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**Three Forewards**

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## Foreword to

Roman Frydman and Michael D. Goldberg,

### ***Imperfect Knowledge Economics: Exchange Rates and Risk***

*by* Edmund S. Phelps\*

Much has been written by historians and sociologists as well as business commentators about the modern economy – the kind that supplanted the traditional economy in several nations in the nineteenth century and many more in the latter half of the twentieth century. The pre-capitalist system dominated by the self-employed and the self-financed gave way to finance capitalism. To call this a “great transformation” was no overstatement.

A traditional economy is one of routine. In the usual illustrative example, rural folk periodically exchange their produce for the goods of the town. The sole disturbances are not of their doing and are beyond their control – rainfall, temperature, and other exogenous shocks. This was the economy modeled in the neoclassical theory of economic equilibrium from Ricardo and Böhm-Bawerk to Walras and Samuelson. It is also the economy described in the subsequent stochastic models of “rational expectations equilibrium” in the face of shocks that were pioneered by Arrow, Samuelson, Muth, and Lucas.

The modern economy is marked by the feasibility of endogenous change. Modernization opens the door for individuals to engage in novel activity – most importantly, the financing, developing and marketing of new products and methods. Furthermore, such innovations, when successful in the marketplace, have unforeseen effects on production possibilities, prices, the differentiation of goods and the specialization of work.

For decades, economics students have quietly asked themselves whether the equilibrium theory of the classroom is adequate for modeling the modern economy. It is one thing to know the prices at hand, another to know all prices far and wide and over the whole future and for every state of the world that shocks might land the economy in. Equilibrium theory implicitly takes the mechanisms that constitute the economy to be completely known: participants have a full understanding of how this organism works, so everyone knows the probability distribution of outcomes to expect in this or that state. This in turn

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implies that everyone knows this understanding to be common knowledge, so there is no diversity of views that would have to be guessed at in estimating what others intend to do.

History records a small band of economists who have called attention to points of dissonance between the modern economy and equilibrium theory, including the theory of rational expectations equilibrium, in which expectations (and thus prices) are taken to be appropriate for equilibrium in each possible current state.<sup>1</sup> In fact the growing perception, starting from the turn of the century, that the new modern economies were generally out of equilibrium, sometimes frighteningly so, is one of the hallmarks of twentieth century thought.

The great interwar theorist at Chicago, Frank Knight, pondering the arrival of capitalism, took the unprecedented position in his 1921 classic *Risk, Uncertainty and Profit* that virtually all business decisions other than the routine ones are to an appreciable extent a step into the unknown. The possible outcomes might have probabilities but those probabilities were unknown, or “unmeasurable” – the radical sort of uncertainty now called “Knightian uncertainty.” Viewing from London and Cambridge a similarly modern economy, John Maynard Keynes proposed in his 1921 *Treatise on Probability* a rewrite of probability theory that would take account of radical uncertainty. His 1936 *General Theory* was an attempt to overthrow equilibrium theory. In subsequent years, economists from Vienna to Copenhagen critiqued “perfect foresight” and its generalization, now known as rational expectations equilibrium.<sup>2</sup> In the glorious 1960s, several American economists broke from equilibrium theory.<sup>3</sup>

Trained professionals in that decade had a sense of what this anti-equilibrium literature was getting at. We thought that, empirically, equilibrium theory would not work well. For one thing, the economies (at least the world economy) we lived in had become too rich for equilibrium theory to fit at all well: forming correct expectations about a sole experiment, such as a lower price or a new variation on a product, is one thing but forming expectations when most or all firms are simultaneously experimenting is qualitatively different. For another thing, these economies were not really fluctuating around the stationary state or steady-growth path of neoclassical theory; they were

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<sup>1</sup> I discuss some of the implications of this anti-equilibrium view for economic activity and inflation-unemployment tradeoffs in the context of my early expectations-driven macroeconomic models in my Prize Lecture (Phelps, 2007).

<sup>2</sup> I am thinking of the game theorists Morgenstern (1949) and Zeuthen (1955).

<sup>3</sup> Ambiguity and vagueness were introduced by Ellsberg (1961) and Fellner (1961), personal knowledge by Polanyi (1958). Several applied papers followed in this spirit. For example, in a macroeconomic context, Friedman (1961, p.449) pointed out that long and variable lags in the effects of monetary policy imply that forecasting the consequences of monetary action is “not an easy requirement in the present state of our knowledge.” Phelps (1968a) argued that a (expectational) disequilibrium may be created by an undiagnosed structural shift.

constantly evolving in their structure and were changing too fast for economic relationships (between prices and quantities, for example) to have the durability that would be necessary for formation of accurate expectations about present and future data.<sup>4</sup>

Yet, a few years later, the community of macroeconomists, far from distancing themselves farther and farther from equilibrium theory, proceeded almost unanimously to embrace the rational expectations models of business activity introduced in the early 1970s. Keynes and the Cantabridgians were out. Hayek and the Austrians were out. So was Spiethoff and his German school.

This marvelous book by Frydman and Goldberg documents in its first two chapters invaluable insights of the “early modern” theory of capitalism that were lost when the profession endorsed rational expectations equilibrium. And it exposes to the light the arguments offered by the advocates of the premise of rational expectations. There can’t be many readers who won’t be fascinated by this story. In letting the two sides speak in their own carefully chosen words, the authors allow the expressed points of disagreement to come into sharp focus.

These chapters, however, soon probe to a deeper level. It isn’t just that the postulate of rational expectations is unrealistic in the same way that the postulate of rational choice is conceded to be unrealistic. We agree to work with rational choice in spite of its limitations, so why not rational expectations too? The primary issue is not an empirical one. Even if no firms at the current time were actually venturing a new price list, conceiving a new way to cut costs, devising new financial vehicles, contemplating a new product, and so forth, there would still be a problem: rational expectations equilibrium theory as an element of our models of the modern sort of economies contradicts the very essence of an economy in which economic actors are free to exercise their “creativity” by venturing to do something innovative.

The authors argue that if we aspire to build models that apply to modern economies – economies whose central functioning is the manufacture of change through their innovative activity and their adoption and mastery of the innovations made available – it is contradictory to adopt the rational expectations postulate that whatever change takes place in the future is already knowable and known in the present: that the economic change to be experienced is in a sense predetermined. Yet contemporary model builders embracing rational expectations have been undeterred or unaware of the contradiction: they either specify that there is no change in the world (the world they would describe with their models) or that whatever process of change is going on in the world can be incorporated in their models in a fully predetermined way.

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<sup>4</sup> The former argument is the theme of Frydman (1982). The latter argument is the theme of a recent paper of mine (Phelps, 2006a).

This criticism is not a narrow point that would be straightforward to remedy. The authors are not referring to the fact that the archetypal models of an economy enjoying rational expectations equilibrium have built into them an invariant trend-growth path to which the economy is constantly returning (as described by some transition dynamics). It is obvious that such a trend path is predetermined; the possibilities and probabilities are “prespecified” (in the authors’ preferred term). The authors’ argument is broader than that. If a rational expectations model supposed instead that the future was governed by a probabilistic linear birth process, so the model has no trend path to which the economy is tethered, there is still a fundamental indeterminacy: the possible states at a given future date are all known already and there is at present a calculable probability, conditional on the present state, of each such future state’s occurrence. In this model too, then, there is implicitly no possibility for the actors in the economy to create something unforeseeable, surprising, genuinely innovative. Thus, there is a methodological choice: to model on the premise of fully pre-specified future possibilities, which rational expectations requires, or to model an economy capable of endogenous change, which the modern economy is.

A recent case in point is the state-of-the-art model of the real business cycle type, where recognition is given to the accepted idea that opportunities are rosier at some times than others – the notion of “regimes” in which there are outside rates of return in prospect for investment.<sup>5</sup> At first blush this construct appears to capture an economy undergoing the occasional boom and the occasional slump at unpredictable times and having a future that feels not fully predetermined – and all this without sacrificing the precision of rational expectations equilibrium. The truth is, however, that this is a highly mechanical apparatus implying a finite number of states at any future date and imputing to each a calculable probability conditional on the economy’s present state.

An older case of equilibrium theory in macroeconomics is Joseph Schumpeter’s great 1911 work *Theory of Economic Development*. He saw the need to go beyond the Spiethoff-Cassel model, in which no entrepreneur appears and none is needed, only the occasional discovery of an exogenous scientist or explorer. Forced to choose whether to remain with the equilibrium perspective of his idol Walras or instead to regard entrepreneurs as creators in their own – figures creating the future – Schumpeter clung to the equilibrium perspective. The Schumpeterian entrepreneurs were merely the vessels the economy needs to carry out the commercial innovations made possible by the technology. The stock of undeveloped innovations were all “in the air,” each waiting for one of Schumpeter’s “entrepreneurs” to find it convenient to take on its financing, developing, and marketing. The rate of return of every project was known, at any rate to the experienced banker. The Schumpeterian model makes determinate (at least probabilistically) the rate of innovative activity and the

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<sup>5</sup> See Beaudry and Portier (2004).

time path of productivity – as if the creativity is all science and no commerce.

In contrast, to elaborate on earlier remarks, Keynes saw the rate of return as quite unknown and the demand for investment funds as driven by entrepreneurs' "animal spirits." Hayek saw that every participant has little or no knowledge of how the economy works as a whole, contrary to rational expectations; that a participant is apt to have only some highly specialized knowledge about his or her industry, which is itself apt to be quite specialized; that in some cases it is so deep as to be "private knowledge;" and that such knowledge may permit a creative person to conceive some new business strategy or new business product that is *not* in the air, *not* already known by all. In the struggle between these two worldviews, Keynes and Hayek were right but did not carry the day.<sup>6</sup>

As the rational expectations view has come under increasing suspicion, parts of the profession have jumped to the conclusion that the problem is "sticky" prices or some sort of rote behavior or "irrational exuberance" in asset prices or all of these. What Keynes and Hayek in the 1930s and Phelps in the 1960s understood is that there may be a problem with expectational equilibrium and it need not be sticky prices or irrationality but mainly the ever-imperfect knowledge of the structure of the economy and the attempt of purposeful market participants and policy makers to cope with it.<sup>7</sup>

If rational expectations equilibrium and its doppelganger predeterminacy must be regarded as inapplicable to the modern economy, the profession needs embark on its own voyage of discovery. The present book is devoted to setting out a fresh approach, one that is neither rational-expectationist nor behavioralist.

The authors of this book show that if we want to do coherent macroeconomic theorizing about a modern economy we are going to have to allow in our models for non-routine decision making and unforeseeable changes in the social context within which individuals make decisions. How do we build such a *theory* for modern economies?

The authors devote most of the book to developing such a theory, which they dub "imperfect knowledge economics." This economics builds in mathematical microfoundations of aggregate outcomes and yet it allows for non-routine ways in which market participants might alter the way they deploy

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<sup>6</sup> I would add that in relatively recent work (Phelps, 1994) I simply treat every shock as *de novo*, so the state it brings was fully unanticipated. Obviously this treatment is at odds with rational expectations. However, I regard the implications of that model to fit more closely with the behavior of national economies than do the models that invoke a stochastic stationary state with no room for parametric shifts.

<sup>7</sup> Leijonhufvud (1968) also attributes this view to Keynes, and he identifies himself with that view. I should add that, although I participated in the New Keynesian venture in the 1970s to rewrite Keynesian economy on the basis of rational expectations coupled with non-synchronous wage/price setting, my heart was always with the model in which wages and prices were continually being revised.

resources. The remarkable feature of these imperfect knowledge models is that, while they do not assume away non-routine activities, they nevertheless generate implications that allow an economist to compare empirically the performance of alternative explanations of outcomes.

How is this done? The key point is that imperfect knowledge economics focuses on *change* and looks for *qualitative* regularities, not quantitative ones. The authors' models impose qualitative restrictions on the way forecasting strategies are revised. While placing enough structure on an economist's model, these restrictions are general enough to be compatible with a myriad of ways in which market participants might revise their views of the future. Moreover, these restrictions recognize that *sharp* forecasts of what an individual will do are beyond the reach of any economic analysis of *modern* economies.

This approach resolves Knight's and Keynes's problem of how to reconcile the use of probability theory in modeling decisions under uncertainty. As Knight and Keynes recognized, neither the actors nor the economist-modeler know the probability distribution of outcomes. The key innovation of the authors is to model the change across time in these distributions and in a purely qualitative way (the authors refer to these as "partially predetermining restrictions") rather than to model the probability distribution at each point in time.

The three-decade-long debate between the Neoclassical and "Keynesian" schools over whether prices are sticky or flexible appears to be a mere distraction. In the context of the foreign exchange market, the authors show that, with incomplete knowledge, long swings in real exchange rates do not depend on whether prices are sticky or flexible. Rather, they arise from the imperfection of knowledge concerning the structure of the economy and market participants' attempts to cope with it. Moreover, in contrast to recently fashionable behavioral models, the authors' explanation of swings does not abandon the long tradition in economics that individuals behave in largely rational, or reasonable, ways.

Remarkably, once the authors allow for imperfect knowledge on how fundamentals influence the exchange rate, long swings can arise even if all market participants' diverse forecasting strategies depend solely on the macroeconomic fundamentals. It would not be surprising, therefore, if it is later found that a similar mechanism generates swings in overall business activity. (This possibility suggests that if modified by the authors' imperfect knowledge framework, my models of "structural slumps" would generate not a monotone shift from the initial steady state to the new one but rather a cyclical transition.)

In the conventional conception, as I pointed out above, market outcomes are mere vibration around a steady state path. Swings are viewed as anomalous and puzzling. Once imperfect knowledge is placed at the center of the analysis,

swings arise as part of the discovery process of how prices are related to macroeconomic fundamentals.

The authors' imperfect knowledge economics sees the modern economy as possessing bounded instability around historical benchmark values, which themselves may be evolving over time. The importance of historical benchmarks in characterizing individual behavior and aggregate outcomes was emphasized by almost all important early modern economists: Wicksell, Keynes, and Tobin, who built on this in his work on "behavior toward risk." (I imagine this view will be found to link well to my own work on movements of the medium-term natural unemployment rate.)

Another hallmark of the imperfect knowledge view is its qualification of fixed policy rules. The necessary point is that the optimum rule is not the same from one structure of the economy to another. As with the rest of macroeconomics, the issues have to be rethought in a way that makes the ever-imperfect knowledge of market participants and policymakers an integral part of the analysis.

I had the great good fortune in the 1960s to initiate the profession's work on plausible microfoundations for macroeconomic modeling, taking into account the knowledge and the information that the micro-actors could reasonably be supposed to have – a revolutionary movement it seems. Unfortunately, the rational expectations models, appearing in the 1970s, sidestepped the problem of expectations formation under uncertainty by blithely supposing that the model's actors (tellingly dubbed "agents") knew the "correct" model and the correct model was the analyst's model – whatever that model might be that day. The stampede toward "rational expectations," widely called a "revolution" though it was only a generalization of the neoclassical idea of equilibrium, derailed the expectations-driven model building that had just left the station. In the end, this way of modeling has not illuminated how the world economy works. Happily for me and I believe for the profession of economics, this deeply original and important book gives signs of bringing us back on track – on a road toward an economics possessing a genuine microfoundation and at the same time a capacity to illuminate some of the many aspects of the modern economy that the rational expectations approach cannot by its nature explain.



Foreword to  
Graciana del Castillo,  
***Rebuilding War-torn States:  
The Challenge of Post-Conflict Economic Reconstruction***  
by Edmund S. Phelps\*

A wave of civil war and cross-border conflict has swept over numerous countries in the past two decades: Afghanistan, Kosovo, El Salvador, Iraq, and others in Africa and Asia. It could have been expected that, when the conflict stopped, these countries would have found their footing again and set about to make up the lost ground. In fact, economic development has still not restarted in most of these war-torn countries. Many of them have regressed to a lower stage of development than they had attained before their conflicts. This state of affairs presents dangers to the rest of the world but it must be understood before it can be addressed with any prospect of success.

That the post-conflict countries, generally speaking, have not yet returned to visible development might be seen by some as an indication that these countries are permanently resistant to development – or have become so as a result of their conflicts. We know, however, that development has proved widely possible in spite of difficulties: on every continent we find countries that overcame enough hurdles for development to have begun. Indeed, some of the war-torn countries had shown some development prior to their conflicts. Rather than jump to the conclusion that most war-torn countries are barren of development possibilities, we might better look to see whether development has not resumed because some key pre-conditions for development have not been satisfied.

It could well be that governmental efforts at “reconstruction” in the war-torn countries have failed to address and even to identify some pre-conditions that have been missing in the aftermath of the conflicts. It is inevitable that governments will not get right all the conditions that a resumption of development would require. Reconstruction aid might be shaped by a conceptual framework for thinking about development and that framework is inadequate to the task – in all less developed countries and particularly in the war-torn countries.

The classical theory of development has undoubtedly been influential in

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shaping reconstruction aid, in part because of its outstanding simplicity. In this theory, development will self-start in a country once it undertakes the task of establishing property rights and the more difficult task of establishing a “rule” of law, which the economy’s participants can depend on. Then private interests can safely form enterprises and invest in the kinds of business for which they are best suited. By doing so, they will earn a living according to their human capital and the amount of land and other resources available to them.

Unfortunately, the classical perspective on development does not fully comprehend the richness of what development is – or could be. In the classical theory, leisure is better than work. Passive consuming is the final purpose of economic life. In any adequate view of what a rich development means, there is far more to development than that.

To set up a well-aimed reconstruction program it is necessary to have in mind a clear idea of what a rich development is and what it requires. The central elements in any such conception of development include mental stimulation, work to become engaged in, personal growth from meeting opportunities and challenges, the excitement of the new and the tingle of uncertainty. In my terminology, this means an economic dynamism – an economy of innovativeness in directions believed to be profitable. Of course, economic justice requires ample inclusion in this sort of economy. Obviously, these elements are fundamentally foreign to the classical conception of development.

Such development, it may be argued, requires what the Scottish Enlightenment called a “commercial society.” Thus, real development requires a country to create market institutions and a market culture in which business firms may safely function as well as a supply of conventional infrastructure and public services. As a result, the classical perspective, to the extent it is influential, focuses reconstruction efforts on a woefully narrow sub-set of the pre-conditions needed for development to start or re-start. The baleful influence of classical thinking could be a large part of the explanation why economic assistance has not been directed toward the reactivation of legitimate business enterprise, with its attendant investment projects, job creation and increases in productivity – and why little progress toward these goals has been seen.

Carrying out an effective reconstruction program to hasten the restart of such a rich development is a challenge, of course. There is no cookbook with recipes for all the institutions and cultures that serve to build an enterprising economy. In deciding on institutions and mores it will not do to look at other economies to infer what would work well or badly in a given country: copying institutions and cultures from countries at very different levels of development or different contexts is particularly risky. The idea of Rational Institutions – that a country can be depended on to choose the right institutions simply by virtue of its rationality and careful observation of other countries – is seriously mistaken. As the Enlightenment’s David Hume would have said, choosing the right

institutions requires “imagination.”

Besides its shortcomings, the classical doctrine is pernicious. It proscribes some kinds of programs that may be sorely needed. The idea promoted by some multilateral and bilateral donors that the war-torn countries can afford to follow laissez-faire policies – that in these countries unfettered markets work best and only the advanced countries need the paraphernalia of subsidies, licenses, regulations, corrective taxes, and so forth – is a costly ideology.

In a war-torn country where the economy has been devastated and may not bear the fruits of centuries of experimentation and diversification, there may be a need for judicious and well-designed departures from *laissez-faire* – just as the United States in the early years of the republic adopted some of the infant industry ideas of Alexander Hamilton. Prohibitions against any and all interventions in the market place in a country whose institutions and culture have been destabilized seems dogmatic and unjudicious.

In her insightful and timely book *Rebuilding War-Torn States*, Graciana del Castillo understands that reconstruction in the war-torn countries must aim toward a commercial society. She points to the failures of most of these countries, ranging from Afghanistan to Iraq and Kosovo, to create adequate job opportunities for the population, particularly for the younger population, which constitutes a large part of the labor force in these countries. Inclusion, integration, jobs and the dynamism that helps to realize these qualities: these ought to be the quest of the war-torn countries, del Castillo implies, just as they ought to be the quest of the economically advanced countries.

A vicious circle has followed in the war-torn countries. Lack of productive alternatives has driven farmers in post-conflict countries to growing illicit crops and has led others in these countries to engage in all kinds of illegal activities. The resulting lack of adequate jobs has contributed to public insecurity; it has also been a major factor in the tendency of these countries to revert to war. These results have in turn weakened the already slender incentives to restart normal business activities in farms and towns.

The author understands also that the nature of economic aid from the advanced economies of the West has much to do with the plight of the war-torn nations. She notes that there is no lack of western aid and assistance going to these countries. There has been humanitarian aid, which serves to support life and provide minimum levels of consumption. But it does nothing to promote the survivors’ development. There has also been much “reconstruction aid” channeled with the aim of creating conditions for subsequent redevelopment. But there are evidently deficiencies of understanding – and misunderstandings – of what reconstruction must do in these war-torn countries. Reconstruction aid has not been directed toward the reactivation of legitimate business enterprise and thus creation of jobs and increases in productivity.

The author understands too that some departures from laissez-faire are acutely needed in the war-torn nations. On the evidence of these countries, del Castillo argues that effective reconstruction, besides establishing the usual pre-conditions for development, needs to carry out a number of activities to reintegrate into the productive activities of the business sector an array of former combatants, returnees, displaced persons and other groups dislocated by the conflict. The economic, financial, and operational challenges of carrying out those activities are particularly difficult amid the democratization and the institution of the rule of law that must take place simultaneously and that impose a variety of constraints on reconstruction.

The book recognizes that one of the challenges of reconstruction is the difficulty of reactivating investment in the presence of uncertainty about property rights. Such uncertainty is always present in countries coming out of war where governments may lack legitimacy to solve long-standing conflicts with regard to property rights, or to establish property rights going forward, since investors will fear that these may change as a legitimate government takes over. At the same time, establishing law and order is particularly difficult in these countries where “spoilers,” who benefited from illicit activities during the war, make every possible effort to restore unlawfulness.

Based on case studies and other relevant experiences, the author presents the basic premises, lessons, best practices and policy guidelines which she posits are necessary to design an effective strategy for post-conflict economic reconstruction. In her view, unless jobs are created and the political and security objectives are assured, rebuilding war-torn states will not succeed and peace will be ephemeral.

New York  
April 2008

## Foreword to

Leo M. Tilman,

### *Financial Darwinism*

by Edmund S. Phelps<sup>8</sup>

The great economic theorist at Chicago, Frank Knight, observing American business experience, took the unprecedented position in his 1921 classic *Risk, Uncertainty and Profit* that most business decisions, especially strategic ones, are to varying degree steps into the unknown. Each of the possible outcomes of a business venture can be considered to have some probability of occurring, but those probabilities are not known to the players. Thus was born the concept of *Knightian uncertainty*. The great theorist at Cambridge and Knight's contemporary, John Maynard Keynes, produced major ideas on the consequences of this uncertainty in his 1921 book *Essay on Probability* and his 1936 book *The General Theory*.

Knightian uncertainty does not stem from some failure to study on the part of decision makers. Rather, it results from the unknowability of some of the conditions, present and future, on which the consequences of the decisions depend. If gamblers keep betting heads or tails, the evolving holdings may be knowable in a probabilistic sort of way. In the world of Knight and Keynes, though, the economic future is, in large part, not even probabilistic—it is to an important degree indeterminate. And if the probabilities governing the future cannot be known to a participant, they cannot be known to an outside observer or theorist, either. The driver in Keynes's "general theory" is entrepreneurs' intuition about the profitability of investments they contemplate; with their limited understanding, his entrepreneurs can have little idea what the correct expectation of profitability would be.

The heightened uncertainty and indeterminacy in economic life that Knight and Keynes captured came with the rise of the modern economy in the last decades of the nineteenth century. The arrival of finance capitalism, with its restless experimentalism, created *economies of dynamism*—economies with a propensity to innovate in ways that prove viable. It is this new dynamism that radically increased the unknowability that the actors in these economies had to confront. Dynamism—and the accompanying uncertainty and indeterminacy were virtually unheard of in the so-called *traditional economies* of the eighteenth

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century, in those economies, uncertainties seldom intruded except in the case of exogenous forces—the occasional scientific discovery, a natural disaster, and so forth. In contrast, in the modern economies that followed, new commercial ideas—thus elements of unknowability and uncertainty were generated by the operation of economies themselves. From time to time some businessperson, observing current practice first hand, would hit upon an original idea for a better way to do things. First in Britain, then on a wider scale and with greater force in Germany, and later the United States, finance capitalism generated a torrent of endogenous innovations from the 1860s onward for decades—a torrent that in the United States stretched through the 1930s and has had significant recurrences since.

This economic dynamism, though not measured directly, is manifest in several ways. It injects new kinds of activity into business life: employment in the financing, development, and marketing of new commercial products for launch into the marketplace and a cadre of managers deciding what to produce and how to produce it. It appears to lift job satisfaction and employee engagement. It increases turnover in the ranks of the economy's largest firms, as some new firms grow large and displace old firms. Last but not least, it lifts productivity onto a higher (whether or not a faster growing) path. It must be emphasized that rapid growth for a time is not evidence of much or any dynamism; and slow growth for a time is not evidence of a lack of it: Dynamism and growth are not synonymous.

The importance of dynamism in understanding and appreciating the standout economies—going back more than a century—is no secret among economists and business historians. It has been present for years in the pages of Friedrich Hayek, Alfred Chandler, Richard Nelson and Sidney Winter, Roman Frydman and Andrzej Rapaczynsky, Amar Bhide, Virginia Postrel, and some work of mine. Yet the general public has been led to believe the myth that high productivity, wages, and wealth are driven by the great technological advances of unworldly scientists operating outside the nation's economy: Columbus, Magellan, Watt, Volta, Faraday, Marconi, von Neumann, Berners-Lee, and the rest. It has to be added that large numbers of economists find it inconvenient to recognize originality and novelty in their formal economic models. Empirically, however, we do not find that productivity growth arrives in great waves, each linked to a scientific breakthrough. Furthermore, looking across countries, we do not see the patterns that the popular myth would predict: There are wide gaps in productivity levels and in some of the other manifestations of dynamism. It is clear that, in many countries though not all, something big is going on besides science—namely, ideas for new commercial products and new ways to produce.

Historically, capitalism—despite its many imperfections and episodic malfunctions—has proved the premiere economic system for dynamism. Capitalism is all about commercial innovation – the birth of the idea, the

development and marketing, and the adoption. Once key freedoms, supporting institutions and favorable attitudes have evolved, some participants step forward with entrepreneurial proposals, others step into roles as lenders or investors to finance some of these projects, still others, as managers or consumers, evaluate and sometimes make pioneering adoptions of the new products.

Of course, the uncertainty and the learning costs entailed by economic dynamism make business life treacherous, though exciting and challenging. There are hazards in acting without allowance for one's limited understanding. Unfortunately, it has become the style in business decision making to pretend that the economy and the financial markets are well understood and that the pertinent numerical parameters of financial and economic models, including the relevant probabilities, are fully known (or close enough to it). The misadventures of recent times—the monetary policy blunders, regulatory mistakes, astonishing financial losses, and worldwide systemic financial crises—are dramatic evidence to the contrary.

The recent problems in the banking sector in the United States are indicative of some of the failures. While many believed for some time that subprime lending and securitization would enable more people to own homes, decision makers had no foundation on which to estimate either the valuations or the risks of the novel assets acquired. Mistakenly, many thought that portfolio diversification could eliminate Knightian uncertainty as well as other risks. Furthermore, models did not allow for macroeconomic swings and for the unknown numbers of new financial companies that might enter the business. The irony here was that the financial sector, in the practices it introduced to capture what it thought were opportunities for a pure profit, ended up creating new and colossal uncertainties for itself and the global economy.

Capitalism has thus been disgraced precisely in the area of its greatest competence. The relatively capitalist economies, notwithstanding the considerable dynamism that classic capitalism showed in its glorious past—the knack for efficient and profitable innovation—have betrayed a lack of awareness and sophistication about what is required for making successful decisions of an innovative nature. Yet we can hope to find in the faults of standard practice and governance some ways to reorient the financial sector toward business development and commercial innovation – with resulting dividends in increased dynamism in the economy. As I have argued for some time, an economically advanced country is not doing justice to the potentialities of the population for self-actualization and self-discovery if it does not examine institutions, attitudes and beliefs for ways to shore up its dynamism.

This original and provocative book by Leo Tilman therefore comes in our hour of need. It starts off by making sense of the tectonic shifts that occurred in finance over the past quarter century. It then proceeds to offer a decision-making framework for operating in the new financial world. Tilman argues that the mechanism of how economic value is created (and destroyed) in finance is central to understanding modern financial institutions and capital markets. Equally intriguingly, he proposes that it is the dynamism of financial institutions' risk-taking and business decisions that both distinguishes the modern financial world from prior financial regimes and serves as the main determinant of their success going forward. He calls this evolutionary thesis *Dynamic Finance*.

This thesis contrasts the brave new world of finance with the old regime of the post-WW II economy. In the past, Tilman argues, financial institutions used to fulfill their chartered roles in ways that, from the risk-management perspective, were very traditional and static. Measures of economic success based on accounting earnings and standard financial disclosures may have been the adequate lens through which to view reality in the good-old days of the banker George Bailey in Frank Capra's *It's a Wonderful Life*, to borrow the author's apt image. However, they are not applicable to the new dynamic state of affairs and thus often lead to confusion and inoptimal decisions. This depiction reminded me of the "traditional economy"—the economy of routine captured by the neoclassical models of economic equilibrium: they excluded change for which there was no prior information and departures for which there was no known knowledge to go by.

The modern economy opens the door for individuals to exercise their creativity by venturing to do something innovative—financing, developing, and marketing of new products and methods. Models of such an economy must recognize the nonroutine ways in which market participants make decisions or deploy resources. These models must also be general enough to be compatible with the myriad of ways in which market participants might revise their views of the future and act on them. In applying a similar line of thinking to financial institutions, Tilman develops a concept of risk-based economic performance that underlies the book's evolutionary thesis and leads to a decision-making framework that he calls *Financial Darwinism*. This book introduces a new intellectual paradigm that can be used to guide strategic and investment decisions. Importantly, however, by recognizing the essence of dynamism, it does not impose the author's views or advocate any particular paths to success, leaving it to financial executives to use their creativity, proprietary knowledge, and ingenuity when ultimately deciding what is best for their firms.

This brings me back to the interaction of uncertainty and dynamism. Given that nonroutine business decisions are steps into the unknown, I have always found it odd that financial executives seemed to think so little about



Knightian uncertainty. Tilman does not view this lack of concern as surprising at all, attributing it to old mental paradigms and static business models that obscured the roles of risk taking and uncertainty during the old financial regime. He argues that, as a result of the tectonic financial shift, active risk taking has become a much greater contributor to economic value creation, and, therefore, the role of risk in the lives of financial institutions must be made explicit. Tilman points out that the greater complexity of today's financial world stems from more dynamic economies, more dynamic financial institutions, greater connectivity of the capital markets, and a set of other powerful secular forces. Therefore, the nature of executives' strategic vision and their understanding of uncertainty must change accordingly.

The book's author and I first met at the World Economic Forum in Davos and have since continued our discussion of economic dynamism and the attendant uncertainties at Columbia's Center on Capitalism and Society. We are in agreement that dynamism, though messy and the cause of some volatility and irremediable inequalities, is important not only for its effects on productivity and employment—which serve in turn to increase the inclusion of people into the commercial economy—but also for itself. An economy of dynamism meets some of our very basic needs: to exercise our imaginations, to enjoy the mental stimulus of change, to have an endless series of new problems to solve, to expand our capabilities, to feel the thrill of discovery, and to sense our personal growth. From the start, Tilman and I were intrigued by the many parallels between economic dynamism and the dynamism in finance. He sees the latter as essential for modern financial institutions' survival and success.

I believe this thought-provoking book, in interpreting major financial trends, in pointing to the need for financial dynamism, and in providing the relevant arsenal of ideas and decision-making tools to that end, will be of great interest to executives, investors, regulators, academics, and students of economics and finance. Let us hope that the banking industry will be given the opportunity to reform itself – to acquire the strategic vision and management practice that will create real and lasting economic value, thus benefiting shareowners, employees, indeed, the whole society.

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