Assessing Progress in Research on Industrial Evolution and Economic Growth: A Review Article

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ABSTRACT. This article reviews a new three-volume collection of previously published articles on how industry structures change, edited by David Audretsch and Steven Klepper. Central themes in the chosen contributions are summarized, and the disproportionate emphasis on relatively new rather than clearly seminal articles is noted. A more detailed analysis is undertaken on how the chosen articles illuminate the determinants of incumbent firms' survival and growth advantages, i.e., scale economies, first-mover advantages, organizational competence, and growth-affecting strategies such as preemptive innovation and mergers.

In a new three-volume set, David Audretsch and Steven Klepper (2000) have collected 55 previously published contributions (plus in a few instances discussants' comments) to offer a comprehensive view of how industry structures evolve and how innovation simultaneously shapes structure and is affected by it. The editors' perspective is stated in an introductory survey essay, replicated in all three volumes:

> Traditional economics has been remarkably silent on pressing questions such as [how economic growth is sustained or falters in diverse contexts and time periods]. The most obvious explanation for this reticence is the inherently static nature of the discipline. The intellectual heritage of economics is rooted in equilibrium thinking, yet the common denominator among the questions posed above is change. The growing gap between the methods of the economics discipline and their ability to explain, understand, and predict the most compelling economic events of our time is alarming.

If this ringing indictment were literally true, there would not be enough material to fill out 1467 printed pages, and this review article could end here. But of course, there has been much work that might be called non-traditional on the questions addressed by the editors; hence the collection's raison d'être.

That attention to change should be non-traditional is paradoxical. Economists derive their emphasis on equilibrium concepts in part from the example of physics, yet physics is all about differential equations, not one-shot equation systems.

Nevertheless, the editors' concern is justified: the microeconomics of change attracts attention much less than proportional to its paramount importance.

The essays in these volumes are organized into 14 parts. Rather than regurgitating the section headings, I offer my own summary of the main themes. The collection begins with essays on how innovation, firm entry, and firm exit patterns vary over product life cycles. There follow numerous papers on how much entry and exit product markets experience over time; what the characteristics are of the entrants and how they differ from those of long-run survivors; how firm growth rates and the variance of growth rates relate to firm size, age, and other characteristics; how firms improve their performance through learning-by-doing and how productivity and industry structure are thereupon influenced; and the role of new firms as compared to incumbents in introducing technological innovations. Linking many of the contributions is the logic of stochastic growth theories, stemming more or less directly from Gibrat's (1931) proportionate growth model, to explain the relationships between turbulence and the size distribution of firms within a market. Also treated are firm strategies that either sustain incumbent firms' market dominance or undermine it when technological innovations occur; how

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inter-firm and inter-industry interactions affect the industrial structure of urban and regional areas; how innovation and learning at the firm level are related to international competitiveness; and nations' policies for encouraging industrial innovation.

To simplify the formidable task of reviewing such a massive collection, I pursue three main questions: (1) the editors' choice of included contribution vintages; (2) the balance between theory and empirical work; and (3), how well the fundamental determinants of incumbent firm survival and growth are pinned down.

Among the 55 contributions chosen for inclusion, 34 were first published during the 1990s, 17 during the 1980s, one in the 1970s, two in the 1960s, and one (by Herbert Simon and Charles Bonini) from the 1950s. This, plus the editors' statement that their goal was to "bring together the seminal contributions of this emerging new literature", would seem to suggest that progress toward understanding the microeconomics of innovation, growth, and industry structure has occurred mainly during the past two decades.

Were economists really so ignorant before 1980 that 91 percent of our relevant understanding has been accumulated since then? For an old fogey like myself who labored in the relevant vineyards during three earlier decades, it appeared that the choice of vintages vastly underestimated the knowledge base upon which work during the past two decades built. As a crude test, I counted for several categories on which a fairly clear mapping existed the number of relevant scholarly articles and books cited in the second edition of my textbook (Scherer, 1980), which surveys fairly comprehensively the state of the art as it existed in 1978. On the characteristics of new entrants, I found 25 pre-1980 contributions; on stochastic variables influencing the size distribution of firms, 23 contributions; determinants of the firm growth other than mergers, 6 contributions; learning-by-doing, 11 contributions; market structure turbulence, 9 contributions; and on the theory of innovation-specific strategies for maintaining a firm's market dominance, 12 contributions.

Yes, but perhaps those early contributions were not seminal – a criterion expressly applied in the editors' choice process. Here too I take exception. How can the seminal thinking about product life cycles be surveyed without including Raymond Vernon's 1966 article on the product life cycle and the emergence of multinational enterprises? (The chosen articles largely ignore the international dimension of company structure.) If I were implementing a seminality criterion, I would also consider Chapter III of the prodigiously influential book by Adolf Berle and Gardiner Means (1932) and Chapter IV of U.S. Temporary National Economic Study monograph no. 22 (1941). On the importance of firm diversity to innovation, few authors, old or new, have offered more stimulating insights than Burton Klein's 1977 book. If the editors wanted to identify contributions that provoked our research on the relationships between market structure and innovation, how could they fail to include Chapter 8 of Schumpeter's (1942) classic, and Chapter 4 of Friedrich Hayek's Nobel prize-winning 1944 book? (Schumpeter is, as Robert Solow once observed, a patron saint in this field, so the misspelling of his given name in the editors' introduction is more than a venial sin.) And, to go back to the Urtext, who since has been more prescient in characterizing the emergence of specialized industrial innovation laboratories than Adam Smith in 1776:

> "[E]very body must be sensible how much labor is facilitated and abridged by the application of proper machinery. Men are much more likely to discover easier and readier methods of attaining any object, when the whole attention of their minds is directed towards that single object, than when it is dissipated among a great variety of things. All the improvements in machinery, however, have by no means been the inventions of those who had occasion to use the machines. Many improvements have been made by the ingenuity of the makers of the machines, when to make them became the business of a peculiar trade; and some by that of those who are called philosophers or men of speculation, whose trade it is not to do any thing but to observe every thing; and who, upon that account, are often capable of combining together the powers of the most distant and dissimilar objects. In the progress of society, philosophy or speculation becomes, like every other employment, the principal or sole occupation of a particular class of citizens".

I could (and will) multiply examples, but my point remains. The editors have imposed criteria hard to reconcile with seminality in their choice of contributions to illuminate the progress of our thought.

Characterizing the included contributions

between theoretical and empirical works is more difficult. By my subjective reckoning, 12 of the articles present relatively unadulterated theory, 9 offer theory supplemented by an empirical test, 31 are mainly of an empirical character, and 3 derive policy lessons from the chosen authors' accumulated experience and observation. This strong emphasis on real-world evidence, I showed in Scherer (1996, pp. 11–12), is more like the research pattern one finds within distinguished physics faculties than in many leading economics journals. I am convinced that it is the right path to genuine progress.

I would go farther and assert that the purely theoretical articles chosen for inclusion added rather little to our understanding of the phenomena addressed. There are two main reasons: the lack of a serious effort to root theoretical constructs in observed real-world phenomena, and the fact that with an often minor twiddling of assumptions, the models yield quite different conclusions. No better evidence of these two points can be mustered than the editors' choice of five separate theoretical papers on the same narrow question: whether firms that dominate an industry have systematic incentives to innovate more rapidly than non-dominant firms and thereby to preclude entry by innovative outsiders.

The first paper, emphasizing a winner-take-all model at odds with virtually all the empirical evidence revealing that many firms participate in innovations' life cycles, concludes that the dominant incumbent does tend to preempt innovation by outsiders. The second paper, introducing uncertainty, arrives at the opposite inference. The third paper argues that the assumed timing of rivals' moves determines the outcome. The fourth agrees, but demonstrates that the assumed power of first-mover advantages is also critical. The fifth paper shows that the outcome can be reversed again if transaction costs are sufficiently low to let the incumbent buy off challengers.

One emerges from reading these contributions wondering, did the editors choose them cynically to illustrate how fragile the contributions of relevant theory are? If so, it would have been helpful for the editors to provide at least some signal of their exasperation over the whole business – e.g., to place a Smiley face in the margin of their summary. I might add, reinforcing a point made previously, that the first paper in this sequence retraced, apparently unknowingly, theoretical ground already plowed 13 to 15 years earlier by myself (1967) and Baldwin and Childs (1969). Those first-moving articles had variable market shares (with winner-take-all as a special case), depending upon the timing of moves, first-mover advantages, the use of parallel R&D paths to hedge against uncertainty, incumbency asymmetries, and (with Baldwin and Childs) inter-firm learning. They did not consider buying out small innovators, a strategy used by General Motors 80 to 90 years ago and more recently e.g. by Cisco and Microsoft.

The selected empirical contributions have a stronger claim to novelty, at least within recognizable limits. The last two decades have been different from preceding decades in two important respects: computers have become much more powerful and accessible, and partly because of that, the data available to researchers are vastly richer than those with which we had to work during the 1960s and 1970s. Long gone, except perhaps for the voters of Florida, are the days when we had to struggle with balky punch cards to compile and analyze data. New data sets such as the Census Bureau's Longitudinal Research Database have provided the foundation for sharpening considerably our understanding of entry and firm mortality rates, the characteristics of new entrants and exiting firms, and how turbulence affects industry productivity. I say "sharpen" because, from my review of the literature as it existed in the late 1970s, we already had fairly good insights into how these phenomena unfolded. These have been modified to some extent by the analyses of newer and richer data. It would have been helpful had the editors tried in their summary to pinpoint how our perceptions changed as a result of empirical research during the past two decades and to extract from the maze of results an integrated coherent view, if indeed one can exist, of how change occurs in a progressive economy. But no such synthetic effort was attempted.

The empirical research reinforces earlier impressions that in any given year there are hordes of new entrants, typically small, into some (but not all) product and service markets; that the statistically noisy growth and survival experiences of

new entrants differ significantly from those of incumbents; and that exiting firms, which are also abundant, have characteristics discernibly different from those who survive. Incumbents tend to have both slightly lower average growth and substantially lower growth rate variances, implying less mortality risk, than newcomers do. The most successful newcomers grow spectacularly and eventually become statistically indistinguishable from incumbents.

A key question posed by this picture is: what are the sources of incumbents' survival and growth advantages?

My reading of the papers and omitted materials identifies four main contenders: economies of scale, first-mover advantages, advantages embodied in the human beings who make up a business organization, and the implementation of growth-affecting strategies such as mergers and sell-offs. I address each in turn.

Several empirical papers include explanatory variables serving as proxies for the importance of conventional scale economies. None of the included papers addresses in a systematic way the sources of scale economies or the evidence on how large plants or multi-plant firms must be relative to the size of their markets before minimum long-run unit costs are achieved. Several papers deal either theoretically or empirically with dynamic scale economies, as reflected inter alia in the phenomenon of learning-by-doing. Here too, however, I found the choice of papers puzzling. A 1981 selection by Michael Spence is characterized by the editors as "seminal." It is indeed of high quality. But there were many good earlier papers on the subject, including contributions by such eminent economists as Armen Alchian (1959), Kenneth Arrow (1962), and Jack Hirshleifer (1962). Spence's paper illustrates the variations in industry structure that can result from learning-by-doing under various conditions, including differential head starts, but by modeling the pricing behavior of firms using symmetric Cournot-Nash assumptions, he overlooks the more interesting alternative: Stackelberg price leadership by the first mover, which is more apt to sustain market dominance. A leading example is Intel's experience over numerous microprocessor generations from 1982 into the late 1990s. Seminal theoretical contributions embodying Stackelberg assumptions and diverse case studies were ignored by the editors.

Learning curves are also a prominent component of an included article by Nobel laureate Robert Lucas. The contribution hardly reveals Lucas at his best. The paper's empirical section suffers from the author's apparent unawareness that superior high-quality data were available on U.S. aircraft production learning experiences during World War II. The conclusion applies Lucas' theory to imply homogeneity of the East Asian national growth miracles – a leap that could hardly depart farther from reality. The technological achievements of Japan, Taiwan, and Korea were dramatically different from those of other Asian tigers such as Hong Kong, Malaysia, Singapore, and Thailand. Readily available statistics reveal that the latter generate far fewer U.S. invention patents, either absolutely or per capita, than the first three. A dissertation by Ishtiaq Mahmood (1999) shows that the patentable inventions of the first three were largely made by indigenous enterprises, whereas most of the inventions in the latter nations came from local branches of multinational enterprises (MNEs). How are we to understand the subtleties of economic growth without clear recognition (provided to some extent in perceptive policy essays by Joe Stiglitz and Richard Nelson) that nations may pursue widely differing industrial innovation strategies? And in that understanding, the role of do-it-yourself as compared to MNE-dependent development strategies must play a key role.

That leading incumbent firms enjoy appreciable first-mover advantages is brought out by several of the collection's papers. Very little is said, however, about the nature of those advantages. There are substantial and highly informative literatures on the image advantages a successful product innovator tends to enjoy, on the first-mover advantages that real-world R&D decision-makers consider important, and on the (typically weak) protection from imitation afforded by patents. The collection would have benefited from sampling or at least summarizing the relevant articles.

Many of the included contributions assert or

assume that individual firm growth experiences differ in large part because the human capital possessed by entering and incumbent firms varies enormously in quality and in its suitability to particular market conditions. Could something more concrete have been articulated about this variable? There is now a substantial cottage industry, located mainly in business schools, that attempts to discern the bases of companies' "core competencies" or "sustainable competitive advantage". From it we know that such advantages exist, embodied partly in individuals but mainly in the chemistry of organizational group interactions, and that they are important. The extant literature is good at identifying organizational competence ex-post from evidence of superior performance, but weak at predicting ex-ante which organizations will perform well or even how long well-performing organizations can sustain their achievements. Nevertheless, it would have been useful to have an essay laying out what is known about this source of incumbent firm advantage and the intellectual obstacles blocking a more complete understanding.

Finally, it is clear that firm size distributions and the survival and growth of particular firms are influenced by the structural strategies firms pursue, not only in attempting to impede innovative entry, as stressed in one part of the collection, but also by mergers and sell-offs (which may be misinterpreted in naive analyses as exits) and other organizational control changes. The collection does include a paper by Frank Lichtenberg on mergers. It is an excellent paper, imaginatively analyzing data from the Census Bureau's Longitudinal Research Database. But it is far from a comprehensive treatment of a phenomenon responsible for much structural change in the contemporary economy and (since investing in a startup company may be encouraged by the expectation of later sell-out opportunities) affecting incentives for innovation. Historical perspective and balance could have been enhanced by adding Shaw Livermore's classic 1935 article on the aftermath of the huge merger wave a century ago. It would have been useful too if the editors had noted in their brief summary that Lichtenberg returned to his data set to extract more nuanced inferences, e.g., about the relative success of mergers joining previously independent entities, as distinguished from acquisitions by one enterprise of another organization's discarded (i.e., sold off) segments.

In summary, what these three volumes offer is an idiosyncratic collection of recent articles on an extremely important subject. The central message – that understanding change at the firm and industry level is vital to knowing how economies grow – is correct. Economists need to absorb that message and could become better at their craft by reading many of the reprinted contributions. However, by failing to provide a baseline on what was known two or more decades ago, the editors have offered no insight into the extent to which our knowledge about the microeconomics of growth has grown. We have in fact learned a lot. But there is more to be done. One wishes also that the editors had provided a more comprehensive analytic synthesis, among other things staking out a suggested agenda for future research.

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