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## WORKING PAPER SERIES

### **Pursuing the Evolutionary Agenda in Economics and Management Research**

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## **Pursuing the Evolutionary Agenda in**

## **Economics and Management Research**

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This essay first reviews what Nelson and Winter were trying to accomplish when they put forward *An Evolutionary Theory of Economic Change* (Belknap Press, Harvard, 1982). It then does a fast-forward to controversies and contributions in the recent past, and speculates on where the intellectual enterprise might be headed from here. The issues involved in the original motivations are definitely alive and well. Aside from the review of the basic issues behind the NW project, an important theme here is that an evolutionary approach to economics entails a degree of engagement with the realities of business organization and the quest for profit that has no parallel in mainstream economics. Thus it makes sense in retrospect that the evolutionary program has proved more influential in other research fields, including strategic management, technology studies and organization theory, than it is in economics proper. Recent controversies underscore the continuing existence of a challenging research agenda featuring the interactions among the dynamic processes at different levels – individuals, firms and market environments.



## **Pursuing the Evolutionary Agenda in Economics and Management Research**

In this essay, I seek to link the past with the present and future. We are now approaching the thirty-fifth anniversary of the publication of *An Evolutionary Theory of Economic Change*, by Richard Nelson and Sidney Winter (referred to henceforth as NW 1982). The world has obviously changed dramatically, in many respects, over that period. It has changed even more in the almost half-century span that I will sketchily address here, a period that starts at one of the candidate “origin” points that one might conveniently (and somewhat arbitrarily) identify for the Nelson-Winter enterprise as a whole. The world presents us, however, with endless, fascinating and oftentimes paradoxical mixtures of change and continuity. It has changed enormously in some respects, but in others ... as current slang would have it, “not so much.” Or, more traditionally, “Plus ça change ....”

This is certainly true of the perennial discussion of economics, its foundations and scientific status as a discipline, and its role and contribution to society. An important component of that discussion involves the relationship of economics to business behavior. Is the account of business behavior typically offered by economists substantially true? If not, does that matter? Has economics made useful contributions to business practice, and if so, what is the nature of these contributions? These questions were on the table before Nelson and Winter began their collaboration – and in fact, before Nelson and Winter were born. The same questions are very much on the table now; and their significance for the future, in both scientific and social terms, may well be at a historic high. There are fresh opportunities to advance the discussion.

There is the story about the alum, back at the *alma mater* for the 25<sup>th</sup> reunion, who goes to visit his old professor. He glimpses a copy of the recent final exam on the professor's desk and finds it familiar. He looks more closely, and then says "Why professor, these are the very same questions that were on your exam 25 years ago!" And the professor replies, "Of course, but the answers are different." In the case of the paradigm struggles in economics, the questions are the same all right, but the answers are pretty much the same too. It's just that they aren't unique. It is a matter of multiple graders and multiple answer sheets. The argument has been joined only to a modest extent, and it seems that concern with it was in secular decline at least until 2008. In some circles it is clearly disreputable to re-raise a well-settled question like "Do firms maximize profits?" But the leaves are now rustling again in the groves of academe, and perhaps they portend winds of change.

### **1. Preliminaries: Terms and Assumptions.**

Labeling the target. As its title declared, the 1982 book was concerned with economic change. Particularly in the domain of firm behavior, the book said a good deal about topics engaged by microeconomic theory. Since the position offered ran counter to the ruling paradigm, it was necessary to identify and name that target, and to present a critique of it.<sup>1</sup> We used, and defended, the term "orthodoxy"; we referred the reader to the leading textbooks in intermediate microeconomics for the prototypical manifestations of that phenomenon. The same reference still serves the purpose, and it can be amended by inserting "and advanced" after "intermediate". The term "neoclassical economics" is frequently used to mean approximately

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<sup>1</sup> Some complained that we spent too much space on the critique, even that we offered *nothing but* critique. The table of contents refutes the latter very emphatically.

the same thing, and the term “mainstream economics” can also serve. While these terms are not synonymous, they will be taken to be so here.

While little depends on the choice of label, it is important to identify the denotation more precisely. The reference is to a style of economic analysis involving rational choice modeling of individual actors and a focus on conditions that are in some sense “equilibrium” conditions. Very typically, the actors are not merely rational but self-interested in a narrow, fully egoistic sense. Further, and to a greater extent than in 1982, rational choice is now often understood to subsume rational expectations. That is, the optimizing actor’s view of the decision environment (including other actors) must be modeled as fundamentally correct at the structural level, though typically considered to be imperfect due to limitations deriving from incomplete information.

Finally, in the case of the theory of the firm, textbook orthodoxy has long been based on the assumption that firms are *unitary* rational actors. They behave as a rational individual with the same preferences would behave.<sup>2</sup> In the long tradition of verbal theorizing in economics, this commitment was very commonly reflected in usage which equated the decisions of the firm with those of “the entrepreneur”. Today this usage has faded, mostly as a result of mathematization of the theory, which has further suppressed the question of how “profit maximization” actually happens. Perhaps the rising interest in actual entrepreneurs has also tended to make people increasingly uncomfortable with referring to giant corporations as “entrepreneurs”.

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<sup>2</sup> Those preferences are not the preferences of the rational individual encountered in consumer theory, and in particular do not involve any taste for leisure, or for that matter, sleep.

There is a substantial amount of research activity in the economics discipline today that is accepted and accorded high status but is not “mainstream” in the above sense. This applies in particular to experimental economics, behavioral economics and neuro-economics (which are partially overlapping categories). The concerns of these “tolerated heresies” are overwhelmingly at the individual level. At the organizational level, there are various deviant branches, transaction cost economics (TCE) (Williamson 1985) and organizational economics (Gibbons and Roberts 2012), when done in an essentially game-theoretic style. The former is perhaps weakly tolerated. Though many in the mainstream were scandalized by Williamson’s Nobel Prize, generous acknowledgments from some leading scholars of the organizational economics camp have lent credibility to TCE. That organizational economics camp itself is clearly mainstream by the above test, but nevertheless carries an exotic whiff of heresy, due to its departure from the *unitary* rational actor tradition. As for the tradition based in the Carnegie School version of “behavioral” economics, it was and is profoundly heretical, and not really tolerated. That is indicative of a larger phenomenon, which is that mainstream theoretical economics maintains a cautious distance from the organizational and institutional facts of life in modern economies.

Meanwhile, beyond the sphere of microeconomics proper, the discussion takes a somewhat different form. In mainstream macroeconomics, there is considerable consensus that the “right way” is a way consistent with rational choice modeling of the actors, but less consensus on what precisely that means.

Mainstream theorizing: MEOC at the margin? A vigorous critique of a dominant scientific paradigm (such as the critique I reiterate here) should not be read as entailing rejection

or deprecation of continuing contributions from that research tradition. Newtonian physics still contributes practical utility and insight, though demoted from its long-standing position as the dominant paradigm. The question is properly posed as one of priorities at the margin. The valid complaint is that there is *too much* neoclassical economics in the disciplinary research portfolio, not that none of it could possibly prove to be helpful to an empirical science. Specifically there is far too much *theoretical* neoclassical economics, and far too much reliance on orthodoxy as a simple litmus test for theoretical contributions. Because of that reliance, the development of alternative theories is stifled. Considering that a reasonable overall judgment on the empirical status of rational choice theory would declare it overwhelmingly refuted (at the individual actor level), the notion that it still deserves a privileged status as foundational to the discipline is bizarre – assuming that the discipline is an empirical science concerned with economic life, and not simply a comfortable home for builders of mathematical models that respect a certain constricted aesthetic.

That last aspect of the “mainstream” sometimes makes it seem like the Moral Equivalent of Chess (MEOC).<sup>3</sup> Specifically, it is an activity that parallels chess in these respects:

- Intellectually demanding
- Differentially rewarding of logical intelligence
- Internationally competitive

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<sup>3</sup> Here I am following the hazardous path of Jimmy Carter’s ill-fated speech on the “Moral Equivalent of War,” but at least the acronym isn’t MEOW. The phrase originally came from William James.

- Elites in the field make a reasonable living doing it
- Productive of entertainment and distraction for the participants, but largely irrelevant to the welfare of non-participants.

Realism (or truth) vs. instrumentalism. How could this happen? How could a set of ideas that are so obviously weak at the empirical level not only retain influence, but retain a privileged status as foundational in an aspiring scientific discipline? So privileged, in fact, that a substantial level of MEOC-type, infinite-horizon, often publically-funded investments in this kind of intellectual capital can go on without a serious plausibility check? Recall that Newtonian physics lost the fight to general relativity over a flawed prediction that was in error by about 40 arc seconds per century. Physicists did not shrug and say “that’s not so much.”<sup>4</sup>

In the economics discipline, by contrast, there is a strong methodological tradition that says that good theory does not have to be accurate (true) at a detailed level. In that, there is an implied tolerance for descriptive error, and it is pervasive in the discipline. What matters particularly here is that there is one large category of “detail” that the mainstream considers ignorable – it comprises anything relating to the internal processes of business firms. This is not just a matter of small quantitative prediction errors being exempted from an overly rigorous truth criterion. It is a rejection of a whole category of evidence on the basis of a highly contestable methodological claim.

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<sup>4</sup> The reference is to the rate of precession of the perihelion of Mercury, recognized as anomalous according to Newtonian calculations since 1859, and proposed by Einstein as a test in 1916.

To attempt to sort this out – to discuss seriously what “true” means in the context of scientific theory, what “realistic” means in such a context, and what merit there is in “instrumentalism” – is a project far beyond the scope of this essay. It is quite clear that the historical origin of the tradition lies in, or at the least was powerfully shaped by, Milton Friedman’s methodology essay (Friedman 1953). Today the tradition continues in cruder forms among practicing economists, while a much more sophisticated, but isolated, discussion goes on among scholars actually interested in methodology. Substantial defenses of the tradition (like, for example, (Boland 1979)) are rarely encountered today in the broader literature of the discipline, but it seems not to require defending. Whether Friedman’s sophisticated, carefully-crafted essay is actually read nowadays or is just deeply ingrained in the disciplinary culture is unclear, but I tend to think the latter. I would be happy to engage in a serious debate about this, but it is not clear where I could find an opponent. In brief, my position is as follows:

“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.” (Winter 2005)<sup>5</sup>

Virtues of the mainstream. Many economists of heterodox leanings are so fed up with the pretensions and evasions of orthodoxy that they cannot bring themselves to engage the question of what scientific virtues it has. “Time enough,” they say, “Time enough to worry about

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<sup>5</sup> I have addressed the methodological issues in too many places to enumerate here. See (Winter 2005) for most of those references.

that when orthodoxy is overthrown!” Yet, even setting aside the question of when the overthrow is likely to occur, the points made above suggest some reasons why a secret meeting of heterodox conspirators should be interested in engaging that question. (Perhaps we should come out of hiding and convene an open conference on it.) Without such engagement, empirical successes that actually have zero power to discriminate between the mainstream and significant alternatives (such as evolutionary economics) will inevitably continue to be scored as successes for orthodoxy. Such scoring is done according to the usual scientific rules, which are strongly verificationist in practice regardless of any announced principles to the contrary.

This might be called the “bills on the sidewalk problem.” Here, says the mainstream economist, “Let’s walk down the street together and you tell me when you see a hundred dollar bill on the sidewalk. If you see one, we’ll even let you pick it up. OK? Let’s go!”<sup>6</sup> This is legitimate, even admirable, scientific rhetoric; we heretics need to be prepared for such moves. (One can only wish that the mainstream would become similarly sensitive to “let’s just walk down the street” empirics on more significant questions.)

It is not a trivial matter to sort the trivial from the non-trivial. There are the cases like the bills on the sidewalk, where the claimed prediction should simply be endorsed and differently interpreted. There are the cases where we clearly should be eager to bet against the mainstream, and the problem is how to entice them into a reputational bet that we know to be perilous for

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<sup>6</sup> The “bills on the sidewalk problem” here stands in here for a number of examples of similar rhetorical value and similarly limited significance; there is in particular the “demand curves slope down” story. .

them.<sup>7</sup> There is the fund of cases where the mainstream has already lost its bet, but displays either denial or selective memory loss. And then there are the many challenging questions where solid evidence is in short supply and a challenge to the mainstream should be stated in nuanced terms. In that zone, there is the ever-present hazard of jumping to conclusions that feel comfortable on ideological or other non-scientific grounds, including superficial alignment with other positions on quite different questions -- questions that somebody has perhaps bundled together for their own political purposes.

My own concise account of the virtues of orthodoxy comes down to three points. First, orthodoxy strongly emphasizes that economic motivation of a self-interested kind is a powerful factor shaping the social world, and that is true. Second, it offers a tool kit of methods for exploring the implications of point one in a reasonably systematic way, and that is at least convenient. Third, it offers innumerable heuristic insights about the possible consequences of interactions among many self-interested individuals, occasionally suggesting surprising conclusions. And that is both valuable and distinctive.

## **2. A history of randomly converging streams.**

Partial historical accounts of the Nelson-Winter collaboration have been sketched in various places, e.g. (Winter 2005), (Nelson 2006). Here, I seek to avoid replicating those accounts in excessive detail, while possibly throwing some slightly different light on the

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<sup>7</sup> In his recent book *Misbehaving: The Making of Behavioral Economics*, Richard Thaler recapitulates a career marked by remarkable success in pinning down theoretical questions and then developing powerful evidence, often experimental, against the mainstream view on those questions.

“origins” question. In retrospect, the story seems to be one in which happenstance played a remarkably large role.

In 1967, Dick Nelson and I were colleagues for the third out of four times, thus far, in our careers. We were at The RAND Corporation, the Santa Monica, CA, a think tank funded substantially by the U.S. Air Force. Its Economics Department, at least, had been a bastion of research freedom, and to some extent it still was.<sup>8</sup> This was the “RAND II” phase in our careers; we had also been colleagues at RAND in 1959-61. Over both phases, there were active conversations about matters of mutual interest, and in particular about technology, knowledge, firm behavior, and the relationship of these to economic growth.

As 1967 began, there were three significant strands of thinking in hand that contributed ultimately to the 1982 book.

Evolution and firm behavior. There was the evolutionary/ firm behavior strand, as represented in particular by my Yale dissertation, officially finished in 1964 (Winter 1964), but begun five years earlier. The dissertation drew inspiration from the evolutionary thinking of (Alchian 1950; Friedman 1953). It focused particularly on the validity of Friedman’s argument that the validity of traditional, maximization-based arguments could be supported by “natural selection” arguments – referencing mechanisms that would be operative regardless of what detailed examination of firm behavior might suggest. By that time, there was already substantial indication that such detailed examination would not be supportive of the hypothesis of profit

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<sup>8</sup> See the recent account of RAND in Augier et al, I have some minor disagreements with those authors regarding when and why the intellectual “flaring” they describe came to end.

maximization.<sup>9</sup> No matter, Friedman suggested, evolution will make it happen. My assessment of this claim included exploration of simple formal models of firm behavior in a model industry undergoing economic evolution. The models indicated that the conclusion was sensitive to assumptions on a number of key issues not mentioned by Friedman.

Technical change and economic growth. As of 1967, the intellectual tide of neoclassical growth theory had been running strong for a decade. That current derived much of its power from the recognition that long-term growth in per capita incomes could not be adequately accounted for by capital accumulation alone, i.e. growth in physical capital services per man-hour.<sup>10</sup> This point held a prominent place in the early neoclassical models, but the main research focus thereafter was not on the explanation of technical change itself but on the existence and characteristics of steady-state growth paths under various circumstances. Subsequently, there was attention at least to the lurking issues about the role of incentives for innovation. Generally, leading proponents of the theory tended to ignore its puzzles and its implausible aspects.

There were, however, significant critics of the neoclassical program, many of them associated with Cambridge UK. They evinced greater curiosity about micro-level details and a stronger demand for face plausibility. They questioned, for example, the theoretical image of

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<sup>9</sup> Indeed, the background of the methodological discussion included the “marginalist controversy,” see, e.g.(Hall and Hitch 1939; Machlup 1946; Gordon 1948)

<sup>10</sup> Here, the official mainstream candidate for the *locus classicus* designation is (Solow 1957). As Nelson pointed out, however, statistical analysis supporting this conclusion dates back farther (Nelson 1998).

smooth factor substitution in a world where many production processes involve discrete, indivisible machines, and ultimately challenged the production function itself. (See, for example, (Robinson 1956; Kaldor and Mirrlees 1962)). Most famously, they inquired into the measurement of capital and challenged the basic idea of an aggregate capital stock. This led to the “Cambridge controversies,” an episode that arguably should have been the death knell of the neoclassical analysis of growth, but clearly wasn’t (Cohen and Harcourt 2003).

The fact that these sorts of critiques of neoclassicism were going on was a supportive influence on the particular critique that Nelson and I were developing in the same time period. Like, the Cambridge (UK) critics, we had serious reservations about the production function concept as the basic representation of productive knowledge (Winter 1982). In our view, the dominance of that concept was linked to the commitment to optimization in the analysis of firm behavior, because such a commitment creates an implicit demand for a clean and tractable characterization of the opportunities facing the actor. Without a challenge to the optimization commitment, it was essentially impossible to envisage a path by which fundamental understanding of economic growth could be enhanced by drawing on the wealth of process knowledge accumulated by scholars of economic and technological history -- not to speak of the insights of theoretical giants like Smith and Schumpeter, and of contemporary economists who worked closer to policy questions in fields like industrial organization.

The foregoing themes, the associated intellectual frustrations, and resulting extended reflections on the question of what to do about it were the subject of conversations at RAND in 1966-68. The background of those discussions includes the substantial commitment of the RAND Economics Department to the cause of finding prescriptively useful insights into research

and development activity, and a body of prior work by Nelson on economic growth, invention and technology. There was in particular the 1967 book by Nelson, Peck and Kalachek (Nelson, Peck et al. 1967), which included an early attempt at a very micro level account of production realities. And the background also included some of the early contributions to the gradually emerging “Stanford-Yale-Sussex synthesis” on the nature of technological knowledge (Dosi and Nelson 2010).

Firm behavior and the Carnegie School. The later 1950s and early 60s were the high point of a remarkable “golden age” at the institution then known as the Carnegie Institute of Technology (it became Carnegie Mellon University in 1967 ). Major research advances occurred across a broad front, but for present purposes the important front is the study of business firm behavior, and of decision making and organizational behavior generally. The leading figures in that effort were Richard M. Cyert, James G. March and Herbert A. Simon, but history has since revealed that the graduate students attracted to Carnegie in that period were also a formidable crew. In terms of publications, landmark achievements included (Simon 1955; March and Simon 1958), (March 1962), and ultimately (Cyert and March 1963).

The research program of the Carnegie School emphasized the value of direct observation as a guide for descriptive theorizing about firm behavior. Thus, the orthodox view that the theory of the firm should remain trapped in the optimizing actor framework was rejected first at the level of methodological principle. The direct observations encouraged by the methodological commitments produced new facts that were at odds with mainstream optimization notions. But, as noted previously here, there was already a considerable store of established facts to the same effect. In particular, the Carnegie observations underscored the point that the sorts of choices

made at the operational level in a large business organization --- including choices of input combinations, output levels and prices – were not guided by a case-by-case optimization analysis of alternatives, as the economics textbook pretended (and pretends), or indeed by any optimization analysis at all. Rather, they derived directly from “standard operating procedures” and local negotiation or problem-solving, and indirectly from a variety of high-level commitments to strategies, policies, heuristics and cognitive frames.<sup>11</sup> Further, those commitments often involved considerations extending well beyond the particulars of the operational decisions, so they deserve a place in a proper framing of the decision problems.

Assuming that the power of the profit motivation is greatest at the top management level, there is commonly a substantial gulf between the locus of that experienced profit motive and the decision domains featured in the textbook (Gordon 1945; Gordon 1948). This gulf is not necessarily a reflection of bad management. It can certainly be argued that a profit-seeking management is well advised to let the company run on “automatic pilot” at the operational level, at least most of the time, for there are other important things to attend to. Even after giving full weight to that point, however, there remains the question of the actual purpose and value of the account given in those textbook chapters.

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<sup>11</sup> A standard operating procedure is, according to (my) preferred terminology, an example of “nominal routine,” i.e., a prescriptive characterization of a procedure for doing something. The ironic use of “SOP” – characterizing a shortfall as the usual sort of mess we make of things – underscores the distinction between a nominal routine and the true routine, i.e., the repetitive behavioral pattern.

As I pursued the development of my critique of Friedman, particularly in 1959, I was certainly aware that a quite different assault on orthodoxy was being mounted at Carnegie. I did not dedicate myself to fully understanding it however, perhaps out of anxiety that the overlap with my own work might prove to be uncomfortably large. (The graduate student struggling with a dissertation welcomes news of potential sympathizers, but responds with anxiety when hearing of rivals on a parallel track.) It was only when I read the completed Cyert and March book that the relationship of their work to my own began to come into focus. I had the good fortune to be asked to review the book for the *American Economic Review* (Winter 1964), which doubtless led me to read more carefully and reflect more deeply than I might otherwise have done.

As presented in Cyert and March 1963, the behavioral theory of the firm was a rival to the mainstream theory of the firm, but its descriptive strengths were not exploited in the analysis of events at higher levels of analysis – the levels of markets, industries or whole economies. Thus it did not pose a direct challenge to the mainstream on the sorts of questions that were the mainstream’s traditional concerns. For example, the role that competition might play as an “enforcer” of economic efficiency was not explored via an explicit model or account of a competitive situation, although some relevant illumination was provided in various places.<sup>12</sup>

It was not difficult to see that there was a strong anti-symmetry between the Carnegie version of a behavioral program and my own explorations of the “Friedman conjecture.” The Carnegie scholars were centrally concerned with getting the picture of the firm right, and they

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<sup>12</sup> Notably, in the discussion of organizational slack and in Williamson’s “managerialist” chapter 9.

were not much concerned with how that image might fit into a picture of a larger system. I was concerned with the dynamic logic of the larger system, and whether and how it could compel individual firms to behave in certain ways. For the most part, the behavioral rules that I ascribed to firms were designed to be convenient in the analysis of the larger system (paralleling the way mainstream theorists commonly behave); they were only lightly seasoned with concerns about descriptive accuracy. The anti-symmetry suggested a possible complementarity and a “What if ...” question: What if the behavioral assumptions of an evolutionary model were chosen with an eye to empirical plausibility rather than formal convenience? That might be the path to make the behavioral theory of the firm come to life in the analysis of larger systems. And that was substantially the message of my AER review, which congealed into a maintained assumption that Carnegie-style behavioralism and organizational theory would be an important ingredient of a successful evolutionary program.

A neo-Schumpeterian synthesis? As I noted at the beginning of this essay, the path to NW 1982 was marked by a number of significant turns in which chance played a major role. A particularly striking example involves the story of how the legacy of Joseph Schumpeter joined the list of major influences, and provided the “neo-Schumpeterian” label for the cause. “It could reasonably be said that we are evolutionary theorists *for the sake* of being neo-Schumpeterians...’. (NW 1982: 39). There are layers to the story, and the deeper ones would be hard to excavate. Nelson and I had certainly read a portion of Schumpeter’s work by the time we met at RAND, and doubtless that exposure shaped the way we thought about things. One could argue on that ground that Schumpeter would have counted as a major influence for that reason alone, but it would be hard to prove.

The more accessible layer of the story dates to the fall of 1967. Burton Klein was then the head of the RAND Economics Department. Formally, he was my “boss,” but his style of intellectual leadership was decidedly informal and non-directive. One day he came into my office and asked if I owned a copy of Schumpeter’s early work, *The Theory of Economic Development* (Schumpeter 1934 [1911, 1926]). I did, and as I lent him the book we talked about Schumpeter, the relationship of Schumpeter’s ideas to my work and Nelson’s, and, particularly, the question of whether Schumpeter’s influence would have been greater if his basic ideas had been expressed mathematically. The upshot of the conversation was that I was signed up to give a talk on Schumpeter’s view of technological change in an undergraduate course that Klein was then teaching at the California Institute of Technology.

In preparation for that talk, I re-read “TED” with eyes that were prepared by recent discussions with Nelson on the growth and technology themes sketched above. I found the book (particularly Chapters 1 and 2) to be a revelation. As a stepping stone to the expression of Schumpeter’s large-scale vision of capitalist development, these chapters first provided a striking characterization of the economic system as it would exist *absent* entrepreneurs and innovation – the situation of the “equilibrium circular flow.” What is interesting is not the respectful nod to Walras that is certainly a part of Schumpeter’s intention in that discussion, but the sketch of how the production possibilities that are “data” at a point of time can be viewed not as permanently “given” at the firm level, but as a temporary system outcome in a historical process.<sup>13</sup> The sketch pointed to the question of what it really means for a firm to know how to

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<sup>13</sup> Nelson has recently reviewed the contribution of TED and its relationship to evolutionary theory (Nelson 2012). He sees more destructive tension, or incoherence,

produce something (the focus of much of the conversation with Nelson), and how that changes in historical time. This way of reading Schumpeter was reflected in the Cal Tech talk. In the months that followed I pursued the matter further, and ultimately wrote up the results in a RAND paper, “Toward a Neo-Schumpeterian Theory of the Firm”(Winter 2006 [1968]).

Through the frame that Schumpeter provided in TED, one can see how the reality of “profit seeking” in a dynamic economy contrasts with the “profit maximization” assumed in mainstream theory. The really fundamental problem with the latter is not with the motivational assumptions, or even with the (missing) account of realistic processes that might accomplish the maximization. The deep problem is with the opportunity sets, and the failure of the conventional apparatus of production theory to make contact with the way productive knowledge actually operates in the world, the way it evolves, and how that evolution shapes the choices open to firms. As noted previously, the isolation of neoclassical growth theory from the micro-detail of technological change was a motivating concern of the Nelson-Winter enterprise from the start. With the benefit of hindsight and the illumination from much other work, it became apparent that the Schumpeter of TED had actually made a significant start on a more satisfactory approach.

Schumpeter’s later work, *Capitalism, Socialism and Democracy* (Schumpeter 1950) portrayed capitalist development on a broader canvas, a picture that strongly reflects its

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between the “Walras nod” and the innovation story than I do. We agree on the point that Schumpeter’s own microeconomics of the “circular flow” is not conventional neoclassicism, but strongly emphasizes habit and routine. Hence it suggests the plausible conceptual baseline for the innovation story that neoclassicism lacks. In this connection, see the Schumpeter discussion in (Winter 1971).

historical context in the middle of the twentieth century. That portrayal focused attention on innovation incentives and capabilities, and particularly on how the circumstances of large and small firms, or concentrated and un-concentrated industries, might contrast in these respects. These issues received much attention from scholars of diverse persuasions, including mainstream industrial organization economics. In NW 1982, they were addressed particularly in Part V, Chapters 12-14. A substantial stream of evolutionary modeling moved forward from there.

Looking back to Burt Klein's original question about Schumpeter and mathematics, it is clear that the CSD ideas related to the "Schumpeterian hypothesis" yielded more easily to formal treatment than the productive knowledge insights of TED. The latter still pose major challenges to formal modeling, but they are also more fundamental to the evolutionary program.

The RAND phase of the long conversation came to an end in the summer of 1968, as Dick Nelson and I accepted professorship offers at Yale and Michigan respectively. I went back to the "business" of teaching mainstream microeconomic theory at the doctoral level, a function in which evolutionary theory played a minimal role. Beginning in 1969, the conversation resumed, focused initially on the idea of converting the 1968 working paper into an article. And it was initially focused on the problem of connecting the micro-level discussion of production to much larger issues. That project failed. Instead the product of those early efforts emerged, thirteen years later, in a book. The discussion of organizational routines and capabilities was presented as background for discussions and evolutionary models of larger-scale issues.

### **3. Individuals in (or for) evolving organizations**

The concepts of organizational routines and capabilities have been quite influential in the literatures of management and organization studies, and particularly in the field of strategic

management. The ultimate wellsprings of this influence are widely dispersed in the social sciences and beyond, and the influence itself has shown a typical evolutionary tendency toward the proliferation of types. The account given in NW 1982 was to a large extent a synthesis inspired by many sources, and particularly by the Carnegie School sources already mentioned.<sup>14</sup> Its generous reception among management scholars and others outside the economics discipline probably derives in some part from the sorts of frustrations that motivated the account in the first place: It is easy to believe that there is a powerful economic logic at work in the world, but much harder to believe that mainstream economics has it right. Part of that shortfall is plausibly attributable to the weakness (or absence) of the account of organizations in mainstream theory.

The microfoundations challenge. The relatively general and calm acceptance of the usefulness of the concepts was disrupted, in the management literature, by a sharply- worded challenge from Teppo Felin and Nicolai Foss (Felin and Foss 2005). Felin and Foss argued that the routines and capabilities story was flawed by a kind of original sin, namely, defiance of the philosophically sacrosanct (according to them) principle of methodological individualism. They challenged the legitimacy of positing entities that reside conceptually at the organizational level, making a charge of “methodological collectivism” against those who considered the concepts of “routines and capabilities” to provide useful guidance in the quest for theoretical explanations. Philosophical arguments aside, they called for more serious attention to the role of individuals in these organizational phenomena. In early statements, they made explicit reference to the parallel

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<sup>14</sup> Some of the most relevant history from the viewpoint of management scholars is sketched in (Jacobides and Winter 2012). The available handbooks are effective in suggesting the broad scope of the relevant literatures, see (Helfat 2003; Becker 2008)

issue in mainstream macroeconomics, where microfoundations are deemed sound only if they involve optimizing behavior at the individual actor level. As time went on, that specific concern attracted relatively less attention, but a rich and voluminous discussion emerged on the broader question of the role of individuals. A substantial spate of journal activity was devoted to it.<sup>15</sup> Among my several contributions to that discussion, Winter (2013) is most squarely directed to the original criticisms – the philosophical points and the challenge of characterizing the role of individuals in routines and capabilities.<sup>16</sup>

Today as in 1967, the shortcomings of our knowledge with respect to the behavior of large organizations seem to me to be much more consequential than the shortcomings regarding individuals – and therefore, a reasonable sense of research priorities suggests further attention to the former. That is not to say, however, that further attention to the individuals and (particularly) the cross-level connections, is unproductive.

Although our recent critics never engaged with it, Nelson and I did at least sketch (in Chapter 4) a sort of “microfoundation” for the organization-level analysis in NW 1982 (Chapter 5) We proposed a shift of focus in the individual-level account from the universal calculative

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15 The list includes: *Journal of Institutional Economics* 7(2), 2011; *Organization Science* 23 (5), 2012. *Journal of Management Studies*, 2012, *European Management Review*, 2012 . Doubtless there is more.

16 My own views on the philosophical issues derive from a number of sources, but in general they are well aligned with the arguments of John Dupré (Dupre 1995). Thanks to Paul Nightingale for this reference.

excellence of mainstream optimizers to the specialized excellence that resides in the habits and skills of experienced individuals. These two kinds of excellence are quite distinct, although there are zones of overlap – competence at mental arithmetic, for example. The failure to recognize the importance of that distinction is major source of confusion that afflicts mainstream economic thinking in all forms, all the way from the assumptions of formal models to through the highly-touted capacity to “think like an economist” to the policy recommendations that derive from those intuitions.

To develop that case, we drew upon then-recent psychological research on the behavior exhibited in repetitive or quasi-repetitive situations. We also drew considerable inspiration from the work of Michael Polanyi, particularly from his account of skills and tacit knowledge (Polanyi 1962 [1958]). Though not a psychologist, Polanyi was a penetrating observer, a deep thinker, and a persuasive expositor. After reading Polanyi, one comes away with profoundly altered view of what knowledge is and how it “works” in human thought and action – at least, that was my experience.

The recent call for stronger microfoundations in the case of routines and capabilities directs attention to the need for something that goes beyond the sketch that we provided in 1982. Progress in that undertaking would not only further evolutionary research today, it might have fruitful implications in a far wider domain – since the basic issues are relevant across the social sciences. The need is for a theoretical conceptualization of individuals that establishes notional building blocks for theories of higher level systems, such as organizations, industries or whole economies.. To posit such an objective is to offer an implicit acknowledgment of the success of mainstream economics in creating interesting accounts of larger structures by building on its

preferred foundation of rational choice at the individual actor level. But those super-stylized neoclassical individuals need not be the only candidates for such a theoretical role. In particular, as suggested above, we cannot abstract from the fact that humans have habits and skills derived from their experience and then hope to understand organizational routines and capabilities – and many other phenomena. Speaking to the microfoundations issue in evolutionary economics and management research, Warglien et al. commented as follows:

We do indeed need greater precision and logical coherence, as has been argued, but the formalizations we employ must be consistent with the psychological processes of actors whose actions are determined in large part by learned habits and associations rather than by deliberating over the likely consequences of exogenously defined alternatives.”

--Warglien, Cohen and Levinthal (2014)

That corresponds to my own view of the matter. Of course, such emphasis on learned skills and habits as the *immediate* source of action does not rule out a possible role for deliberation at an earlier stage; many individual skills are deliberately acquired, many organizational routines are deliberately designed and systematically introduced. When action takes highly repetitive forms, however, it tends to become highly automatic regardless of the presence of deliberation in the causal background:

“The necessary and sufficient conditions for automation are frequency and consistency of use of the same set of component mental processes under the same circumstances— regardless of whether the frequency and consistency occur because of a desire to attain a skill, or whether they occur just because we have tended in the past to make the same choices or to do the same thing or react emotionally or evaluatively in the same way each time.”

--Bargh and Chartrand (1999: 469)

#### **4. Relevant progress in psychology.**

Fortunately, the attributes of the needed theoretical individual have become a lot clearer as a result of psychological research. As is suggested by the above 1999 quote from two eminent psychologists, the passing years have been quite kind to the skills/routines capabilities story told in NW 1982 – provided one looks to psychology and not to economics for the supportive voices. The first and arguably the most important of the advances was the discovery in the 1980s of the phenomenon of “procedural memory,” otherwise known as “skill memory.” This advance was described and brought to bear on understanding of organizational routines by (Cohen and Bacdayan 1994), in their paper “Organizational routines are stored as procedural memory.” At a basic level, the paper is an empirical contribution, reporting an experiment that offers compelling support for the significant claim in the article’s title.<sup>17</sup> Its significance for the microfoundations discussion goes well beyond that, however. It sketches an explanatory bridge that runs all the way from brain physiology at one end – the physical locus of procedural memory – to the capabilities of large organizations at the other. The experiment specifically supports the central span in that bridge, which links the well-practiced behavior of individuals (skill) to the well-practiced and *coordinated* behavior of a group (i.e., organizational capability, in this case that of a dyad).

The Cohen-Bacdayan paper provides a persuasive model of strong “microfoundations” for the study of routines and capabilities. Its grounding lies at a level that is physiologically and causally “inside the black box” of the human individual. It is appropriately focused on the critical step from the individual to the collective level. The measured outcomes in the experiment stand

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<sup>17</sup> The experimental paradigm presented in that paper, involving a cooperative two-person game, was subsequently pursued by other researchers. See for example (Egidi 1995).

in for highly relevant aspects of “performance” at the collective level – the speed at which productive tasks are performed, sometimes called “productivity.” The problem of “origins” is addressed with due regard to causal ordering in the time dimension -- by identifying an initial state inherited from the past (the *ab initio* attributes of the experimental subjects), and then detailing and measuring the dynamic process leading from that state to the creation of a new entity at the level of collective behavior. As the paper demonstrates, some of the implications of this account of effective behavior are quite different, and testably different, from the choice-based accounts of mainstream economics. In particular, the paper shows how the performance speed that comes with experience is sensitive to the specific representation of the problem addressed, and not to its abstract logical structure. This in my view is the most fundamental of the many experimental challenges to the rational actor model: Rational actors make the same basic choices across all logical isomorphs of the same problem, but that is not the way humans behave.

More recently, a large body of relevant psychological evidence has been brought to bear and expounded with great effectiveness by Daniel Kahneman (Kahneman 2011). His discussion focuses on the “System 1 vs. System 2” distinction in contemporary psychology, which he also mobilizes as an expository device by treating the events in an individual mind as a “psychodrama with two characters” (Kahneman 2011: 22). Fast “System 1” includes “... mental activities (that) become fast and automatic through prolonged practice” (Kahneman, 2011: 22), which is an important part of the domain of habit. Among the things thus practiced are not only program for overt actions, but also *interpretive* skills -- the skills for reading people, situations and ambiguous signals, to which Kahneman gives great emphasis. System 2 deals with activities

that require attention, including computations, logical operations, following complex instructions, or attending to or identifying particular events or cases within a complex scene. Just as John Dewey spoke of “thought,” (i.e., deliberation), as born of “impeded habit,” (Dewey 1922), Kahneman declares that “When System 1 runs into difficulty, it calls on System 2 to support more detailed and specific processing that may solve the problem of the moment. System 2 is mobilized when a question arises for which System 1 does not offer an answer ....” (Kahneman, 2011: 24). Further, System 2 plays important role as planner, programmer and monitor for System 1, again paralleling the role of deliberation in Dewey’s account.<sup>18</sup> However, System 2 is “lazy”; displaying “a reluctance to exert more effort than is strictly necessary” (Kahneman 2011: 31). System 1, by contrast, springs into action unbidden, and often at the unconscious level. These contrasting features are two complementary aspects of the bounded character of human rationality at the individual level.

The first order contribution of the two- systems view is simply the fact that there are two, physiologically distinct system in action; some behaviors are shaped primarily by one system and some by the other, while many reflect a complex interplay between them. The second order contribution, illustrated by numerous examples in Kahneman, is the revelation of the complex interactions of the two systems. In particular, the automatic interpretations of stimuli that are speedily offered by System 1 often provide the basic material for the slow, reflective processes

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<sup>18</sup> Dewey’s distinction between habit and deliberation provides many of the insights of the System 1, System 2 distinction. Here I emphasize the more contemporary sources and, for reasons of brevity, do not develop the parallel arguments in Dewey’s terms -- but I agree with (Cohen 2007) that more attention to Dewey would be fruitful.

of System 2 – which may then choose action in the form of speedy, automatic enactment of some skill embedded back in System 1. Thus, at both ends of the process, System 1 is powerfully shaping behavioral outcomes in ways that cannot be understood by inquiring into the deliberative logic of System 2, whatever its own strengths or limitations may be. At the third and most challenging level, the systems must be recognized as not merely interacting, but mutually infusing. The deliberative mind (System 2) deals in categories that it “knows,” imperfectly, to correspond to System 1 processes. And it deals in those terms even when ranging far out from the immediate context, in a speculative or creative way.<sup>19</sup>

Giving due credit to the microfoundations challenge, it is true that the question of how those individual Lego pieces get arranged into a performance of a complex organizational capability is an important and very challenging question – much harder than the how and why of the mere maintenance of an existing organizational routine. Progress on this challenge is certainly needed, but there are some visible illustrations of it available, such as (Warglien, Cohen et al. 2014), which is most obviously consistent with the stance taken in this essay. There are other examples of responses to this challenge, such as (Pentland, Feldman et al. 2012). It is a significant challenge, and the responses to it will have a long life in the literature of the subject.

The two systems view underscores the analytical value of the basic Schumpeterian dichotomy between routine and innovation, as well as related ones like the “routine as truce” idea, at the individual level as well as the organizational level.<sup>20</sup> Yes, we should move beyond these dichotomies; and in many ways we have. At least, we have in evolutionary economics and in significant parts of the management and organization literatures. But acknowledging the

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<sup>19</sup> In this connection, see NW 1982: 85-88, on “the uses of skill names.”

dichotomy is the beginning of wisdom, and that level of wisdom has yet to diffuse widely enough. In economics, the mainstream continues its reliance on rational choice formulations that impute to actors the combined strengths of habit and deliberation, or System 1 and System 2, while failing to acknowledge the weaknesses of either and the existence of a contextually-determined division of labor between them. Thus, the theory imputes to the rational actor the speed and precision, and capacity for complex computation, that often characterize System 1 behaviors that lie within the practiced (or the innate) domain. The theory also imputes to the rational actor such System 2 attributes as an invulnerability to being deceived by trivial re-labeling of a presented problem, or by the presence of information that is in principle irrelevant to the decision; also, the capacity for checking the logic of an argument or to discover a plausible “creative” solution to an ambiguous and novel problem.

In trying to promote a microfoundational view that gives due weight to habit, we face the obstacle of the natural human tendency to accord ourselves the exalted status of reasoning beings. “When we think of ourselves, we identify with System 2, the conscious, reasoning self that has beliefs, makes choices, and decides what to think about and what to do....System 2 believes itself to be where the action is ....” (Kahneman 2011: 21) While that is to some extent true of all of us, it is particularly true of mainstream economists.

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20 I would add to the package the distinction between “on line” and “off line” search, between “deliberate” and “fully experiential” learning, and also the distinctions between levels in a “management by exception” system. The point is, it is the bottom, operating level where the environment is truly engaged, and that level is supervised, imperfectly, by levels above it.

While the complex relationships of the two systems offer many promising research avenues, it seems that researchers in the evolutionary economics and management communities have tended to blur the basic dichotomy unnecessarily as they have tried to move beyond it. There have been serious efforts to capture something of the “deliberative” firm in models of organizational problem-solving and learning. Unlike models in the mainstream tradition, these attempts take some form of bounded rationality as given. The models are numerous and come from diverse traditions; examples include (Dosi and Marengo 1994; Gavetti and Levinthal 2000; Dosi, Hobday et al. 2003; Rivkin and Siggelkow 2003; Marengo and Dosi 2005; Winter, Cattani et al. 2007; Christensen and Knudsen 2010). Largely apart from that sort of effort, there is a too-big-to-cite family of evolutionary models exploring market-level interactions of firms characterized by behavioral routines largely designed *ab initio* by the researcher -- albeit with due attention to empirical plausibility, particularly in regard to responsiveness to environmental feedback.

These two parts of the broad evolutionary tradition seem in my view to be weakly joined. The “System 2” features of model firms should participate in the design and modification of their “System 1” features. Some of that is built into existing models, at least in the form of basic “satisficing” principles governing the modification of routines. But that does not create firms that are as smart as the “System 2” models seem to imply.

On to the organizational level. The foregoing summary points to a well-supported and satisfying “foundational” account of human behavior, an account that (excitingly) awaits full exploitation at the organizational level in a research program that would follow the general lines pioneered by Cohen and Bacdayan. The first step across the bridge between levels involves

acknowledgement of the “multi-person skill” aspect of routines, an aspect that is alive in a literal sense in many examples, while serving as a metaphorical interpretation in others. Habitual behavior is evoked by contextual cues, and the fact that the cues flow inter-personally in an organizational situation does not by itself impose novel requirements on the habitual behavior of an individual or demand a special-purpose theory of human nature. In an organization, the skill repertoires of individual are like pieces in a giant Lego set, from which an innumerable set of complex performances can be constructed – and it is an expanding set because experiential learning in the shared organizational context adds new skills to the individual repertoires.

Further steps across the bridge involve attention to the organizational consequences of the affective and deliberative aspects of individual behavior, including the potential for intra-organizational conflict. This point was not neglected in NW 1982, and in fact the “routine as truce” metaphor introduced in that connection has proven fruitful in subsequent research, e.g. (Coriat and Dosi 1998; Zbaracki and Bergen 2010). While this metaphor is helpful in understanding the persistence of routines and the phenomena attending change, it does not do much to illuminate likely directions of change. On that, the histories and case studies tell us that processes of intra-organizational politics are particularly prominent and consequential in such episodes – with the implication that the unitary actor model of the firm is particularly misleading here. Modifying our image of the theoretical firm to take that into account remains a major challenge, but there have been some promising sallies. In particular, there are formal models of

firm decision that present it as *neither* unitary nor rational (in the neoclassical sense), yet are clearly capable of competing in sensible ways with similar entities in a shared environment. <sup>21</sup>

## 5. History, Institutions and Innovation

The concern with microfoundations could be considered anomalous in the context of an “evolutionary” theory, since the term is usually associated with emphasis on population-level logics and on processes of long-term change at scales above that of the individual. In this concluding section of the present essay, I consider some of the developments since 1982 that relate to those more traditional concerns – but I begin with a link to the individual level.

As was observed previously, the most fundamental difficulty associated with the neoclassical commitment to rational choice is the requirement to confront the model actor with objects of choice that are clearly specified *ab initio*. That requirement rules out from the start the analytical recognition of the open-endedness that arises in the many situations where alternatives have to be discovered or invented, or simply selected from an unmanageably large set of possibilities, before the task of choosing among them can be addressed. In such situations, the reality of resource scarcity means that some sort of satisficing approach to the delineation of alternatives is a forced move for a real actor. Further, this satisficing approach need not be one

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<sup>21</sup> See (Rivkin and Siggelkow 2003) , (Christiensen and Knudsen 2010) (Csaszar 2013). These models feature the structure of the decision process as opposed to politics per se, but the two are intimately connected. (As the aphorism goes, where you stand depends on where you sit.)

that involves *any* determinate stopping point for search for alternatives – the meta-alternative, “expand search” can remain on the list until the actual choice is made. Similarly, the passive meta-alternative, “remain receptive to suggestions and hints” can also remain on the list. The existence of these two meta-alternatives is a familiar fact of experience in many contexts. The fact is that the set of alternatives is typically endogenous to the process, and any process that is highly atypical in that respect is appropriately viewed with skepticism.

On the other hand, the list of alternatives that is ultimately considered is typically a small subset of those objectively available, and the selection is shaped by a wide range of conditions and contingencies that are in large part determined by wider social processes. This is true whether “the focal decision maker is a mega-corp CEO looking for an acquisition, a project leader looking for a new –technology that will address seemingly impossible requirements, or a college student looking for marketable skills. The relevant social processes are different, but the fact that the alternatives are neither intrinsic to the actor nor pre-ordained is a unifying theme. For a social scientist trying for any sort of prediction of the decisions (however loose), it is important to try to identify the processes that determine the decision options in the specific case under examination.

This discussion suggests one path from the micro to the macro level, involving attention to the multiplicity of institutions that shape the decision options faced by individuals and firms.. Researchers guided by the evolutionary view have been particularly active in the study of technological change and innovation, a domain where the institutional shaping of alternatives is obvious and particularly significant. Indeed, a survey study conducted by Verspagen and Werker concluded that “... that the label ‘evolutionary economics’ is a relevant one for

describing the core of scholars in the field of the economics of innovation and technology” (Verspagen and Werker 2004). Inquiry has pursued a number of different paths. The three paths briefly described below illustrate the sharp divergence of the evolutionary approach from the mainstream view of technology and the firm. The emphasis is on change, on processes that are extended in time, and on levels of social organization above that of the firm.

Innovations systems and policies. A substantial literature on “national innovation systems” developed following the seminal 15-country comparative study by Nelson and colleagues (Nelson 1992). The country studies feature the innovative activities of firms, seen in contexts set by the levels and patterns of government funding of R&D, the training and research functions of universities and the impact of government policies, and other institutions that demand attention in the context of a particular country or sectors. The overall picture is one of great diversity, reflecting in part the diverse situations of the countries but also the many different pulls deriving from the objectives and interests affecting national policy.

There are high-level propositions from theoretical welfare economics that have some realistic bearing on what is seen in innovation policy. In many countries, even small ones, national security concerns drive spending both on armaments and on efforts to advance military technologies. This is plausibly interpretable as a reflection of the fact that “defense is a public good” and also “information (or “knowledge”) is a public good. Public investment in basic research, and in the educational infrastructure that enables both the conduct and exploitation of such research, is another area where a “market failure” explanation has a face plausibility. Today, the massive negative externality from the burning of fossil fuel has become a significant driver of innovation policy as well as bringing the phrase “Pigouvian taxation” into the popular

(or at least non-expert) discussion. In my view, the high-level theoretical understanding of these matters is one of the accomplishments of mainstream economics that is valuable and ought to be cherished. It would be a big step forward if the general public shared that general understanding rather than being perpetually bogged down in contesting the absolute merits/ shortcomings of “the government” versus those of “the market.”

The illumination from that high-level theory fades quickly, however, as attention is directed to more detailed levels. National innovation “systems” are generally more of a congeries than a system, if “system” is taken to imply a logically coherent structure. They reflect the accumulation of responses to relatively specific policy issues, but the impact of those responses may extend very widely. In the U.S. case, the Cold War provided a high-level rationale for the federal government’s investment in many technologies, but specifics of funding allocations and program designs turned on bureaucratic visions of a (then) remote future. The long-running involvement in the development of information technology on the part of the U.S. national security establishment is a leading case in point. The list of the specific projects is far too long to present, much less discuss, here. See Mazzucato (2014) for a review of many of the high spots, especially particularly her review of the government’s role in the technological origins of the smartphone (Chapter 5). Or see Malerba et al. , (forthcoming) for the short histories of the U.S. computer, semiconductor pharmaceutical industries, each of which involves the hand of government at critical developmental stages.

These examples, and many like them, illustrate the magnitude of the error involved in thinking of the production activities of the contemporary economy in the terms suggested by the general equilibrium model – decentralized among a number of independent firms, each endowed

with its individual production possibilities, and coordinated primarily by markets. Show me a situation that is approximately like that and I will show you a technological backwater, a potential customer for a World Bank development aid mission. Evolutionary economists seek paths that lead away from that error, and toward a broad understanding of the social processes that are the sources of the alternatives that turn up when a firm goes searching for better routines.

Dynamic capabilities. In a world of continuing change, the ability to cope with change is obviously a key trait affecting organizational performance and survival. While the quasi-automatic responses of established routines cope with a certain range of familiar variation, outside that range, the inertia of those established routines can spell trouble. At the same time, a firm confronting the challenges of change typically has existing dispositions to search for alternatives in some directions and not in others. Thus the set of adaptation possibilities that emerge is not the fruit of a fundamental confrontation with the logic of the changing situation, but reflects the joint influence of prior experience, existing assets and perceptions of the current circumstances. The option of starting over, with no preconceptions and no commitments, not available for an established firm.

In NW 1982, we took note of the typical hierarchical structure of organizational routines, basically drawing on Carnegie insights about “problemistic search.” Given a perceived problem and some frustration of the effort to solve it, the domain of the search for solutions tends to expand. That is, in particular, the tendency of “management by exception” systems. On top of the operating routines, and their direct engagement with the external environment, large organizations often have layers of practices or “meta-routines” devoted to control and correction mechanisms, with each layer devoted to noticing and possibly fixing any problems that might

bubble up from the layer below –or alternatively, passing the problem to the layer above. After many layers of potentially constructive response or buck-passing have come up short, the problem reaches the top management team or the CEO. As Harry Truman famously said about his job as President of the U.S., “The buck stops here.”

The notion that a firm’s adjustment to change can be facilitated by higher-order decision processes has been influential in the strategic management field since the publication of a paper by Teece, Pisano and Shuen, entitled “Dynamic Capabilities and Strategic Management,” which appeared in the *Strategic Management Journal* in 1997 (Teece, Pisano et al. 1997). By the time of that publication, the paper was already quite influential, because various working paper versions had been circulating for five years or more. Its principal published version has accumulated, as of my most recent check, 22,377 Google Scholar citations.

According to Teece et al, “dynamic capability” refers to a “firm’s ability to integrate, build and reconfigure internal and external competencies to address rapidly changing environments.” Since the publication of the seminal paper, there has been an extended quest for the best definition of the concept, and also a substantial amount of controversy about the concept itself. A major issue shaping those linked discussions is the reliance on a “what it does” characterization of the concept (as above), as distinguished from a “what it is” characterization. One consequence is vulnerability to charges of tautology (the capability is recognized when effective adaptation is seen), and a second is a shortage of conceptual guidance for an operational approach that is *not* vulnerable to the charge of tautology. Beyond that (but still linked), there is the question of whether dynamic capability is something that characterizes the firm as an

organization, or its leadership, and perhaps only its CEO. These rival possibilities are probably best regarded as empirical hypotheses, on which some data might be relevant.

Dynamic capability, like ordinary capability, derives its power from organizational learning that is the fruit of experience. Further, while the benefits of experience are expressed more significantly in some organizational roles than others, it is far from the case that they are narrowly confined to a small group at the top of the organization.

For a highly significant example of dynamic capability, one could hardly do better than to consider the multi-decade trajectory of Intel, which is intimately linked to the technological trajectory of semiconductor miniaturization summarized in Moore's Law (Dosi 1982), (Winter 2008). As every 18 to 24 month generation went by, Intel repeatedly did something quite new ... or on the other view, "not so much." Not so much, at least, if the standard is the degree of novelty to Intel, as distinguished from the degree of novelty to the rest of the world. The distinction between what brings novelty to the world and what is experienced as novelty by its producers is fundamental (Winter 2008). The world rides a turbulent wave of technological and organizational change, not primarily because of new creations that are impressively and "objectively" novel by some global standard, but because the world sustains a greatly diversified fund of knowledge and competence, from which influential novelties frequently emerge. What seems novel and may be transformative to the world is typically an incident in a process of incremental evolution at its point of origin – as the successive generations of semiconductor devices forcefully illustrate.

The foregoing account represents dynamic capability as a sort of System 1 function at the organizational level: Things go relatively smoothly and quickly, because, even though it is a

change process, much of the process has been enacted before. There is also a role for mechanisms that are the organizational counterpart of System 2, a capacious conceptual box in which we could identify several factors, including managerial leadership, and elements of organizational structure and culture, and the particular processes that shape organizational learning. In assessing the role of managerial leadership, the distinction between the individual and the organization comes to the fore. If the CEO, or the top management team, is the key to dynamic capability, then we need to look more carefully at the processes that determine which individuals fall into these roles in large organizations. If it is the organization, we need to understand how the organization-level manifestations of System 2 actually work ... the domain of projects, meetings and so forth. While the substantial literatures on such issues range far beyond dynamic capability, evolutionary economics is not strongly represented there.

Intel provides a leading example of a situation that is commonplace in that part of the contemporary economy that involves complex products and advancing technologies. In the product market manifestations of the situation, it is common to see progress represented by a sequence of product generations, with each new generation incorporating a cluster of advances in component technologies, materials and system designs. These advances are predominantly incremental but larger steps are occasionally taken.

The ability to accomplish product innovations in this way-- the dynamic capability -- involves the orchestration of specialized personnel, equipment and facilities that are dedicated to the tasks of change, which implies that the firm bears significant costs that would not be incurred if the product were unchanged. Viewed in the context of the current product generation these costs are an overhead. The ability to carry that overhead and thus stay in the game for future

generations depends on the markups and volumes realized by the firm's products in the current generation. Dynamic capability of this kind therefore tends to be an attribute of relatively large firms and ones that have of strong technical reputation or otherwise command customer loyalty. For many important product technologies, the story of advances over the years could hardly be told without reference to the dynamic capabilities of particular firms. The details depend of course on the specifics of the technologies and products involved.<sup>22</sup>

## **6. Concluding Perspectives**

For evolutionary economists, the problem of understanding the processes of long-term economic change is central on the research agenda. Those processes extend over substantial time periods and confront decision makers with pervasive uncertainty. As Veblen argued long ago, engagement with that problem requires first of all an engagement with the reality of cumulative causation, i.e., the layering of new changes on past changes and the proliferating recombination processes that are a feature of that layering. It also demands attention to the relatively constant background factors that persist through the generations of economic change, including the institutions that support technical advance and those of the market economy. As has been partially described in the previous section, many of the world's large firms operate at the junction of these considerations, being deeply embedded in the institutions of the market as well as those of the knowledge system, and enacting Veblen's "cumulative causation" through

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<sup>22</sup> The role of informational scale economies in the joint determination of firm size and R&D intensity was addressed in NW 1982, Chapters 13-15. It was also a prominent feature of Steven Klepper's work on industry evolution, e.g. (Klepper 1996), (Cohen and Klepper 1996), and figures importantly in (Malerba et al., 2016 forthcoming).

the exercise of their dynamic capabilities -- repeatedly constructing new futures on the platform of the past. In the other tail of the relevant distributions, where firms are small and their average lifetimes short, a quite different process of change is driven by the small portion of highly innovative firms in successive cohorts of entrants. Those “gazelles” typically rely more heavily on their direct connections to the knowledge system, and their ability to exploit the networks of productive competence in their specific fields. They generally attempt things that are both newer and smaller than what the large survivors attempt, but sometimes succeed at it quite spectacularly.

The intertwined roles of time, uncertainty, advancing knowledge and changing markets pose a formidable challenge for participants and analysts alike. In approaching the analytical challenge, evolutionary economists have the important advantage of standing on the side of realism in the debate that goes back to (Friedman 1953). We are not limited to the exploration of toy models, whether of the type expounded on blackboards in 1953 or the modern type typically captured in a Bellman equation with a single state variable. We rely on empirical insights derived from close examination of actual firms, whether conducted by scholars within our ranks, or by specialists in the “old” industrial organization, historians of various types, or behavioral scientists of various types. We subscribe to a view of individual psychology that gives due weight to habit, and do “reject the lore of nicely calculated less or more,” except (to some degree) in financial markets. This research program has helped to energize a wide range of scholars who are not much tempted by the abstractions and toy models of mainstream economics. One could hope that there are some graduate students in mainstream economics programs who have actually read some of the critiques of the lessons they are being taught,

especially those featuring the predictive failures regarding the Financial Crisis and the ongoing Great Recession. Perhaps some of them, even some who are enrolled in prestigious American programs, will be led to think more carefully about what the economics discipline should be trying to do, and how to do it. With that cheerful thought, I conclude this essay.

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