a set of facts that might be expected to throw light on the merit of the theory. Just how this “light-throwing” works is not obvious. It is actually a deep and sometimes contentious issue, though elementary accounts of the scientific method often posit a simple and reassuring answer. One particular puzzle concerns the appropriateness of leaving a theoretical term without any direct empirical reference of its own, so that it serves only as a convenient place-holder in a longer argument that engages observable reality at some distant point. Friedman’s position was that the notion of “profit maximization” in economic theory was a theoretical term of this kind: what the

theory says, per Friedman, is that firms behave *as if* they maximize profits. Hence, mounting an effort to examine firm decision-making at close range is simply misguided (as economic science), because economic theory makes no real prediction as to what you should expect to find. Friedman suggested that other processes – such as “natural selection” or tacit skill – might create the observable consequences of profit maximization.³ This could be happening even if the maximization itself -- in the sense of clear objectives, explicit calculation and careful comparison of alternatives -- were not only unobservable, but absent. He also expressed skepticism about the possibility of discovering how business decisions are made through observation or interviews, suggesting that respondents might dissemble in some way or perhaps were actually not consciously aware of the mental processes involved (the tacit skill point). For example,

“The billiard player, if asked how he decides where to hit the ball, may say that he ‘just figures it out’ but then also rubs a rabbit’s foot just to make sure; and the businessman may well say that he prices at average cost, with of course some minor deviations when the market makes it necessary. The one statement is about as helpful as the other, and neither is a relevant test of the associated (maximization) hypothesis.” (Friedman, 1953, p. 22)

This skepticism about the value of direct observation of firms is by no means peculiar to Friedman, or to those who are explicitly committed to something like his methodological outlook. It remains a broadly held attitude in the economics discipline, though perhaps not so broadly as when Friedman wrote. Anyone who undertakes a direct approach to studying firm behavior is sure to encounter it, sooner rather than later, when discussing the project with

³ Friedman did not use the terminology of “tacit skill,” but it seems fully appropriate in retrospect.

economists.⁴ To be clear, there certainly is merit in warning against the possibility that respondents are dissembling, or reporting socially approved motivations and procedures, or exercising tacit skills that they cannot explicate effectively. These points are familiar and accepted in social science research, and for that matter are widely relevant in everyday life. What is distinctive about the response often encountered from economists is its extreme and unqualified nature. Instead of being the beginning of a discussion of how likely it actually is, given the actual context, that the results are tainted in these ways, it tends to be offered as the end of the discussion – both for the present and the foreseeable future.

The methodological issues surrounding profit maximization have rough parallels in other sciences. The case of the neutrino is a classic of the type. When originally proposed, the new particle appeared to be nothing more than an *ex post* adjustment to prevailing physical theory to protect it from apparently disconfirming observations. Even the proposer, Wolfgang Pauli, referred to the proposal as a “desperate expedient.” As a patch to the theory, the neutrino seemed to have the disturbing property that it was apparently impossible to check its validity, since the assumed properties of zero mass and zero charge posed a major obstacle to observation. Thus, paralleling the case of “as if” profit maximization, the proposed patch was put forward in a context of cogent reasoning as to why it was impossible to check on its validity. Physicists and philosophers debated the legitimacy of the neutrino patch for some decades – after which the question faded, as first indirect and then relatively direct confirming evidence was developed.

Closer to home (i.e., management theory), a similar dispute exists concerning concepts of “legitimation” and “legitimacy” in organizational ecology. Given the unquestioned success in accumulating a mass of indirect statistical evidence (said to be) indicative of a significant role for

⁴ For a recent example, see Truman Bewley’s discussion of these attitudes, which he encountered in connection with his interview-based study of why firms don’t cut wages in recession (Bewley 1999), esp. pp. 8-16. More generally, see also (Schwartz 1998)

legitimacy in the evolution of organizational populations, is it reasonable to ask for new kinds of measurements that would go more directly to the concept of legitimacy as that concept is understood in sociology, or even more broadly? Perhaps, but perhaps not. (See (Hannan and Carroll 1995), accusing critics (Baum and Powell 1995) of “cheap talk.”).

When a mechanism or entity featured in a theory is declared “off limits” to observation, suspicions generally arise that this declaration might be nothing more than a convenient device to protect against some unwelcome observations that might be considered threatening to the theory. Such devices are objectionable on the ground that continuing recourse to them would ultimately deprive the theory of all empirical content, turning it into a mere tautology. There are also, however, reasonable grounds for tolerating the use of such *ad hoc* and *ex post* adjustments, at least on an occasional and temporary basis. First, given that theories typically have nothing very sharp to say about the appropriate steps for making them operational, it is clear that any *specific* method of observation generally lacks a clear, theoretically-grounded claim to appropriateness. Apparent trouble for the theory could therefore represent nothing more than an empirical technique that is flawed in the sense (at least) that it is not precisely what the theory requires. Second, it is hardly plausible to suggest that a useful and broadly accurate theory should be abandoned merely because it conflicts with some particular observations, especially if no viable alternative theory is available at the moment. The second point is plainly supported by the first; it would be particularly short-sighted to let a useful theory fall victim to bad or irrelevant observations. Friedman’s essay involves both of these general points. His argument, however, seems go far beyond objecting to the relevance of some actual observations of firm decision making, and perhaps extends even to the extreme claim that no *conceivable* direct observations of firm decision-making could legitimately cast doubt on the maximization hypothesis.

The deep issues involved here have long received great attention in the philosophy of science, consider for example (Popper 1959; Kuhn 1970; Quine, 1961) and (Lakatos 1970). In the substantial economics literature on these matters, much of the discussion has focused on the Friedman essay specifically – and has had the peculiar feature of making little reference to the broader discussion while at the same time making considerable use of examples from physical theory. The particularly valuable contributions, in my view, include (Massey 1965; Blaug 1980). My own comments on the methodological issues have largely been incidental to discussion of the theory of the firm; see in particular (Winter 1964a; 1975; 1986a; 1986b; 1987). It is not my purpose here to further explore the general methodological questions.

In the interest of clarity, however, I should declare where I stand on the specific issue of profit maximization. It does seem clear to me that the idea of “as if” maximization, along with its associated constellation of highly skeptical attitudes regarding the value of direct observation, is basically a defensive maneuver that serves to protect a seriously flawed theory. In my view, the theory thus defended is not actually supported by any compelling evidence – although, as I discuss below, understanding why the allegedly supporting evidence is not probative does require a careful parsing of the issues at stake. Since business decisions are manifestly a key part of the functioning of the economic system, the strong disciplinary commitment to analyzing them on the basis of mistaken theoretical premises is a large obstacle to scientific progress. This assessment of mine is hardly idiosyncratic. It is widely shared among social scientists and business people, whose work often leads them much closer to business decision making than most academic economists ever get.⁵ Like many of these other observers from outside of

⁵ Criticism of the profit maximization assumption, or of rational choice models more broadly, is a perennial feature of economic discourse. Since I began my own engagement with these issues, particularly influential academic criticism has come from psychologists such as Amos Tversky, Daniel Kahneman, Paul Slovic, George Lowenstein and Robyn Dawes. Among economists who have taken some part of the criticism seriously (although not

mainstream economics, I seem to encounter evidence of the negative consequences quite frequently.

The parsing of the issues begins with considering the relevance of the mass of statistical evidence that supports the qualitative predictions of standard economic theory – “supply curves are upward sloping” is the prototype here. This sort of evidence does not actually discriminate between the profit maximization hypothesis and plausible alternative behaviors. Indeed, this in a sense was Friedman’s point – some things do indeed happen “as if” there were profit maximization, thereby producing a spurious impression of true maximization at work. Not all things happen that way, however. Empirical discrimination between (true, causally fundamental) maximization and the alternatives is generally quite possible, with details depending on the precise formulations that we are talking about on both sides. (Of course, the really obvious opportunity for such discrimination lies in – direct observation of decision making!) The second key point is to recognize that the dispute is not about the motivational claim that business firms and individuals are often, or generally, “trying to make money”. That claim has, by itself, no empirical content. If, for example, this “trying” is afflicted with a lot of randomness and erratic adherence to superstitious belief (as Friedman also suggested), the logic by which the usual qualitative predictions might somehow follow has not been adduced. Acknowledging that other motivations might also be at work will generally make this impasse worse.

The point that requires emphasis here is that the characteristic predictions of mainstream theory are not the implications of the motivational assumption alone, but of that assumption plus

identifying fully with the evolutionary view), prominent names include George Akerlof, John Conlisk, Richard Day, David Laibson, Roy Radner, Robert Shiller, and Richard Thaler. The rationality of *organizational* behavior has received little attention from these authors, however. Most of the attention has gone to individual behavior, or to market phenomena that directly reflect it. (Schwartz 1998) is a useful and wide-ranging survey of the literature.

constant constraints (opportunity sets) plus true maximization – the actors consistently get it right! That last is the claim that is centrally at issue.⁶

To conclude the parsing, I note that critics like myself do not have to burden ourselves with the extreme claim that nothing resembling true maximization is ever found in business behavior. Due partly to the normative role of standard economics, but probably more to the practical value of operations research, that claim is far from correct. There is some tendency for real actors to enact the theories that economists have about them, or at least to try to. A complete picture has to include this piece, and evolutionary economics does include it. The contextual factors that favor the appearance of these pockets of “true maximization” are an interesting object of study. One key practical consideration obviously plays a major role in setting a favorable context: the data required for a systematic comparison of alternative policies are actually available. Beyond that, it does not seem that the context is typically ruled by narrowly economic considerations. As a result, such studies require the tools of sociology as much as those of economics (see, e.g., (Beunza and Stark 2004)).

If it were possible to address the economics of the firm and industry in a way that avoided fundamental commitment to fictions about decision making, would that be desirable? The discussion above only sketches some of the relevant points. It suggests, contrary to Friedman’s classic argument, that the answer should be in the affirmative. But is it in fact possible? Somewhat paradoxically, Friedman’s case for “as if” maximization contains key elements of a program that dispenses with maximization (as a *fundamental* postulate) altogether.⁷ Those

⁶ The theory of rational choice says that actors do not make *ex ante* mistakes, but it can readily acknowledge the reality of *ex post* mistakes. To fend off a reply along this line, we have to begin by accepting one point about “they always get it right”: it does depend what you mean by “it.” Addressing this qualification in a careful way makes the necessary argument longer, but does not basically change the conclusion: to the extent that the theory has predictive content, its predictions derive in crucial part from the assumption that the actors get it right.

⁷ Displacing maximization from the foundations of the theory does not mean discarding it entirely from the theoretical tool box. Some discussion of Friedman, featuring the term “instrumentalism,” essentially frames the

elements, however, are plainly insufficient to define the needed program. A commitment to greater “realism” clearly entails a more substantial concern with characterizing reality. But, what is that reality?

3. From the “Friedman Conjecture” to the “The Carnegie School”

Defining the stakes. Neither in management nor in public policy analysis is there real interest in discussing whether business decisions deviate *by minor amounts* from norms of perfection, such as those that those theoretical economics conventionally offers. Also, in neither of those scholarly communities could you round up a patient audience for an argument that the important deficiencies in decision making are primarily attributable to *a widespread deficiency of myopic greed*. Rather, there is a broad consensus that levels of myopic greed tend to be on the high side – assuredly from the public policy viewpoint, but, in many cases, even from the viewpoint of the long-term self-interest of the actor. Combining these two observations, we reach a conclusion that can be expressed in terms of the familiar “money left on the table” metaphor: it is not small change that we are talking about here, and evidently the real money must be hidden under the tablecloth or somewhere. For, at least in the historical and cultural circumstances of advanced economies today, we simply do not put much credence in the suggestion that serious money is sitting there unclaimed on the table, in full view. Consequential decision-making failures involve substantial stakes, and a satisfying explanation for such failures includes an account of the

dispute as being about the tool box, not the theory (see (Boland 1979)). In my view, a theory involves commitments about the nature of reality that go beyond specific considerations of instrumental effectiveness. A good “engineering approximation” (for a particular context) may be a very poor theory (in general). See (Friedman 1953, pp. 17-19) on how bodies fall “as if” in a vacuum.). A good theory suggests useful advice about engineering approximations, and a good theory of the firm would illuminate when profit maximization is a reasonable working assumption and when not.

sources of flawed perception on the part of decision makers – not an assumption of willful indifference to large stakes.

The practical questions thus delimited are, unfortunately, difficult ones. In particular, an adequate assessment of what might be obscuring the decision makers' view (playing the role of the tablecloth) would have to take into account a wide range of considerations that have been identified and discussed in social science literature, from individual-level cognitive limitations through social pressures in groups to system-level coordination issues. These various failure mechanisms fall within the domains of different social science disciplines, and also cut across them. In short, the practical questions do not constitute practical objectives for research, at least in any direct, near-term sense. More limited objectives are needed, ones that can be pursued via identifiable research approaches.

The Friedman conjecture. Theoretical analysis of the Friedman conjecture is one such approach. Essentially, the question is whether money will be left on the table in the long run if it is being pursued by profit-seeking firms with plausible, though typically not optimal, policies. In its basic form, such analysis first posits a situation in which it is logically possible for business firms to get the right answers to their decision problems, for at least there *is* a right answer. (Without this very substantial assumption, the Friedman conjecture is dead on arrival as a matter of strict logic.) The second constituent of the analysis is some postulated set of possible behavior patterns for firms, such that at least some of these patterns are not comprehensively optimal. That is, contrary to the standard assumptions of economics, not all firms are necessarily getting the right answer all the time. (Without this assumption, the conclusion “firms maximize profits” is the trivial result of the familiar postulate, requiring no evolutionary logic or process to establish it.) The final constituent is a characterization of the dynamic process by which firms

interact competitively, determining their survival and growth. With the details of a hypothetical context thus specified, the problem of such analysis is to characterize how the dynamic process turns out, and whether this outcome is consistent with Friedman's conjecture of "as if" profit maximization.

To consider a simple example,⁸ suppose all firms in an industry base their capacity investment decisions on a firm specific, aspiration-level rate of return that (for some firms at least) is higher than the market cost of capital they all face. This behavior does not automatically align with maximization of profit (or, here, net present value), since it can imply leaving money on the table in the form of positive NPV investments that are passed up. To this modestly non-standard behavioral assumption add the standard ingredients of an economic model of a competitive industry, and let the situation unfold over time. If firms are otherwise identical, this situation is essentially a long-run competition in which the lowest aspiration-level rate of return wins.

This outcome scores as a partial victory for the Friedman conjecture, in the sense that the industry-level outcome is the standard long run competitive outcome provided *some* firms aspire only to cover the cost of capital.⁹ The assumption "all firms maximize profits" has thus been effectively weakened to "some firms maximize profit," and the remainder of the work has been done by the evolutionary process, producing an outcome "as if all firms maximize profits". It is only a *partial* victory because, first, we do need the "some firms" assumption. Without it the same model can just as well illustrate the point that the evolutionary process may deliver non-

⁸ Even this example is not as simple as it is here pretended to be, for the sake of brevity. For more painstaking discussion of this kind of exercise see (Winter 1964a; 1971; 1987; 1990); Hodgson 1994; Nelson and Winter 1982, chapter 6).

⁹ There is a cluster of complications here around the issue of whether firms act strictly as "price-takers" or can perceive their market power and act accordingly. Allowing for these does not change the moral of the story any, so I ignore them and rely on the standard logic of competition.

standard outcomes. Second, it is a *long run* outcome; it takes time for evolution to do its work. This raises the question: suppose exogenous change occurs intermittently, do we have to wait for a new evolutionary process to do its work every time the game is changed? Or is there basically only one competition in spite of the changes? In this model there is only one, but this is not a general result.¹⁰ Finally, there is a crucial, understated assumption of *stability* implicit in the behavior patterns assumed. The aspiration-level rate-of-return is a durable “quasi-genetic trait.”

The example just discussed illustrates the general flavor of the analysis that appeared in my early dissertation-based article (Winter 1964a). Its focal concern is with the logic of the Friedman conjecture. It shows on the one hand that it is possible to spell out a logical basis that converts Friedman’s intuition into a theorem. On the other hand, the “audit” provided by this explicit formal modeling points out considerations that limit the real significance of the result. While the considerations identified above are relevant to a broad range of such models, there are other important ones that are not in view because of the simplicity of the example – e.g., issues involving exit and entry processes, or the consequences of multi- rather than single-dimensional heterogeneity in the “genetic” attributes of firms, or the implications of search processes that modify those attributes over time. The latter considerations also interact with the former, creating a complex variety of specific situations and corresponding answers regarding the conjecture. Finally, there is the very important and general “rules vs. actions” problem. An evolutionary contest among firms whose actions are rule-based cannot test the optimality of the responses the rules offer in environments that never appear, or appear rarely, in the course of the contest. Hence, even a result that confirms the Friedman conjecture with respect to *actions* in the long run cannot possibly confirm it with respect to *rules* (Winter 1964a)

¹⁰ That is, given a common environment and firms that are all identical except in aspirations and scale (capacity), the growth rates of firms are always ranked inversely to the aspiration-level rates of return, regardless of what else may be affecting them in the common environment.

What there is to be learned from this type of inquiry cannot be learned from studying any single model, and consists largely in a sharpened understanding of the range of relevant mechanisms and their interactions, plus an enhanced appreciation that the real import of the conclusions depends on quantitative aspects that the qualitative analysis suppresses.

As the above summary suggests, my early work on the Friedman conjecture did not involve a serious attempt to answer the question about the reality of firm decision making – beyond the clear reality of “not nearly as perfect as usually assumed in economics.” The subsequent development of evolutionary economics involved downplaying the Friedman conjecture as a focal issue and, instead, turning up the light on reality. Substantial illumination was drawn from several different sources, of which a key one was the behavioral theory of the firm.

Behavioralism. At the time I was beginning my dissertation research, the “Carnegie School” was reaching an advanced stage of development in Pittsburgh. Herbert Simon’s famous article on satisficing, “A Behavioral Model of Rational Choice,” had appeared in 1955 (Simon 1955), and I had had the good fortune to encounter it in graduate school.¹¹ The classic *Organizations* volume by Simon and James March appeared in 1958 (March and Simon 1958). Much of the research that in 1963 appeared as the Richard Cyert and James March book, *A Behavioral Theory of the Firm* (Cyert and March 1963), was under way and was beginning to appear in working paper form. What the Carnegie scholars had to say about firm behavior was partly familiar, being in some ways parallel to what had been said earlier by the economists who criticized the orthodoxy in the theory of the firm. These were the very critics to whom Friedman responded in his essay, and I was well aware of their work. In retrospect, it may appear that

¹¹ I doubt that Simon’s article ever made an appearance on many reading lists for economics courses, and certainly not by 1957. But it was on the list for Jacob Marschak’s seminar on Economics of Information and Organization, which I took at Yale in that year. Even the title of Marschak’s seminar now seems quite remarkable, given the date.

even at that early stage there was an evident opportunity to use an evolutionary approach to build on and complement the “micro-foundations” of firm behavior contributed by the Carnegie School.

In fact, that did not happen – at the time. There was some cross-fertilization, and some sense of encouragement (at least in the Carnegie-to-Winter direction), but not much. The “behavioral theory of the firm” was not easy to absorb, especially in its unfinished form. It involved novel theory, novel research techniques (especially computer simulation) and novel-seeming blind spots (especially, an apparent indifference to the role of markets as understood by economists).

When the Cyert and March book appeared in 1963, I was invited to review it for the *American Economic Review* (Winter 1964b). In the course of reading the book and preparing the review, I was able to see the Carnegie work as a *program* for the first time – and to see it as complementary to the evolutionary approach, as suggested above. My review noted that the authors seemed content to regard firm behavior as a significant scientific problem in its own right, and willing therefore to set aside the task of predicting market phenomena – and suggested that this should not be the permanent state of affairs:

“Also, it is to be hoped that someone will eventually accept the challenge of attempting to provide a better definition of the relationship between the behavioral theory and the traditional theory than is provided by the assertion that the two theories are concerned with different problems....”

“ ... the consistency of the behavioral theory with the more persuasive portion of the *empirical evidence* for the traditional theory has yet to be determined.

Investigation of the relationship between the two theories will probably involve

closer attention to the circumstances that determine when the profit goal is evoked and when profit aspirations adjust upward, as well as to the ways in which competition may force an approach to profit maximization by firms whose decision processes are governed in the short run by crude rule-of-thumb decision rules.”

-- (Winter 1964b), p. 147 (emphasis in original)

Although it was not fully spelled out in my review, any more than in the book itself, I could see that the Cyert and March book suggested the possibility of a new division of scientific labor. Firm behavior could be regarded as a subject matter in its own right, which on the face of it appeared to involve aspects appropriately studied in psychology, sociology, organizational behavior, engineering, operations research, management, finance, accounting, marketing, and perhaps other disciplines as well, in addition to economics. The primary role of economics was not to strive for imperial control over these other intellectual domains, and certainly not to ignore them, but to point out the systemic and long-run implications of whatever firm-level truths might be brought forward, from whatever source. This role is especially suitable for economists insofar as those implications are largely the result of firms interacting through markets. At the same time, operations research and the business-oriented disciplines might reasonably concern themselves (at least in part) with how existing modes of business behavior might realistically be improved— and that, too, is not the central role of economics. This vision of the appropriate division of labor represents my present view.

Given this view of the general relationship of economics to business behavior, one can identify specific analytical tasks of the following kind. Take any empirical pattern of business behavior that has been identified and alleged to be a general phenomenon, and analyze its

survival prospects in an evolutionary contest among similar firms. Such analysis begins by positing that the identified pattern is widespread, but acknowledging also that market discipline generally provides real constraint in the long run. The required analysis is particularly feasible and informative if the “identified pattern” involves some relatively simple rule-governed behavior. As previously noted, the claim that behavior often takes that form was a prominent part of the behavioralist position, and in fact the specific example of mark-up pricing behavior had been prominent in the whole controversy about profit maximization since before World War II (Hall and Hitch 1939). Recall also that my own early work on corporate R&D spending involved the puzzle presented by precisely this sort of observation: the research-to-sales ratio functions as a decision rule. There was, (and remains), a substantial backlog of such generalizations about behavior patterns to which the proposed type of analysis is relevant.

The task of such analysis is to determine whether, or under what conditions, the identified pattern can survive the constraints imposed by market discipline in the long run – especially if its practitioners are challenged by otherwise similar firms who behave, in this particular domain, according to plausible rules that are seemingly more “rational.” It can happen that such an analysis yields the conclusion “under no conditions,” i.e., that the behavior pattern is inevitably selected against in the long run. This implies either that the pattern is in fact a temporary aberration, or the pattern itself has been mis-characterized, or the force of market discipline has been overstated.

More often, the conclusions have a different tendency, suggesting that there are particular environments where the observed pattern might be viable. Consider the following general pattern, for which specific examples are easily found: firms are lavish in their use of input X; they essentially behave as if it were free. In an environment where input X is indeed

(approximately) free, this behavior imposes a negligible burden. It cannot be competed out of existence by rivals who are more circumspect in the use of X, unless these rivals tend also to be otherwise advantaged. (What will happen if the price of X increases dramatically, so that its cost share is no longer trivial? The rules vs. actions problem arises here: is there an underlying behavioral rule connecting the use of X to its price? Evolutionary processes do not cannot guarantee that.)

If being “more circumspect” itself entails a modest cost, these rivals may in fact be *disadvantaged*. – providing an instance of the general proposition, “it doesn’t pay to pay attention to things that don’t matter.” This proposition provides a ready interpretation of much behavior that appears, in the logic of its *form*, to defy considerations of efficiency or cost (e.g., leaving the office supplies cabinet unlocked). It re-directs attention to the substantive consequences – particularly those bearing on organizational growth and survival.

Thus, the work at Carnegie provided important support for the notion that parts of business behavior are based on simple rules. This provided some specific fodder for theoretical analysis in the evolutionary style, somewhat paralleling the logical analysis of the Friedman conjecture but having a more explicit grounding in behavioral reality. More importantly, it also underscored the point that a firm-level, empirically-based component of the theory was needed to complement the predominantly long-run, system-level insights of the evolutionary approach. Indeed, such a component was not only needed, but at least to some extent the Carnegie work had made it available. “Simple rules” were, of course, only part of the Carnegie story. For example, the concepts of “problemistic search” and “quasi-resolution of conflict” effected permanent changes in the lenses through which I, and many others, viewed business behavior.

Above all, the concept of satisficing behavior became a tool of pervasive relevance to evolutionary thinking about organizations.

Simon's development of satisficing placed the concept in the context of costly search of some set of potential problem solutions. It was quite clear that the problems he had in mind were the sorts of problems that economists typically considered, and analyzed with the familiar tools of mathematical optimization. Satisficing was a theory of bounded rationality, put forward as a contrast with full optimization. As Simon subsequently explained, it was a theory about searching a haystack for "a needle sharp enough to sew with (satisficing)" as opposed to "the sharpest needle in the haystack (optimization)" (Simon 1987, p. 244). Considerations of cost and feasibility decree that search should stop before the true optimum is found.¹² In the appendix to the 1955 article, Simon supported this view by establishing it as the conclusion of a meta-level optimization of the search process itself.¹³

As adapted to evolutionary thinking about organizations, satisficing is not so much about stopping search as about starting it. Also, it is not so much about finding a "solution" that can be definitively scored according to some criterion, but about finding a way of doing things that at least promises to be superior to an existing way that is perceived to be inadequate (results are below aspiration). This evolutionary twist on satisficing joins it to the concept of problemistic search – search motivated by the appearance of a problem, and conducted in a way that is in some sense local to the problem.¹⁴

¹² One can only marvel at the "pointedness" of Simon's little example: the legendary quasi-impossibility of finding a needle in a haystack, the manifest idiocy of continuing such a difficult search once "a needle sharp enough to sew with" is found.

¹³ In my view, the appendix muddled the message of a great paper in an unfortunate way. It did accurately reflect an important fact about Simon: he was a rationalist. In this sense, the appendix was a characteristic move, having a certain "boundedly, yet more rational than thou" aspect to it.

¹⁴ Similar "twists" have been made by others; see (Winter 2000) for discussion.

Thus adapted, the satisficing concept suggests that the power of economic evolution is enhanced by powerful mechanism that is notably lacking in biological evolution: a source of endogenous control on the mutation rate. When things are going well, satisficing favors behavioral stability. When they are going poorly, the satisficing trigger produces search for superior alternatives. The specific consequences of this asymmetric search propensity depend on how “well” and “poorly” are defined by the aspiration level adjustment mechanism, on the way the competitive context affects aspirations, on the nature of the space that is searched, and on the quality of the test that determines whether the status quo is rejected in favor of a newly identified alternative. In general, however, satisficing produces a powerful net force for “improvement” in an absolute sense – upward motion on the same scale on which aspiration level floats as a moving target. It does so even if the search itself is totally uninformed as to which alternatives deserve examination. To appreciate the potential economic significance of this idea, think of that scale as labeled “productivity.”

I pursued this line of thinking in my 1971 paper, “Satisficing, Selection and the Innovating Remnant” (Winter 1971) The paper built explicitly on the portrayal of business behavior in the Cyert and March volume, invoked satisficing in the manner just described, and proposed innovative entry and the stick of competition as the mechanisms that tended to drive firms below their aspiration levels whenever their achievements could somehow be improved upon. These elements were built into a mathematical model that was structured as a Markov process in a set of “industry states” – a scheme that has many advantages and that was employed in much subsequent work.

Unfortunately, I made a serious strategic blunder: the featured result of the paper was a new proof of the Friedman conjecture. I somehow imagined that this result, with its required

stringent assumptions out in the open and (to my mind) virtually begging to be rejected, might provide a bridge that cautious researchers could use to cross from mainstream economics to an evolutionary vantage point. I tried to facilitate this in a later section of the paper, by using the same basic apparatus to sketch a model of continuing progressive change, in a Schumpeterian spirit. The ploy didn't work; most economist readers seem instead to have taken comfort in the fact that the Friedman conjecture could actually be proved. My hopes were naïve, and they were not realized. I should have known better. Certainly, I was by that time well aware that assessing the Friedman conjecture was not where the real promise of evolutionary thinking lay. There were more important questions to address.

4. Technology and Economic Growth

At the end of 1959, I joined the staff of the RAND Corporation. This was a good move from several points of view, but particularly because it made me Dick Nelson's colleague. At that stage, I had done considerable work on my new, theoretical dissertation focused on the Friedman conjecture. It was far from complete, however. At RAND, I had the benefit of Dick's remarkable intellectual enthusiasm and high-quality feedback (for which, after a few decades of this sort of thing, a multitude of scholars are in his debt). Over the ensuing nine years, we shared a total of about four years at RAND in two different episodes, and were also together for a brief period in Washington, on the staff of the Council of Economic Advisers. Only in a rather modest fraction of that total time were we doing things that turned out to contribute significantly to the development of evolutionary economics. What we did do counted for a lot. By the end of that period, and largely due to Dick's influence, the questions at the top of the agenda for

evolutionary economics had to do with the sources of economic progress, at the level of firms, industries, and national economies.

When I arrived at RAND, Dick had been involved for some time with the research program on R&D management and technological change, centered in the economics department and headed by Burton Klein. He had published a classic paper on “The Simple Economics of Basic Scientific Research” (Nelson 1959), and a valuable survey article on the economics of invention (Nelson 1959), and had conducted a penetrating case study of the invention of the transistor (ultimately published as (Nelson 1962)). These interests in technology were joined to an existing and more basic interest in the causes of long-term economic growth. His dissertation (published as (Nelson 1956) dealt theoretically with how overpopulation can bar the appearance of cumulative economic growth (in the sense of rising real incomes per capita).

In Dick’s view (then and now), technological advance has been the key driving force of economic growth. The advance of technology, however, involves interaction with other mechanisms and domains – among them the advance of scientific knowledge, capital accumulation, processes of market competition, and the development of institutions for education and research. For the most part, these processes have played out over the past few centuries within the broad historical frame of “capitalism” – by which is meant, not the pure market economy of the economics textbooks, but the much more complex and diverse institutional phenomenon seen in modern history, complete with kings, presidents, congressional committees, military establishments, wars, academies, bureaucracies, pressure groups, pension systems, government-funded think tanks and so on.

This is not a particularly radical perspective. The number of people who would accept it, at least as a plausible first approximation, is undoubtedly much larger than the number who have

let the research priorities of an entire career be governed by a determination to illuminate these issues. Dick Nelson is in the latter camp; he has followed such a path. To do that requires a great dedication to the proposition that economic growth is centrally important to the human enterprise, the insight and flexibility to keep locating each season's main chance for improved understanding, and the determination to pursue that main chance wherever it leads – even if it leads across disciplinary boundaries that others are disposed to regard as sacrosanct.

Decisions in unfamiliar contexts. Under Burt Klein's leadership, Dick and others in the RAND group probed deeply into the decision making that went on in the course of R&D activities (or "invention").¹⁵ Such inquiry reveals quite a different face of the problems of rational choice than is presented in the familiar arenas of the "profit maximization" discussion, such as the pricing of goods for retail sale. In the R&D context, there is a very real possibility that currently unimagined alternatives will appear down the road. In fact, this possibility is recognized and hoped for (impasse will yield to "Aha!"), deliberately sought (e.g., "brainstorming") and sometimes feared ("all this work will be for nothing"). Knightian uncertainty prevails; objective probabilities are not known. While rational choice theories direct the individual actor toward the use of subjective probabilities in such circumstances, there are actually many relevant actors, and the subjective probabilities are often a highly contentious subject for them. "Contentious" tends to mean that opinions correlate mysteriously with perceived interests and also with background experience, and that "political" influence processes of some kind will be a factor in the resolution. All alternatives, whether foreseen in general terms or not, get fully spelled out only in the course of a lengthy sequential design process. Uncertainty about how the next attempted stage will play out therefore tends to forestall effective

¹⁵ While most of the work of the others had to do with military R&D, Dick addressed broader issues in the economics of technological change, as suggested by the list of publications above.

planning and preparation for later stages – and also makes the current evaluation of future promise more problematic. Only by going forward is it possible to learn what the options are for going further forward. Throughout this design process, there is a dialectical dance between “feasibility” and “desirability,” such that proximate objectives co-evolve with the technical achievements.

With respect to these features, R&D management presents particularly vivid examples of the general problems of making decisions in a highly unfamiliar context. A lot of the key things that happen in the course of an R&D project are happening for the first time ever. The *ex ante* uncertainty about such things does not relate just to *whether they will happen*, it relates to *what they are* – because they haven’t been seen before.

The notion that actors can optimize their behavior is in a different kind of trouble in such unfamiliar situations than it is in familiar ones, and it is worse trouble. This trouble is not a matter of motivation, or of calculating ability, or of training in decision analysis. It is about whether a set of decision alternatives can reasonably be said to exist at all. After all, the essence of optimization is a thorough surveying of a set of alternatives, accompanied by consistent application of decision criteria. In the probing of an unfamiliar context, the typical situation is that the only alternatives actually available for surveying are a collection of first steps in various divergent directions. The further steps are largely hidden, and so are the reachable end states, or outcomes, and the steps in between. Whether a situation of this type can be satisfactorily represented by some formal theory of rational choice is not really the point, though I personally am skeptical. The real point is that it is very difficult to imagine that such a theory could be given any empirical traction, for either descriptive or prescriptive purposes. This is because so

few of the facts that matter are available *ex ante* to guide decisions; they emerge as the *product* of decisions.

I was not quick to absorb the research implications of this. Perhaps because I still hadn't fully shed that portion of my training as an economist, I was still disposed to rely on a scale anchored by a notional "right answer" when thinking about decision making and its possible shortcomings. This is often helpful, but it is sometimes a digression, or even a form of procrastination. (One of the points the RAND group made about the conduct of military R&D was that there was a tendency for planning to drive out doing, for discussions of feasibility to pre-empt the testing of feasibility. (Klein 1962)) The less familiar the context, the more rapid and fundamental the change that is going on, the less helpful it is to get hung up in the quest for the right answer. Through exposure to the work of the RAND group, and particularly through interactions with Dick, I gradually came to understand and accept this viewpoint. I also came to understand that behind the leadership of Burt Klein one could discern the shadow of another leader, now departed from the scene. That was Joseph Schumpeter, who had said rather similar things many years before.

Carrying out a new plan and acting according to a customary one are things as different as making a road and walking along it. (Schumpeter 1934)(p. 85).

Also,

"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 had 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 ... is to hide an essential thing”

(Schumpeter 1934)(p. 80, emphasis added)

Klein had been a student of Schumpeter’s at Harvard.

Innovative competition. Schumpeter’s fame derives from his emphasis on innovation as the driving force of capitalist development. More broadly, he stands out among the great economic thinkers because his theoretical approach to capitalism was fundamentally historical – it is a *theory of economic change*, as experienced historically. At RAND, Dick Nelson and I became increasingly aware that we were following Schumpeter’s path, and increasingly appreciative of how valuable the master’s guidance actually was. He seemed to have a lot of the big things right, though he fortunately left a lot for others to do, and at least a few things for others to straighten out. As the outlines of our joint research program began to emerge at the end of the 1960s, the term “neo-Schumpeterian” came to be one of the ways we described it.

Although most of Schumpeter’s ideas had been largely forgotten in mainstream economics, there was one area where his ideas – or at least an idea named for him – continued to guide research. The question was, what structural conditions are favorable to strong innovative performance in an industry? A possible answer to that was dubbed the “Schumpeterian hypothesis”. In its simplest form, the answer offered was, “oligopoly, relatively tight oligopoly.” That is, innovative performance is enhanced when a small number of large firms are vigorously competing with each other in the domain of new process and (particularly) product development, even while likely sustaining a mutually supportive relationship in the domain of price competition. Schumpeter’s name was attached to this hypothesis because it was claimed to be

the main message of a few pages in his *Capitalism, Socialism and Democracy* (Schumpeter 1950). There are some eloquent words in that passage, including the following very pointed ones:

“The introduction of new methods of production and new commodities is hardly conceivable with perfect – and perfectly prompt – competition from the start.”

(Schumpeter 1950) p. 105

He meant, presumably, the costs of the innovation can never be recovered under those perfectly competitive conditions. That by itself is an important, and valid, comment about the theoretical “ideal” of perfect competition. The passage as whole is, however, quite complex, evoking a number of different considerations. Whether it is reasonable to say that it all adds up to “the Schumpeterian hypothesis,” as known in the literature, is far from clear.

In any case, the industrial organization literature that explored the hypothesis empirically was not very well disciplined, either with respect to theoretical grounding or (in many cases) econometric technique. Nelson and I set out to do something about the theoretical grounding, based on conversations between us going back to the RAND days. We produced three papers, which ultimately were the basis of the section of our book titled “Schumpeterian Competition.” Our approach combined many of the elements identified above. In retrospect, when one considers all the “heresy” that was explicit or implicit in our approach, it is remarkable and also reassuring that we did manage to place the key paper in the *American Economic Review* (Nelson and Winter 1982)

We found a number of interesting things. Perhaps most interesting, we discovered quite unexpectedly the phenomenon known informally as “the monster

imitator". While Schumpeter's remarks about informational scale economies favoring innovation were on target, he failed to point out that the same economies favor the large, technically competent imitator. Such a firm avoids the cost burdens of innovation and simply gobbles up what innovators elsewhere have made available. That behavior shifts the innovation incentives adversely for other firms, and can be very destructive to the industry's innovation performance.

This result suggested the importance of taking a second, long look at any dominant firm that touts itself as a fountain of innovation. In the time of the paper, the plausible target for that long look was IBM. Today, it would probably be Microsoft. The question is, are these firms really the fountains of innovation they claim to be? Or is it their actual virtue that they quickly make the innovations of others widely available, while at the same time depressing the incentives for further innovation?

5. From Skills to Routines and Capabilities

The limitations of production theory. An economist who takes a close look at production activity and its supporting technologies is likely to suffer a form of dissonance quite akin to the effects of a close look at business decision making. The fundamental constructs used to describe technological possibilities in mainstream theory, the production function or production set, do not seem to be much in evidence. To be sure, there are inputs and outputs, and "ways of doing things" that convert the former into the latter. Also, contrary to some heterodox arguments that have at times been advanced in economics, there is often a lot of flexibility in these ways of doing things, and this flexibility is sometimes used to respond to changing prices. This degree of correspondence to reality is not,

however, enough to support the validity of the familiar constructs – any more than “firms try to make money” is enough to support the theoretical reliance on true profit maximization. The commitments of mainstream production theory go well beyond the realistic points noted; they envisage a set of alternative methods, all equally available, that can be comprehensively surveyed for the best ones, and that stays constant long enough so that multiple choices from the same set can be scrutinized for their mutual consistency. If there is real predictive power in this theory, it derives crucially from this constellation of assumptions. Needless to say, the close parallels between these remarks and the corresponding ones about maximization are no accident. The elements are intertwined in the mainstream theory of the firm, and evolutionary economists argue that they are even more intertwined in reality.

There is to my knowledge no paper about the “as if” theory of production that parallels Friedman on profit maximization,¹⁶ but there certainly could be. A world without production functions can mimic a world with true production functions at its causal foundations. One of the first major accomplishments of the Nelson-Winter collaboration was to illustrate this point with respect to the analysis of aggregate data on U.S. economic growth.(Nelson and Winter 1973; Nelson and Winter 1974; Nelson, Winter and Schuette, 1976). This type of analysis can be extended in several directions; we sketched the mathematical framework of one such extension in our book (Nelson and Winter 1982, pp. 175-184). While it may well be true that some econometricians have managed to estimate true short-run production functions, we would argue that most of the

¹⁶ There is, however, an important article by McFadden, containing theorems that can usefully be read as explicating one form of “as if” production theory (McFadden 1969)

econometric work on production (especially long run production functions) is more likely capturing the effects of mechanisms that merely mimic the real thing.

What is it that distinguishes the “as if” mimics from the real thing? In general, we proposed that statistical analysis based on data from a large number of firms, or aggregate data, tends to misinterpret the variation in the data in a way that considerably overstates the flexibility of production at the firm level. In our models, a key feature is that firms always have a “status quo” technique (or set of routines). While they can change that technique, the effective opportunities for doing so are much less rich than the amount of cross-sectional variation would suggest. We believe that this is true in reality as well as in our models. The inflexibility reflects the fact that firms are committed to their ways of doing things in ways that are hard to capture fully in standard economic data, and for reasons that are not reflected in the standard economic analysis of choice of technique. In evolutionary theory, those reasons are a prominent part of the story, as I now discuss.

Probing productive knowledge. Our conversations in the mid-1960s were much concerned with trying to understand technology and production, the role of knowledge in these, and the ways in which all of this might be represented effectively for theoretical purposes. The “theoretical purposes” we had in mind did not, however, have to do with improving the static theory of production, or its econometric implementation. Rather, they had to do with the treatment of technological change, with how technology relates to scientific understanding and other forms of knowledge, and what considerations limit the extent to which methods can travel from firm to firm and nation to nation (See (Nelson 1968)) At the time, the economics discipline was fascinated by neoclassical growth theory, a body of thought in which the production function apparatus was sacrosanct.

Technical change was central to the subject but was introduced in an abstract, analytically convenient way that kept the production function center stage, but thereby produced a picture that was very hard to connect to any specific technology or change process that one might study. We shared an interest both in getting a clear view of the limitations of the neoclassical approach and in trying to find a better path.

Without ever really defining this as a project in its own right, we had a number of conversations that involved trying to re-think the whole problem at a very micro level. This effort drew particularly on Dick's broad understanding of technology, but it also involved a lot of discussion of a class of examples that were more micro and much more accessible than the ones encountered in prior research: cake recipes. This somewhat whimsical line of inquiry rested on the serious premise that, at the level of abstraction we were trying for, one account of a way of doing something is pretty much like another.¹⁷ We might as well think about one that was easy to understand. More accurately, it *seemed* to be easy to understand. It turned out that there is a great deal to understand, and some part of that total problem remains at the top of my research agenda today.

There was one thing that came to light very quickly, however: there is an important contrast between a cake recipe and a cake production function. The former is actually useful in baking a cake, while the latter is not – though it would be useful for the preparatory shopping trip, where you acquire the needed inputs. This can be turned into a puzzle about economic theory. How is it possible to get along with a characterization of the knowledge used in production that leaves out the crucial procedural knowledge

¹⁷ The influence of this “cake paradigm” was seen in Nelson’s published work at an early stage. See (Nelson, Peck et al. 1967), pp. 99-100

from the recipe and deals only in the list of ingredients?¹⁸ This, on reflection, is quite possible provided (1) only input and outflows are considered interesting, not methods, (2) the set of possible input-output flows is given and constant. Under those assumptions, the “interesting” behavior of a cost-minimizing baker is predictable, given the appropriate price list.

Assumption (1) provides a disputable answer to another long-standing question about the scope of economics. But even if (1) is considered acceptable, taken by itself, assumption (2) imposes a crucial limitation on standard economic analysis. It cannot cope with change, because the baker’s capacity to deal with a new recipe cannot be determined simply by examining the new list of ingredients. Rather, it is necessary to know something about the baker’s command of productive *procedures*. (Note the connection to the statistical discussion above: ignoring the knowledge-of-procedures constraint biases analysis in the direction of overstating flexibility.) There is a moral to this little story that seemed compelling to us then and seems so now: if we’re ever going to get serious about understanding technological change as a phenomenon of advancing knowledge, the production function has to go.¹⁹

The second major point that emerges is, the account of procedures given by a recipe relies on words. Words are not procedures. It is not the word “stir” that stirs the batter, nor the words “until smooth” that provides an effective smoothness test. If, therefore, you want to understand how these productive events actually happen, you need to look behind the words and ask how effective connections between the words and the

¹⁸ There is side-exercise involved in expanding the list of ingredients of a typical recipe into an input list of the kind used in economic theory, but this is a minor technical point.

¹⁹ It has to be discarded as a *fundamental commitment*, as the theory’s single accepted way of characterizing technological possibilities, though of course it remains welcome as part of the tool kit.

procedures got created. At that point, you are suddenly and painfully deprived of the cheerful illusion that you were finally closing in on the knowledge aspect of this familiar productive activity. It turns out that you need, at a minimum, to know how language works, and how psychomotor skill works. These requirements present an overwhelmingly large and discouraging agenda. One might reasonably have concluded that the “cake paradigm” exercise, while stimulating, had reached a dead end.

Tacit knowledge. Somehow, the thinking of the chemist-philosopher Michael Polanyi came along to rescue us (Polanyi 1964). I cannot entirely recall how that happened, and some of what I do recall ranges too far afield to include here. Let it just be said that we somewhat fortuitously stumbled into Polanyi’s footsteps, just as we had previously stumbled into Schumpeter’s. Perhaps surprisingly, those two paths are not that far apart.

Reading Polanyi made it clear that the difficulties we had encountered were on the one hand common ones, and on the other very deep philosophically. If you try to plumb the depths of human knowledge, and the sources of human commitment to beliefs, you will eventually run out of rope. Polanyi accepts this conclusion and takes it in a constructive direction, showing by example that understanding can move forward nevertheless. In Polanyi’s famous phrase, “we know more than we can tell” (Polanyi 1966, p. 4). The term “tacit knowledge” labels this circumstance. Sometimes the knower’s incapacities can be at least partly remedied by an external observer, as Polanyi’s discussion illustrates. Often they cannot, if only because some of the parts that resist articulation involve mechanisms that are not well understood by contemporary science.

What was most directly relevant in Polanyi's work was his analysis of skill, a large portion of which we imported directly into our own. Polanyi observes that "aim of a skilled performance is achieved by the observance of a set of rules which are not known as such to the person following them" (Polanyi 1964) p. 49. So much for trying to spell out the procedures that are involved in baking a cake; the skilled baker will likely not be able to tell us. (Again, it is sometimes the case that external observers can helpfully fill in things of which the knower/producer is unaware, but the scope of that is limited.) The depth to which a way of doing things can effectively be explicated is limited by the encounter with human skill. When the probe reaches that level, the inquiry is in deep trouble -- for reasons that Polanyi explicates with great clarity.

Is "tacit knowledge" then, another name for "deep trouble?" Some critics argue that Polanyi labeled a problem but did not solve it. If the "problem" is to achieve ever-deeper understanding of a particular procedure, this observation is quite correct. However, Polanyi's discussion is radically more helpful than merely putting up a sign with "Road Closed" at the bottom of the canyon. His sign actually reads "Road Closed: DETOUR." In particular, one part of the detour goes toward the question of how tacit knowledge is created, and how it can be *transferred* or *reproduced* without being *articulated*. These things surely happen every day, and on a massive scale. Fortunately, what blocks the depth of our understanding does not block production, and that fact itself becomes the new target of understanding.

Routines and Capabilities. Reflect on some highly skilled performer that you have read about or perhaps personally observed -- be it a gymnast, a pianist, a medical diagnostician, a scientist or a CEO. Did you say to yourself, "What a beautiful example

of the universal human capacity for mutually consistent decisions”? Did you say, “Isn’t it remarkable what a boundedly rational individual can do by following a few simple rules”? Or did you perhaps say something like, “Awesome!”?

Skill provides a compelling model of effective behavior that is different, and deeply different, from what we are told either by theories of rational decision or by behavioral theories featuring “bounded rationality”. As far as I can see, the latter theories do not lead one to expect that the word “awesome” will ever be needed to describe human behavior. The former leads you to expect the awesome powers of explicit calculation displayed by a supercomputer – but that (putting aside the illuminating exception of calculating prodigies) is one particular kind of awesomeness that human behavior rarely displays. Thus, these two very impressive intellectual camps both seem to be missing something quite important about human behavior – that it can indeed be awesome, but is rarely so in the supercomputer style.

At least in the case of psychomotor skills, like those of the gymnast and the pianist, it is quite clear that coordination is of the essence, a major part of what makes the performance impressive. The overall “production” has a lot of visible segments, which may be impressive in themselves, but the totality is particularly impressive because the segments flow together with such flawless coherence. Perhaps coordination is of the essence in the other cases too, but it is hidden from us.

Organizations, too, can be awesome. For example, the safety record of U.S. scheduled airlines is awesome. The achievement of high yields in a process as sensitive as semiconductor production is awesome. In these examples, the awesome performance is actually the joint product of a large number of organizations and, of course, the

(skilled) individuals who comprise them. Again, it is clear that coordination is of the essence. To understand how knowledge shapes productive activity, you have to understand coordination above all. At the individual level, the kind of knowledge that underlies impressive coordinated performance goes by the name of skill, and it is the fruit of long practice, attended by much trial-and-error effort. What do you call that knowledge in organizations, and where does it come from? We chose to call it “organizational routines,” and to attribute it broadly to the same source.

It is certainly not possible here to probe the limits and nuances of the notion of organizational routine. The phenomenon poses deep puzzles, as skill does, and besides that the substantial literature of the subject has taken the concept in different directions, which are not necessarily mutually consistent. The treatment in our 1982 book was itself less than consistent, see (Cohen, et al. 1996; Hodgson 2003).

I feature here the interpretation that is central to our book: “organizational routines are multi-person skills.” To explain its origin, I point to the latent tension between the “Carnegie” discussion in Sec. 3 above and the subsequent discussion of technology. The problem is, it doesn’t seem plausible seem that those Carnegie-type organizations could do the impressive things in the technological realm that organizations actually do accomplish – and accomplish through attendant impressive things in the realm of organization. This tension, which was a major concern from the start, we subsequently dubbed “the competence puzzle” (Nelson and Winter 2002). How can organizations, with their numerous well-known flaws, display such extraordinary competence? To solve this puzzle, we drew on the skill model, with attendant insights from Polanyi, to modify the picture that the Carnegie school had bequeathed to us. The

resulting picture is a good deal richer, and it admits the “awesome” aspects of organizational performance alongside the “simple rules” aspect, not to mention the “how could they be so stupid” aspect. These diverse aspects are effectively blended in the concept of routines as multi-person skills, as is beautifully explicated and, in a sense, proved in a key paper by Cohen and Bacdayan (Cohen and Bacdayan 1994).

There are low levels of skill as well as high ones, occasionally giving rise to laughably poor performances. The same is true for routines. Some organizational routines are more like bad habits than skills – but bad habits are a familiar type of imperfection in individual skills as well. For both skills and routines, there are subtle hazards of inflexibility associated with being too good at the wrong thing, being caught in a “competency trap.” In both individuals and organizations, practiced skills and routines have to be complemented by deliberate but unpracticed adjustments. Individuals often do not improvise well, and there are good reasons to think that organizations might be distinctively worse in this respect. Organizations are in this sense more heavily dependent on their routines than individuals are on their skills.

Some have argued that the word “routine” has too many negative connotations, and that if we had wanted to “sell” our skill-like concept we should have chosen some other term for it. One answer to that is that we would thereby give up the nice catchphrase “routines as genes,” which concisely summarize the point that routines are, in our theory, a key source of the continuity in behavior that is required if “ways of doing things” are to be shaped by a truly evolutionary process. A more responsive answer is to point to the term *capabilities*. The original title of the Nelson and Winter book, carried by the draft we circulated for comment, was “An Evolutionary Theory of Economic

Capabilities and Behavior.” Prodded by the publisher, we agreed to a shorter title for the book. But “Organizational Capabilities and Behavior” did remain as the title of Chapter 5, the “routines” chapter.²⁰

Cake-making *capabilities* are what a bakery needs to make cakes. They combine knowledge, particularly in the form of individual skills and organizational routines, with the sorts of inputs recognized in the economic theory of production. Since many of those “inputs” participate in the storage and reproduction of the required knowledge, they are not really the same entities featured in standard economic theory. The knowledge we can identify in skills and routines cannot, however, be the whole story about the knowledge that makes cake production happen – because, as Polanyi explained, the “whole story” is forever beyond our reach.

Sources of routines and technologies. The discussion of routines in the 1982 book said quite a lot about what they were like and why they were important, but very little about where they came from. This was not because the latter question was considered uninteresting. It was because we thought it would be very difficult to address it well, and we did not want to hold up the completion of the book while we made the attempt.

A good deal can now be said about this subject. On the theoretical front, much insight has been derived from work that joins the familiar idea that organizations engage in “local search” to a particular characterization of the space that is searched – a characterization provided by the “NK modeling” technique. As shown by Levinthal

²⁰ We had used the term “capabilities” in the title of the first paper of our evolutionary collaboration (Nelson and Winter 1973). As far as I recall, we were unaware at that time of the use of the term in the fine paper of (Richardson 1972). Since the term was used in a similar sense in military circles, it may be that our RAND experience had something to do with it.

(Levinthal 1997), this combination readily produces a picture that displays key elements of the picture needed in this foundational part of the theory. Here is just a part of that picture. In a population of new organizations occupying a common environment, you will see the development of systematic ways of doing things. Different organizations will generally develop *different* ways of doing things, because of the path dependence that arises from local search that has diverse starting points, and has random elements along the way. The amount of diversity that survives depends on the amount of interaction among policy dimensions, which determines the complexity of the overall problem and thereby determines the “ruggedness” of the landscape searched – the number of local peaks. Significant differences will persist, not merely in ways of doing things, but in performance (fitness).

Although this is all demonstrated at the level of abstract theoretical parable, it is evidently a very powerful parable. It conveys a vision of the key mechanisms that far transcends the particular mathematical form in which they are represented, and thereby suggests that there is probably a wide range of alternative approaches to the same substantive ends – now that we have the idea. For extensions and applications of this approach to management problems, see for example (Gavetti and Levinthal 2000; Rivkin 2000; Rivkin 2001; Rivkin and Siggelkow 2003)

There was more in the 1982 book about where technologies come from. Here the major conceptual point is that it is far from correct to think of new methods as arising from sources that are beyond the reach of economic incentives or are intrinsically very difficult to predict (e.g., creativity, basic science). While these characteristics are certainly present some of the time, it is reasonable to argue that these cases are the

exception to the rule. The rule is that the new emerges from the old, and it does so in ways that are strongly patterned by economic incentives, by intrinsic features of the technologies themselves, and by the specific investments of business firms that are trying to make it happen.

In the book, we discussed a number of these patterning aspects. Following a lead provided by (Rosenberg 1969), we emphasized particularly the phenomenon of “natural trajectories” – a sustained path of improvement in a technology that is generated by the repeated invocation of the same problem-solving approaches or “technological paradigm.” The miniaturization trajectory in semiconductor devices is a particularly compelling and important example, see (Dosi 1982). The idea of a trajectory is closely related to the idea of “dynamic capabilities” in the strategic management literature (Teece, Pisano and Shuen 1997). For example, Intel’s dynamic capabilities are the package of routines and resources that have allowed it, in particular, to pursue the miniaturization trajectory effectively. Among many other works that pursue various aspects of the phenomenon – the patterned way in which new technology emerges – see (Malerba 1985; Levinthal 1998; Murmann 2003) And for related discussion that places more emphasis on routines and organization than technology, see (Winter and Szulanski 2001; Zollo and Winter 2002)

6. A Sampling of Empirics

The range of empirical topics touched upon in the discussion to this point is so wide that it is clearly impossible to do more than mention a few significant examples. Under many

important headings, we would acknowledge that there is a great dearth of empirical work that is directly relevant to the evolutionary economics program that Dick Nelson and I put forward. In large part this is because economists, especially American economists, have not been much interested in the propositions of evolutionary economics, whether for the purpose of developing theory, testing hypotheses or attempting refutation. Much of this missing work is of a kind that *should* be done by economists, either for reasons of characteristic skills or typical interests. Elsewhere – in both a geographical and a disciplinary sense – the situation tends to be much better.²¹ As is discussed below, there are even areas where fortune has truly smiled, giving us supportive, interested colleagues in relevant areas where we did not provide much leadership.

There is one fundamentally important area where we would argue that empirical dearth vs. plenty may lie in the eye of the beholder – and we see plenty. This is the question of the general character of firm decision processes. Because of the peculiar cast given to the whole discussion by the influence of Friedman’s methodological stance, it is unclear who is supposed to be persuading whom about what. Are we called upon to mount a case that our account of firm behavior is considerably more realistic overall than the optimization model favored in mainstream economics? Such a case does not call for new empirical research, it calls for a gigantic survey article. That evidence has long been

²¹ There is a formidable list of scholars who deserve mention and citation for their efforts to further the evolutionary program, or at least their willingness to take it seriously. They must be thanked collectively, for to actually give that recognition in this already-long paper, with its already-voluminous references, is quite impractical. Many of these people are found at the bi-annual meetings of the International J.A. Schumpeter society. For specific indications of the reception of the 1982 book see (Freeman and Pavitt 2002; Dosi 2002; Dosi, 2003), and the papers in those journal issues. See also the introductory essay in Giovanni Dosi’s collected papers (Dosi 2000)

abundant, and more comes in every day²² – but of course it doesn't come from mainstream economics.

The fact is, nobody has ever taken the stance that evolutionary economics is inferior to the mainstream brand in terms of its general conformity to the result of direct observation of business behavior. We take it, therefore, that virtually everyone agrees that we win that contest – which is hardly a surprise, since the need to win it was a key premise of our undertaking and a matter of indifference on the other side. If dramatic new facts about business behavior were to become firmly established, showing evident discord with our prevailing generalizations, would we fold the evolutionary tent and steal away? No, we would make the necessary adjustments and carry on. The premise is that it is the task of economics is to *accommodate* the realities of behavior and determine the implications – not to commit to an econo-centric view of behavior that has to shelter itself from the facts that the rest of the scientific and practical world turns up.

The real question is whether economics can effectively be done in the style that we favor. It is in the worthy cause of doing economics effectively that the Friedman-influenced mainstream asserts its scientific right to hide from inconvenient facts (or, from their viewpoint, “irrelevant details”). We, on the contrary, argue that economics can be done effectively without such hiding, and have tried to illustrate the point. Of course, the discipline's territory is so vast that we can barely provide scattered hints relative to the total, and such hints need not be persuasive on the large point even if considered meritorious on small ones.

²² For some recent, quite striking evidence see (Starbuck and Meziar 2003) (and the papers, including mine, commenting thereon).

Somewhat narrower aspects of the evolutionary proposal present some of the same framing problems posed by the broad characterization of business decision making. Overall, there is abundant evidence, but only a small portion of it was produced with the evolutionary economics agenda directly in view. Here again it is often hard to be sure regarding who needs to be persuaded of what. For example, are organizational routines and capabilities features of reality; do they tend to be firm-specific in significant ways, and do they actually persist for extended periods? Many people do not need persuading, but anybody who does could consult, e.g. (Usselman 1993; Helfat 1994a; Helfat 1994b; Klepper and Simons 2000), plus the many empirical studies in, or cited in, (Dosi, Nelson and Winter 2000) and (Helfat 2003) What about the proposition that durable firm attributes have a powerful shaping effect on the course of competition? If you doubt that, you should examine the “entrants vs. incumbents” literature, including for example (Tushman and Anderson 1986; Henderson and Clark 1990; Tripsas 1997; Tripsas and Gavetti 2000). What about the suggestions that organizational knowledge is not the transparent, transferable, readily exploited asset that standard production theory pretends it to be? See (Kogut and Zander 1992; Szulanski 1996).

There is one important subject that Nelson and I did not address explicitly in our book, and which is obviously of central importance in the evolutionary scheme. This is the subject of industry evolution – the evolutionary patterns that characterize the development of an industry or product market over time. The industry evolution perspective teaches lessons of great importance for economic policy, particular the point that industry structure has to be understood in historical context. I belatedly turned to the theory of this subject in my 1984 paper (Winter 1984). Already at that point, my article

shows, the influence of Steven Klepper's empiricism was being felt – the paper that ultimately appeared as (Klepper and Graddy 1990) was available to me in working paper form. Klepper's subsequent work, including among many others (Klepper 1996; Klepper 1997; Klepper and Simons 2000; Klepper and Sleeper 2000) has documented the pervasive patterns and greatly advanced theoretical understanding of them. Although Klepper's way of characterizing firm behavior in the short run leans in the orthodox direction, his dynamics are more in the evolutionary spirit. In any case, the empirical evidence he presents is both valuable and generally supportive. A great many scholars have done related empirical work. Meanwhile, the theory of industry evolution has been further addressed in an empirically grounded way with the methods of "history friendly" simulation, see (Malerba, et al. 1999; Malerba, et al. 2001)

I conclude this sampler by addressing the linked topics of firm size and firm growth. These two present, respectively, an evident success of evolutionary theory and a currently significant refutation hazard.

A striking fact about the category "business firms" is the extraordinary magnitude of the size discrepancies among the examples in that category. A conservative statement about the magnitude of that discrepancy, top to down, would be a factor of about 100,000. At the high end, there are firms near the top of the Fortune 500, at annual sales above 10^{11} dollars. At the low end, the question arises as to what attributes are required for membership in the "firm" category – for example, does a business conducted by a single individual, part-time, from home qualify? To be very conservative about that point, I put the lower end at annual sales of 10^6 dollars; one obviously could argue for a

much lower figure. This factor of 100,000 is a large number, an in-your-face feature of economic reality that might seem to call for explanation. What is there to say?²³

Evolutionary theory says that it is the product of long-extended processes of cumulative growth (and consolidation). It says that most firms start quite small and grow large because of their success. It notes that particularly rapid firm growth often attends the birth of an industry, or of a new specialized niche within an industry, and that this is all part of the evolutionary struggle among ways of doing things. These patterns arise quite naturally in evolutionary models, as has now been demonstrated innumerable times. Just how much of the pattern is an “assumption” of the model and how much a derived “prediction” varies from model to model, but in any case assumptions *are* predictions (predictions with short derivations, as many of Friedman’s critics have pointed out). Along with the major phenomenon of large size discrepancies, these models typically predict many other patterns that are found in the turbulent competitive processes of reality. Presumably, a critic replying to this point might claim that the possibility of deriving realistic patterns from realistic processes is “obvious” and hence not a significant accomplishment of the theory. Should we conclude then that this part of reality is legitimately ignored? Should the academic discussions of generic “firms” go on indefinitely while almost always maintaining a discrete silence about the factor of 100,000? What do other theories have to say?

Evolutionary theory says, to understand firm size, look at firm growth. Here there appears to be some trouble for the theory. The highly skewed size distributions of business firm have long been explained by a variety of models of cumulative random

²³ Our emphasis on being guided by the realities of firm size and industrial structure is one of the key differences between the Nelson-Winter brand of evolutionism and that of the organizational ecologists. See (Winter 1990b) for further discussion.

growth. That by itself is not trouble; what evolutionary theory offers is an improvement on these models that identifies significant exogenous features of the context in which growth takes place (Nelson and Winter 1978) There clearly is trouble, however, if the specification of the stochastic process involves too austere a version of “randomness” – a version that would seem to rule out the systematic long-term consequences of firm attributes that are featured in evolutionary theory.

That this is in fact the case has been argued forcefully by Paul Geroski (Geroski 2000) In particular, he claims, the strict “Gibrat specification” fits the growth data quite well. This is a statistical model in which random shocks to the log of firm size are distributed identically and independently across firms and time (annual data). Geroski does not target evolutionary economics specifically; he correctly points out that any theory or argument featuring persistent firm traits (e.g., capabilities) is in some trouble here. Indeed, the broad consensus about what business firms mean in the economic system might be considered to be in trouble.

The sorting out of these theoretical and econometric issues has barely begun (but see, for example, (Bottazzi, et al. 2001).) There are lines of possible reply to Geroski that would dispose of the apparent threat to fundamental commitments of evolutionary theory – but whether those replies are empirically credible remains to be seen. So, for the moment, it is possible only to conclude here with two items of good news: (1) there is an exciting research agenda here; (2) there is no merit in the occasional claims that evolutionary theory is not seriously exposed to refutation.

7. Evolutionary Economics and Management

Economics needs to take large firms very seriously because of their major influence on the system as a whole. Taking large firms seriously means taking managers seriously, because managers make real choices under real uncertainty. In organizational economics, there are valiant efforts to take managers seriously within the familiar frame of rational choice modeling (Gibbons 2003). Such efforts, while capable of generating useful insight at the micro level, have limited power to address the evolution of the context, capturing the larger scale interactions in the system. For that purpose, the familiar story of profit maximizing firms and (even) competitive markets provides the backdrop for the analysis, as it does elsewhere in the discipline, for want of anything better (or so it is claimed).

In management, the need to take managers seriously does not require an argument, and is not limited to accounting for the influence of large firms. A possibly more serious question is, does management need to take economics seriously? While a lot of useful work under the broad rubric of management probably does *not* need to take economics seriously, there are areas where economic principles are fundamental to the problems addressed. Strategic management is the obvious case. Like mainstream economics, evolutionary theory illuminates the workings of competition in the marketplace, through which firms influence each others' profitability as well as their prospects for growth and survival. Unlike mainstream economics, its illumination of those "workings" falls directly on the dynamic processes of competition, and not just on equilibrium outcomes or tendencies. Also unlike mainstream economics, its image of a population of firms is an image of *heterogeneous* firms, differing in their ways of doing things and also in size – with the size differences produced endogenously as a consequence of those idiosyncrasies.

Indeed, thanks to the complementary theoretical work in organizational learning and the partial filling of the major gap concerning industry evolution, it should now be within reach to produce a comprehensive model of the creation and evolution of an industry – a sort of “Big Bang” model for an industrial universe. Such a model would map the entry processes, the learning processes, the market competition processes, the differential growth and survival, and the appearance of concentrated structure – all within a frame that represented and controlled the key exogenous forces and structural determinants, but none of the details. It could even extend to the significant problems relating to the determination of industry and firm boundaries, since evolutionary forces are at work there as well (Langlois 1991; Jacobides and Winter forthcoming). Such a model would rest on a layered structure of theoretical commitments about key processes -- commitments that have already been identified and debated, and of course can be debated further. Implemented as a simulation model, it would produce a realistic picture of an industry that responded in systematic ways to differences in the exogenous conditions. It might misrepresent reality, not merely because of the necessarily abstract character of theory, but because it failed to capture significant patterns in the reality. And if it did misrepresent reality in significant ways, that discrepancy would be ascertainable. In short, it would have content.

Most fundamentally from the viewpoint of management theory, evolutionary theory invites detailed attention to individual firms and the problems they face in dealing with competitive environments.²⁴ It does not merely accept, but *urges*, that inquiry extend to the inner workings of firms. It offers the investigator suggestions about what to look for – especially if the inquiry is one that includes a concern with how that firm fits and fares in the larger system. It also urges, however, that an open mind about the nature of decision processes found in firms will prove more useful than a closed one.

²⁴ In this connection, see (Gavetti and Levinthal forthcoming) for an encouraging assessment.

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Another important economist in developing evolutionary economics was Joseph Schumpeter. Schumpeter offered a model of creative destruction. This theory said that a capitalist economy was in a perennial state of change. When firms failed, this was important for freeing resources to be taken by entrepreneurs for more efficient and productive processes. Aspects of evolutionary economics. Like behavioural economics, economic agents are influenced by a complex set of factors " For example rather than just profit, a firm may be motivated by: A desire to survive. A desire to be free to innovate and c