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Preface to the Japanese Translation of *An Evolutionary Theory of Economic Change*

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Preface to the Japanese Translation of *An evolutionary theory of economic change*

When many years ago we turned our minds to creating *An Evolutionary Theory of Economic Change*, and arguing its merits, we had several reasons. Perhaps the prime one was our belief that many economic phenomena of major interest involved economic conditions, behaviors, and structures that were almost always changing rather than constant. This is prominently so in the field of industrial economics, where both of us were doing research and teaching. In many industries innovation was, and is, a principal vehicle of competition, and firms struggle to survive under conditions of continuing change. Similarly, the context within which international trade proceeds, labor markets operate, and economic activity more generally goes on, is also characterized by continuing change of a kind that is not tightly predictable, to which economic agents somehow must adapt. We argued that economic behavior under this kind of economic change could not be adequately framed and analyzed with the kind of economic theory that was the standard then – based as it was, and is, on the assumptions that economic agents can somehow choose the optimal course of action, and that the system as a whole is in equilibrium.

A second reason derived from the observation that while, in many cases, prevailing theory seemed to generate predictions that were broadly consistent with the data, the theory itself was widely regarded as highly "unrealistic." We judged, contrary to much prevailing opinion, that this lack of realism was not a trivial matter – though we would concede that the proper standard for realism is ultimately a deep question. More importantly, we believed that a more "realistic" theory could do the job just as well or better. Here we had in mind, particularly, the prevailing standard theory of the firm, which assumed that firms chose actions that maximized profits, but was mute about how they actually did that. If interpreted as a predictive theory about decision processes in firms, this theory was clearly inconsistent with what was known about the actual processes. A number of then-recent studies by organization theorists and empirically oriented economists had shown quite persuasively that firm behavior at any time was largely driven by a set of routines, decision rules, heuristics and practices that strongly molded what they did under various circumstances.

A related concern we had was that prevailing economic theory was the source of a major communications barrier between economists who adhered to that theory and scholars in neighboring disciplines, like psychology, political science and sociology (particularly organization theory), as well as business and technological history. These scholars were studying and writing about subjects that certainly seemed to be of central importance to economics -- how people actually behaved in the face of uncertainty, how firms actually made decisions, and how business organization and technologies had changed over time. Since the findings in these studies were not couched in the language of prevailing economic theory, economists tended not to pay attention to them, or tried to force them into the format provided by that theory, or argued the empirical facts when the forcing proved to be a problem. Actually, the same divide existed within economics between the theoretical believers and the many empirically oriented economists who did their research and writing in a way that did not adhere to the standard theoretical canons.

These three sources of our concerns were not by any means independent, and as our work progressed the interdependencies seemed increasingly central to the overall case. Particularly important is the fact that, in a world of continuing change, the opportunity sets of economic actors are among the things that are on the move. When opportunity sets are on the move, the distortion

produced by unrealistic assessments of actor competence -- assessments sustained largely by disciplinary parochialism -- is greatly magnified. As Schumpeter memorably said, "Carrying out a new plan and acting according to a customary one are things as different as building a road and walking along it" (*Theory of Economic Development*, p. 85).

Based on these and related concerns and beliefs we built a broad economic theory that treated economic change as an evolutionary process, with the behaviors of economic agents at any time represented as guided by a set of routines that molded what they did under different circumstances, but also subject to change through learning and innovation. We very much hoped that the change in theoretical viewpoint we proposed would be appealing both to empirically oriented economists working in the fields that were the center of our concerns, and to other social scientists who would find our theoretical perspective congenial to and consistent with their own beliefs, and thus would enable much more cross-disciplinary interaction.

In the last paragraph of our book we concluded: "In summary, the analytic vantage point of an evolutionary theory reveals things from a different angle. After one gets used to that viewpoint, it turns out that much of what is seen is familiar. However, previously unnoticed features of the familiar objects become apparent, and some objects once visible from the orthodox angle have mysteriously vanished. Were they real or only an illusion? Things hitherto overlooked come into view – not merely different facets of familiar objects, but also entirely new objects. In all, the view seems clearer, as if the different angle had provided relief from distorting shadows. One hopes that others will come to appreciate the view."

We have been highly gratified that others have. The large interdisciplinary community studying technological change now is virtually unanimous that the process needs to be understood as evolutionary. While we were not the only scholars making that argument, it is clear that our writing has had a very great impact on the field, particularly among social scientists working in it. We had hoped that our evolutionary perspective on technological advance would serve to pull together the orientations of economists and other social scientists working in this field, and the work of historians, and it clearly has.

The theory of firm capabilities and behavior that we espoused provided the basis and frame for a wide range of scholarship on firm management and strategy. In particular, the emphasis we placed on organizational routines has spawned a significant body of research and writing on that

subject. In this area our own contribution drew extensively on the work of others, in particular the organization theorists at (the then) Carnegie Tech. Our emphasis on organizational routines as the basis of organizational capabilities, and on the augmentation of capabilities as generally involving the re-combination of old routines and the mastery of new ones, clearly has influenced the development of research in this field.

Our own interest in developing an evolutionary theory was, as suggested above, strongly influenced by our perception that Schumpeter's characterization of the nature of competition, which stressed innovation, uncertainty, and disequilibrium, was clearly the way that competition and the determinants of industrial structure should be understood in many key industries. The standard mode of economic theorizing seemed totally inadequate for analysis of this kind of competition. Since we wrote, there has developed an extensive body of empirical and theoretical work by economists, based on our evolutionary theory, concerned with Schumpeterian competition and the industrial dynamics in industries where competition is largely through innovation.

Over the quarter century since the original publication of our book, we have been greatly encouraged by the way new research has complemented and supported its program – not merely in the specific areas just discussed, but on a much broader front. Some of this work has been deliberately crafted in relation to our proposals, but most of it has appeared unexpectedly from across those disciplinary barricades that we sought to lower, or has been contributed by economists working from their own perspectives on topics close to our major agenda. To be clear, we make no claim to have stimulated all of this valuable work – we simply say, there it is, and a great deal of it seems to support the program. Given the range and volume of this research, we cannot possibly provide citations here. We limit ourselves to identifying in this text a number of topic areas and key words, and beyond that we provide also a short list of references at the end of this preface. The listed works offer some perspectives on the evolutionary program, its origins and progress, as well as an abundance of further references on specific points of fact and theory.

Our account of individual and organizational behavior included an emphasis on tacit knowledge and what is now called procedural memory. This broad account has since been complemented and supported by a great deal of research in psychology (procedural memory, situated cognition, distributed cognition), including research that probes these phenomena to the

physiological level -- true micro-foundations for an economic theory, if ever such existed. In our book's introductory critique of orthodoxy, we asked a question about the theory of the firm, "Where does the knowledge reside?" and subsequently went on to propose our own answer to that. In the ensuing years, there was an enormous boom of interest in the general subject of organizational knowledge, both in academe and in the world of practice (quality management, knowledge management, organizational learning). Further, our image of organizational knowledge based in the command of routines received such abundant support from studies in knowledge transfer and process improvement that our originally subtle question now seems almost elementary: Of course, *organizational* knowledge resides mostly in the routines (also known as "practices" or sometimes as "systems").

We noted, following Schumpeter, that innovation itself can often be routinized in significant respects. In the management literature, this notion has been extensively and fruitfully explored under the rubric "dynamic capabilities." We argued for a richer view of the connections between firm-level innovative activities and the broader knowledge context, noting that the latter is shaped both by information flows among firms and by a variety of institutional arrangements and policies. This theme too has been extensively explored, with an emphasis on mapping the ways in which the institutions supporting technical change differ from country to country, or, simply, from place to place. A substantial body of research and writing has developed on national innovation systems and innovation systems at the sectoral level.

Among the institutions the matter are those that affect the ability of firms to appropriate returns from their innovations, and those that link the innovative efforts of firms to the scientific fields on which they rest. On these fronts we have collaborated with others to generate a substantial amount of new empirical information, illuminating in particular the areas where our theory suggested that such information was badly needed.

In our modeling efforts, using both analytical techniques and simulation methods, we explored models involving populations of firms that are different in key respects. We said "Firms are different!" and put forward the models to explore the implications – at a time when theoretical economics remained strongly addicted to the "representative firm" abstraction, and/or reflected a view that the knowledge underlying firm capabilities was typically *public* knowledge (or would be public in the absence of intellectual property protection). In the ensuing years, it has been

overwhelmingly well documented that firms are remarkably and persistently different, even when their business is broadly the same and legal protection is not a major factor. This has been shown in different ways in different literatures. We mention in particular the body of research exploiting the large data sets derived from public sector census efforts in various countries, such as the work organized at the Center for Economic Studies of the U.S. Census Bureau. Yet, mysteriously, many economists still seem to react with surprise with confronted with the latest evidence that firms are very different.

We also argued the importance of understanding industry structure in dynamic terms, i.e., as one "outcome" of a continuing evolutionary process. One part of that territory relates to the explanation of the size distribution of business firms – a question which, at the time, was barely respectable within the field of industrial organization economics, perhaps because, once again, the facts are so strikingly at odds with the picture offered by the economics textbook. This was identified early on as promising territory, where simple evolutionary models easily come close to getting the picture right. Much work has been done in this area, both in modeling and empirics, and the question became more respectable. But the territory still ranks as "promising;" there is much that is important and not well understood.

Our book was largely silent in two related areas that are actually quite central to the proposal that it put forward. First, we did not say much about where the routines and capabilities of business firms come from originally; we spent our effort on discussing what they are like. Second, in our modeling of industrial dynamics, we did not attempt to address the facts of the typical development patterns of industries, across the various historical episodes. Instead, we spent our effort on elucidating some key mechanisms.

Fortunately, the scholarship of the ensuing years did not follow our example of neglecting these topics, far from it. Thanks to a body of research of several different kinds, much more is now known about these key aspects of the evolutionary process – and much more is recognized (by ourselves, among others) about their importance. We know a good deal about how routines and capabilities emerge from learning efforts in new firms and industries, and how this process itself is evolutionary, often including an obvious element of borrowing from the past. We understand also that the best and most vivid examples of the process of economic evolution, where

the market is mostly clearly acting as a selection environment for the diverse proposals of diverse firms, tend to be seen in the early decades of a new industry.

A number of distinguished Japanese scholars have been among those who have employed and advanced the concepts and techniques we put forward in our efforts to develop *An Evolutionary Theory of Economic Change*. Now Akira Goto, Atsushi Sunami, and Tatsuo Tanaka have put in the major effort needed to translate our book into Japanese, so that more Japanese readers will be able to draw upon, and build upon, our work. We are deeply grateful and honored to have such knowledgeable scholars make this significant commitment to the further progress of evolutionary thinking.

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Some Perspectives

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