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IIASA Working Paper

September 1994

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Preface

The research project on *Systems Analysis of Technological and Economic Dynamics* at IIASA is concerned with modeling technological and organisational change; the broader economic developments that are associated with technological change, both as cause and effect; the processes by which economic agents – first of all, business firms – acquire and develop the capabilities to generate, imitate and adopt technological and organisational innovations; and the aggregate dynamics – at the levels of single industries and whole economies – engendered by the interactions among agents which are heterogeneous in their innovative abilities, behavioural rules and expectations. The central purpose is to develop stronger theory and better modeling techniques. However, the basic philosophy is that such theoretical and modeling work is most fruitful when attention is paid to the known empirical details of the phenomena the work aims to address: therefore, a considerable effort is put into a better understanding of the 'stylized facts' concerning corporate organisation routines and strategy; industrial evolution and the 'demography' of firms; patterns of macroeconomic growth and trade.

From a modeling perspective, over the last decade considerable progress has been made on various techniques of dynamic modeling. Some of this work has employed ordinary differential and difference equations, and some of it stochastic equations. A number of efforts have taken advantage of the growing power of simulation techniques. Others have employed more traditional mathematics. As a result of this theoretical work, the toolkit for modeling technological and economic dynamics is significantly richer than it was a decade ago.

During the same period, there have been major advances in the empirical understanding. There are now many more detailed technological histories available. Much more is known about the similarities and differences of technical advance in different fields and industries and there is some understanding of the key variables that lie behind those differences. A number of studies have provided rich information about how industry structure co-evolves with technology. In addition to empirical work at the technology or sector level, the last decade has also seen a great deal of empirical research on productivity growth and measured technical advance at the level of whole economies. A considerable body of empirical research now exists on the facts that seem associated with different rates of productivity growth across the range of nations, with the dynamics of convergence and divergence in the levels and rates of growth of income, with the diverse national institutional arrangements in which technological change is embedded.

As a result of this recent empirical work, the questions that successful theory and useful modeling techniques ought to address now are much more clearly defined. The theoretical work has often been undertaken in appreciation of certain stylized facts that needed to be explained. The list of these 'facts' is indeed very long, ranging from the microeconomic evidence concerning for example dynamic increasing returns in learning activities or the persistence of particular sets of problem-solving routines within business firms; the industry-level evidence on entry, exit and size-distributions – approximately log-normal – all the way to the evidence regarding the time-series properties of major economic aggregates. However, the connection between the theoretical work and the empirical phenomena has so far not been very close. The philosophy of this project is that the chances of developing powerful new theory and useful new analytical techniques can be greatly enhanced by performing the work in an environment where scholars who understand the empirical phenomena provide questions and challenges for the theorists and their work.

In particular, the project is meant to pursue an 'evolutionary' interpretation of technological and economic dynamics modeling, first, the processes by which individual agents and organisations learn, search, adapt; second, the economic analogues of 'natural selection' by which interactive environments – often markets – winnow out a population whose members have different attributes and behavioural traits; and, third, the collective emergence of statistical patterns, regularities and higher-level structures as the aggregate outcomes of the two former processes.

Together with a group of researchers located permanently at IIASA, the project coordinates multiple research efforts undertaken in several institutions around the world, organises workshops and provides a venue of scientific discussion among scholars working on evolutionary modeling, computer simulation and non-linear dynamical systems.

The research focuses upon the following three major areas:

1. Learning Processes and Organisational Competence.
2. Technological and Industrial Dynamics
3. Innovation, Competition and Macrodynamics

An Agenda for Formal Growth Theory*

I. Introduction

From at least the late 1950s, most formal models of economic growth have recognized technical advance as the key driving force, and thus have been consistent with a central conclusion of the empirical research on the sources of growth. However most of the earlier formal models were mute or incoherent regarding the sources of technical advance. In recent years a number of new formal models have been developed which make technological advance endogenous, involving centrally the profit-seeking investments of business firms (see e.g. Aghion and Howitt, 1990, Grossman and Helpman, 1989 and Romer, 1990). These models capture in stylized form a number of the understandings about technical advance that have been well documented by empirical scholars. (For a good survey see Freeman, 1982)

To build in features that make R and D profitable for firms, these models depart from the earlier ones in one or both of the following. First, firms are able to keep proprietary at least a portion of the value of the increased productivity or better product performance won through their R and D. Second, to square with the recognition that technology is in some degree proprietary, and also that support of R and D is feasible only if price exceeds production cost by some margin, markets are assumed to be imperfectly, not perfectly, competitive. (For a good statement see Grossman and Helpman, 1994)

The endogenizing of technical advance in this way has been complemented by the building in or deduction of other phenomena. Thus some of the models treat technical advance as a process of "creative

* A number of people have commented helpfully on earlier versions of this essay. In view of the controversial nature of much of the argument, as one would expect some of my respondents were more in accord with my line than were others, but all commented constructively. I particularly want to thank the following, without implicating them in any way: Moses Abramovitz, Giovanni Dosi, Zvi Griliches, John Kendrick, Theo van de Klundert, Franco Malerba, Paul Romer, and Robert Solow.

destruction" in which a new technology obsoletes older ones (See Grossman and Helpman, 1989). In many of the models there are "externalities" from investments in R and D, as in Romer, 1990, or from other activities, for example, education (See Lucas, 1988). Up front R and D investments and, in some models, other factors such as differentiation of intermediate products that enable varied production needs to be met better as an economy gets larger, generate economies of scale. In many of these models the rate of investment in new plant and equipment affects the steady state growth rate, because of scale economies or externalities or both, whereas in most of the older generation of models the steady state growth rate was independent of the investment rate.

The characterization above does not do justice to the elegance of some of the new growth models, nor does it lay out the variety. (For more extended and systematic reviews see Romer, 1991 and 1994, and Verspagen, 1992). However, it suffices to bring out two points. First, these formal models are different than most of the earlier ones, and in ways that make them more "realistic" in the sense of capturing, at least in stylized form, features of growth that many economists studying the topic empirically long have known to be important. Incorporation of these features certainly makes it easier for formal growth theorizing to engage effectively with the empirical work of economists trying to come to grips with the puzzling features of experienced economic growth.

However, second, the brief review also suffices to highlight that the phenomena incorporated in the new formal models, and neglected in many of the old ones, scarcely represent novel new insights or ideas. The basic notions that "technical change is largely endogenous", "technology is to at least some extent proprietary, and market structures supporting technical advance are not perfectly competitive", "new technology often obsoletes old technology", "growth fueled by technical advance involves externalities and economies of scale", and "the investment rate may matter in long run", scarcely smack of novelty. All have been part of the body of understanding of those studying economic growth and technical advance for a long time. (Freeman, 1982, already cited, is a good reference). Indeed, as I shall show in the next section, Abramovitz put forth most of these propositions in his review article on the economics of growth, written over forty years ago, in 1952.

If, in fact, much of what has been included in the new formal growth models long has been understood by scholars in the field, one can ask what is added by formalization of this understanding. I will argue several things are. However to get to them I need to discuss more generally the nature of theorizing in economics.

Sidney Winter and I have argued (1982) that, perhaps because the subject matter and the operative mechanisms of economics are so complex, theorizing in economics tends to proceed at at least two levels of formality, not one. We have called these levels appreciative theory and formal theory.

Appreciative theorizing tends to be close to empirical work and provides both guidance and interpretation. Mostly it is expressed verbally and is the analyst's articulation of what he or she thinks really is going on. However, appreciative theory is very much an abstract body of reasoning. Certain variables and relationships are treated as important, and others are ignored. There generally is explicit causal argument. On the other hand, appreciative theorizing tends to stay quite close to the empirical substance.

In contrast, formal theorizing almost always proceeds at some intellectual distance from what is known empirically, and where it does appeal to data for support, the appeal generally is to "stylized facts", or reasonably good "statistical fits". If the hallmark of appreciative theory is storytelling that is close to the empirical details, the hallmark of formal theorizing is an abstract structure set up to enable one to explore, find, and check proposed logical connections. Good formal theorizing usually will contain fewer strictly logical gaps than appreciative theorizing and will be mostly consistent. Also, the logical inferences will tend to reach farther than those of appreciative theorizing.

We have proposed that, when the intellectual enterprise in economics is going well, empirical research, appreciative theorizing, and formal theorizing work together or rather, empirical work and appreciative theorizing work together, and appreciative and formal theorizing work together. Empirical findings or facts seldom influence formal theorizing directly. Rather, in the first instance they influence appreciative theorizing, that is what the empirical researchers want to highlight and draw from their work. In turn, appreciative theorizing provides challenges to formal theory to encompass its understandings in stylized form. The attempt to do so may identify gaps or inconsistencies in the verbal stories, and suggest new mechanisms and connections to explore. In turn the empirical and appreciative theoretical research enterprise may be reoriented.

Under this account, formal theory should not be understood as exhausting the body of theoretical understanding possessed by economists working in a field. Rather, it needs to be seen as touching on only a portion of that broader body, and as addressing what it does in highly stylized form. On the other hand, formal theory is the most rigorously worked out part of theory.

One very important role of formal theory obviously is to discipline and sharpen appreciative theory. In so doing it may or may not break new ground. But whether it does or not, where appreciative theory is not so disciplined, the body of understanding possessed by economists is of uncertain rigor.

Another important role of formal theorizing in contemporary economics is, somehat paradoxically, to get the understandings it contains into the view of the profession at large. For whatever reasons, graduate training in economics tends to emphasize formal models. When graduate students take a first course in a field, the models are mostly what they are taught. Formal models in a specialized field, if they are analytically interesting, also have a much easier time getting into the more general and widely read economic journals, particularly if they are accompanied by some econometrics, than looser form appreciative theoretic discussions. The latter, therefore, tend to be read and known principally by the specialists in a field.

It is for these reasons that the new formal growth models are having a positive effect on research on economic growth. The scholars who have long been working in the field are, in many cases, being stimulated to think about their appreciative theories in different ways, even though most of them would grumble that they already knew what the models teach. Perhaps more important, the new formal growth theories are spreading the word around the profession, and attracting graduate students into the field.

The perspective I have laid out thus enables me to say where and how I see the new formal growth models as contributing significantly, even if the ideas they contain may not be very new. That perspective also enables me to explain the title of this essay. I will propose that, while it has picked up a number of important strands in appreciative growth theorizing, the new formal modeling enterprise has not recognized several other important strands.

I shall develop my case as follows. In Section II I present an analytic history of "theorizing" about economic growth from the early 1950s to the early 1980s, focusing on two matters. One is the nature of the interaction between appreciative and formal theorizing during this period. The other is the growing sense of malaise in the intellectual enterprise that set in after the 1970s, as conventional theoretical ideas came to be perceived as having little grip on the puzzles associated with growth slowdown. Then, in Section III, I discuss various pieces of new appreciative theory that have come into view since the late 1970s. In particular I describe recent theorizing about technology, firms, and institutions. In the concluding section I pull together my argument that these define an agenda for formal growth theory.

II. Post-war Theorizing About Economic Growth

Economic growth of course was a central interest of Adam Smith, and many of the classical economists of the 19th century. However, during the first half of the 20th century the topic dropped out of vogue, as microeconomic analysis increasingly came under the sway of partial and general equilibrium theory, and, with the Great Depression, macroeconomic analysis became obsessed with unemployment. After World War II a number of economists again became interested in economic growth. The new surge of research on economic growth was not kindled by any arresting new theory in economics. Rather, a principal motivation for the new research was the availability of new economic statistics, particularly the national income and product statistics, which Simon Kuznets pioneered and which for the first time enabled economists to measure growth at a national level.

A. The State of Growth Theory, as of 1952

In 1952 A Survey of Contemporary Economics was published, which attempted to assess the state of the discipline then. It contained an article on the economics of growth by Moses Abramovitz, who was very much involved in this new research. For my purposes there are two important features of that article. First, Abramovitz begins with a statement about the absence at that time of any coherent modern growth theory to guide empirical research. "Unlike most of the topics treated in the Survey, the problem with economic growth lacks any organized and genuinely known body of doctrine whose recent development might furnish the subject of this essay." The reader will of course note that Abramovitz was writing a few years before the publication of the Solow and Swan pieces (1956) which are reputed to have established modern growth theory, and be tempted to take his statement as an indication that those articles filled an intellectual vacuum.

But the other noteworthy aspect of Abramovitz' essay is the up-to-date character, by contemporary standards, of the issues and relationships that he discusses. It is as if most of what formal neoclassical growth theory later taught already was known. Thus there is a clear statement of the logic behind modern growth accounting, a logic which was being used in the empirical work with which he was involved. Abramovitz notes that economists long have professed a theory that the level of output is determined by the quantity of inputs (land, labor and capital) and factors that affect their productivity (the state of the arts, industrial and financial organization, the legal system, etc.). Therefore, at one level at least, economic growth can be understood as a function of changes in or improvements in these "immediate determinants of output." Abramovitz also proposes that analysis of growth simply at this level is not deep enough, and that a satisfactory theory of growth must come to grips with the forces behind changes in the immediate determinants.

His essay goes on to analyze the forces affecting the expansion of the traditional factors of production: land, labor, capital, and of their contribution to growth. Among other features of his discussion, Abramovitz refers to the view common in economics that the marginal productivity of capital will be high or low depending on the ratio of capital to other factors, and will diminish as capital grows relative to them--clearly this characteristic of the "old" neoclassical growth theory did not come as news. But he then goes on to argue that increases in economic efficiency as the scale of output grows may offset diminishing returns--a feature built into some of the "new" neoclassical growth theory.

Abramovitz states that, in his view at least, "technical improvement" must account for "a very large share, if not the bulk, of the increase in output." Thus while the empirical evidence that persuaded others of the economics community on this was not yet in, Abramovitz could not have been surprised by it. Abramovitz clearly sees technical advance as "endogenous", resulting largely from investments aimed to create and exploit it, and anticipates the concept of "knowledge capital" as follows. "And insofar as new applied knowledge results from the deliberate direction of revenues to its discovery and use, the stock of knowledge is increased by a process identical with that which produces increases in the stock of material equipment." Referring to Schumpeter (1950) he observes, "with the development of industrial research departments of corporations...almost all engineering work is undertaken only in conjunction with the deliberate entrepreneurial decision." Some of the proponents of the new growth theory have argued that, until recently, analysts of growth were hung up on a growth theory that assumed perfect competition, but Abramovitz clearly didn't have that hang up. Abramovitz also recognized that the investments that yield new proprietary technology also generate externalities, at least with time, "as experience is gained and knowledge of the new art becomes widespread."

Abramovitz highlights the interdependence of technical progress and the expansion of other factors as sources of growth. Vintage models would not have come as news to him. "The actual exploitation of new knowledge virtually always involves some gross investment (in material equipment)."

Abramovitz goes on to imbed his analyses of expansion of the traditional inputs to production, and of technical advance as the major factor augmenting their productivity, in a discussion of "enterprise" and "institutions". He already had focussed on modern corporations as key actors in technical progress, and in investing in material equipment. He then observes that "the role of enterprise has been slighted by traditional theory because of the theory's generally static character which leads easily to assumptions about perfect knowledge, and rational calculation of profit." He goes on to suggest that, if one finds Schumpeter's analysis persuasive, one is compelled to recognize that "the marginal productivity of capital depends on enterprise to such a degree" that to neglect it is to miss the whole point of capitalist economic development. Here Abramovitz is, in the view I will espouse, far ahead of developments in even the "new" neoclassical growth theory, at least as that work has developed to date.

He also is far out in front in his discussion of the broader cultural and institutional factors surrounding and supporting enterprise. Here he expresses the judgement that the broader context is key, and also his concerns that "The general conclusion suggested by this survey of the factors controlling the vigor of enterprise is that a vast deal of emphasis must be placed on forces that, in the ordinary conception of the bounds of economics, would have to be classed as political, psychological, or sociological." Abramovitz thus flags the challenge for economists, and stresses that, to unravel the mysteries of economic growth, economists have got to get into these issues. Mostly of course we haven't. The focus of almost all research on economic growth since Abramovitz wrote has been on his "immediate" determinants.

I have dwelt at some length on Abramovitz' essay, pointing to its modern tone as well as to its richness. Abramovitz clearly is, and was, a remarkable scholar. But in his 1952 essay he does not present his theorizing about growth as particularly original. Indeed he writes as if he were recounting notions long held in economics, and held at the time he was writing by other scholars getting into empirical study of economic growth, for example, Simon Kuznets.

B. Empirical Research on Growth During the 1950s and 1960s

At the time Abramovitz wrote, a number of economists were hard at work doing empirical research on growth using the new National Income and Product accounts, and other new data. By the early 1950s the results of that research, probing the immediate determinants of growth, began to come in. Studies by Schmookler (1952), Schultz (1953), Fabricant (1954), Kendrick (1956) and Abramovitz himself (1956), all reported that the growth of output experienced in the United States had been significantly greater than reasonably could be attributed to input growth.

In these papers the contribution of total input growth was estimated by weighing the different inputs by their prices, a practice apparently considered so reasonable and obvious that few of the authors even bothered to rationalize it explicitly. (While not recognized by many at the time, an earlier paper by Tinbergen, 1942, had anticipated much of the methodology). The excess of output growth over input growth was attributed to a variety of factors. Technological advance, increasing returns to scale, investments in human capital, the allocation of resources from lower to higher productivity activities, all were recognized as parts of the story, but these authors clearly put heavy stress on the former.

It is interesting that Solow's 1957 piece, which most economists not immersed in research on economic growth regard as the seminal article calculating the "residual" and interpreting it as a measure of technological advance, was published after the studies noted above. The reason for the impact of Solow's piece, I would argue, is that his analysis was structured by a "formal" theory, whereas the theorizing in these earlier pieces was more "appreciative" and looser.

Edward Denison's research and writings based on growth accounting, which began to get published in the early 1960s, enormously increased our understanding of economic growth, at least at the level that Abramovitz had referred to as the "immediate determinants." Regarding growth in the United States, Denison's (1962) conclusions basically were consistent with those published earlier by the scholars cited above, and his contribution mainly involved an ingenious and painstaking attempt to break down the sources of total factor productivity growth into the various components mentioned above. Other economists followed along the same track, developing other kinds of disaggregation and exploring different measures of factor marginal productivity. (See e.g. Jorgenson and Griliches, 1967). Later in the 1960s Denison (1968) extended his framework to examination of growth in the European economies, with findings that were quite similar to what had been found about the U.S. (See also Domar, 1963).

However, this study also probed at the reasons why European worker productivity was significantly lower than American (in the early 1960s roughly half). The key finding here was as remarkable as the earlier finding that growth of total factor productivity accounted for the bulk of productivity growth. It was that differences in inputs per worker could account for only a small share of the differences between American and European productivity levels, and that apparently European nations were operating at significantly lower levels of "total factor productivity" than the Americans.

Denison's work came after the publication of Solow's theoretical and empirical essays on growth. However, there is little evidence that these articles influenced him much. Denison footnotes only the Solow empirical article,and the substance of the footnotes is about differences in statistical details.

While most of the new work on economic growth was focused at the macro or economy-wide level, some was oriented to the sectoral or industry level. Thus during the early 1960s economists came to learn that the significant inter-industry differences in rates of labor productivity growth, of which they long had been aware, largely was associated with differences in rates of growth of total factor productivity, again interpreted as largely reflecting differences in rates of technical advance. The new availability of industry level R and D data permitted Terleckyj (1960) to explore the connections. His work showed a reasonably strong relationship (by the standards of economists) between industry total factor productivity growth and industry R and D intensity. Kendrick (1973, 1980), Edwin Mansfield (1968), and Dale Jorgenson and colleagues (1980) also did important work exploring cross industry differences in growth and the factors behind the differences.

Other parts of the research enterprise focused on various of the sources of growth. Two are important to mention here.

Stimulated by the work of Theodore Schultz (1961) and, later, Gary Becker (1962), a sizable cluster of research grew up concerned with "human capital". That work analyzed investments in human capital through both formal education and work experience. Portions of this research clearly recognized externalities, in some cases of a "network" variety (as it has come to be called), in some cases because workers may leave the firms that trained them carrying those skills (which deters firms from investing in much training), and in some cases because the work some of them do - for example R&D - generates externalities.

Another sizable cluster of research grew up around the topic of technical change. That work proceeded within a number of different styles, from econometric (Griliches 1957, 1973 and Mansfield 1968, 1971, 1977), to historical (Rosenberg 1976, 1982 and Freeman, 1982). Jacob Schmookler (1966) did pioneering work using patents as a measure of inventive input. The research by scholars working in this field covered a range of topics from those stimulated by Schumpeter (Do industries where the firms are large and have considerable market power experience more rapid technical advance than more fragmented industries?), to more general factors that are associated with interindustry differences in technical advance, to the connections between science and technology, to the difference between private and social returns to R&D.

These kinds of analyses certainly alerted many economists both to the fact that technical advance is largely endogenous, and to the fact that private returns to education and to R and D might not measure well the contribution of expansions in these "stocks" to economic output. Some economists also argued that investment in new plant and equipment contributed more to growth than the private rate of return indicated, because new capital embodied new technology and enabled the experience to accumulate to enable its further advance, or because such investment often was associated with the shift of labor from lower to higher value activities, etc. (See e.g. Nelson, 1964)

My discussion above of the Renaissance and blooming of research on economic growth during the 1950's and 60's hardly mentions developments in formal growth theory. As I noted, the enterprise was well on its way before the publication of Solow's and Swan's theoretical essays that are widely regarded as having provided the basic ideas for analyses of growth. My point is that most of the basic theoretical ideas that guided empirical analyses of growth already were there.

By that I certainly do not mean that the early works of Abramovitz and Kendrick or Denison proceeded independently of economic theory, or even neoclassical theory. But the production function idea had been around for a long time, as had the idea that the change in output could be explained in terms of changes in the inputs of the production function and changes in productivity. Abramovitz treats these ideas as essentially "old hat" in his 1952 essay. The notion that, in growth accounting, the output increase stemming from an increase in an input might be approximated by the price of that input was a simple application of neoclassical factor remuneration theory. The idea that the difference between output growth and the factor price weighted growth of input measures growth of "total factor productivity" did not depend on formal neoclassical growth theory.

As the various citations from the Abramovitz 1952 article show, one did not need a formal model to have a theory that new technology often needs to be embedded in new physical capital. The idea that technical advance often is the result of prior investments in R and D, or that education is reflected in human capital similarly were around long before they were incorporated in formal models.

What, then, did the advent and development of formal neoclassical growth theory contribute to the enterprise? In a few cases it added ideas and techniques that were distinctly new. Indeed, the basic proposition in Solow's first growth model, that if diminishing returns to capital are strong, the steady state growth rate is independent of the savings rate, certainly came as something of a surprise to most economists, although many did not believe it. The development and application of duality theory to analysis of growth almost certainly would not have occurred absent formal theoretical work. (See e.g. Gollop and Jorgenson, 1980; Jorgenson, 1986) The putty-clay model (Solow, Tobin, and von Weizacher, 1966) is another example of formal theorizing developing ideas far beyond the stage where appreciative theorizing alone could carry them. However, I think it fair to say that for the most part what formal growth modeling did was to shape up and sharpen appreciative theoretic ideas that had been in the community for some time.

I would propose that the most important contribution of the old neoclassical formal growth theory to research in the field was to make that field more legitimate than it had been, even sexy, and thus to attract many more young economists to its pursuit than would have come, absent the development of formal neoclassical growth theory. One of the tones of Abramovitz' 1952 review piece is that of isolation from the contemporary main stream of economics. Reading it, it is difficult to imagine the surge of young economists coming into the field in the late 1950s and 1960s. It is quite possible that, absent Solow's 1957 article which expressly grounded the empirical calculations in formal neoclassical growth theory, the empirical work of Kendrick and Denison, which involved vastly more digging and calculating, would have received much less attention.

In any case, studies conducted in the 1950's, 1960's, and early 1970's enormously increased our understanding of economic growth. Most of their findings have not been overturned by subsequent studies. The other side of this coin is that subsequent studies following the same line have added little that is new. By the early 1970's there is clear evidence that research of the sort I have described above was experiencing sharply diminishing returns.

C. The Sea-change of the 1970's

This fact was obscured by the sea-change in economic growth that occurred during the late 1960's and early 1970's. By the middle of the 1970's, the key question facing analysts of economic growth was not how to explain the relatively rapid growth that had been experienced by most countries in the early post-war period, but rather "Why has growth slowed down so significantly?".

It was natural that the early studies of the productivity growth slowdown used the same methodology that had been employed in the earlier studies of growth, and focused on much the same variables. (See e.g. Denison, 1979, Griliches, 1980). Denison style analysis showed that, given neoclassical assumptions, while some of the slowdown could be attributed to a fall-off in the rate of physical investment, the principal culprit was a collapse in the rate of growth of total factor productivity.

A number of ingenious explanations were put forth as to why higher energy prices, which were contemporaneous, should have both deterred physical investment (energy and physical capital were complements) and reduced the economic value of technical advance that followed along old lines (technical advance had been energy using). (For a discussion see Jorgenson, 1986). However somehow the explanations never were convincing. And as relative energy prices declined while slow growth continued, the attempts to explain why growth had slowed so much from the heyday of the 1960s changed orientation.

Those whose attention was focused on the United States began to raise the question as to whether the falling off of total factor productivity growth reflected a decline in the rate of technical advance, for some reason. A certain amount of empirical research was directed at that question, but I shall focus here on the more theoretical discussion. There it was proposed that the falling off of the rapid total factor productivity growth after World War II might well have been the consequence of the gradual mining out of the technological opportunities that had been opened up by some major breakthroughs that occurred during the 1930s, and during World War II, but which had not been exploited effectively until the postwar period. Some of this discussion appealed to Schumpeter's theory (1939) that long waves in economic activity were engendered by the fact that fundamental technological breakthroughs occur occasionally, and in clusters. Such developments prefigure a period of rapid economic growth, which then peters out as the new possibilities got mined out, with a resurgence of rapid growth being then dependent upon some major new technological breakthroughs.

A number of economists questioned this interpretation, arguing that, just about the time when productivity growth slowed down, computer technology was beginning its amazing spurt. Thus Robert Solow has been credited with the remark that "computers are everywhere, but in the productivity statistics". This quip was the motivation behind Paul David's careful work (1991) assessing the length of time it took before fundamental advances in electrical technology, most of which occurred before the beginning of the twentieth century, began to have major effects on productivity. David argued that it wasn't until the 1920s that that effect really took hold. His story recounted the very large number of relatively minor technological advances that needed to be achieved before the potential latent in electrical technology could be exploited, and also the changes in firm organization, and institutional arrangements, that were required.

The discussion of whether in fact there has been a significant decline in the rate of technical advance, or at least the reflection of technical advance in productivity growth, since the early 1970s remains inconclusive. However, one consequence of that discussion is that economists, concerned with that question, have a much richer understanding of the nature of technical advance, and of the intertwining of technical advance with organizational and institutional changes, than they used to have.

Another important body of appreciative theorizing set off by economists struggling to understand the sources of the productivity growth slowdown can be grouped under the banner of "convergence theory". The slowdown of growth in the United States was accompanied by sharpening perceptions that the other major industrial nations were growing faster than the Americans, and catching up. However, as attention focused on these phenomena, two others matters came into view. One was that this catching up was not something that began in the 1970s, but rather had been occurring since the 1950s. The other was that the European economies and Japan also had been experiencing significant slowdown in growth since the early 1970s.

The starting point for much of the theorizing, and empirical work, about convergence was the observation that, at the end of World War II, Europe and Japan were far behind the United States technologically and, in many cases, in the organization and management of modern business enterprises. Thus there were major opportunities for those nations to increase their labor and total factor productivity by adopting American practice, and the physical investments needed to do this were highly profitable. According to this theory, the very rapid growth after World War II in Europe and Japan can be explained largely as this catching up or "convergence" process. (See e.g. Abramovitz, 1986 and Baumol, 1986). The other side of this coin is that, as these nations caught up with the United States, opportunities for further rapid growth through emulation diminished. This is why slower growth in Europe and Japan, which set in during the late 1960s, was inevitable. However, as the research on the convergence hypothesis proceeded, various complications came into view. In the first place, the U.S. productivity and technology lead over Great Britain and Europe was very large even prior to World War I, and there is little evidence of "catching up" in the interwar period. What has been different about the postwar era? Second, while the simplest version of the convergence hypothesis would lead one to expect that countries farthest behind the leader would experience the most rapid "catching up", the relationship here is weak. While it is consistent, for example, with Japan's very rapid growth in the 1950s and 1960s, when Japan was, arguably, behind Great Britain and Western Europe as well as the United States, it seems inconsistent with Japan's continuing relatively rapid growth after it surpassed Great Britain.

Regarding the first question, Gavin Wright and I (1992) have elaborated an hypothesis, put forth by Abramovitz in his 1986 essay, that the post World War II environment of relatively open trade in manufactured goods and natural resources, and free international flow of both financial and physical capital, made the postwar era far more conducive to convergence among nations with the requisite skills and institutions than the interwar period. However, while some may find this argument persuasive, it is apparent that it is a complex one about the role of national and international institutions in the growth process.

Regarding the second puzzle, Abramovitz (1986) has used the term "social capabilities" to denote whatever it takes for a national economy to take on board productively the technology and organizational practices of the leading economy. He has provided a sensible list of what might lie behind social capabilities. Adequate "human capital", ability to organize and manage competent firms, financial and educational institutions that can support economic development, all are on the list. Abramovitz' use of the term "social capabilities" suggests that more generally he means by them the "political, psychological, or sociological" factors he had in mind in his 1952 essay. Other economists have used the term "institutions" to denote much the same thing (see e.g. North, 1990).

There is a long tradition in economics of pointing to institutional factors as explaining differing economic performance among nations. Much of Adam Smith's The Wealth of Nations is exactly about this. Institutional factors are central in many arguments about why Britain lost her position of technological leadership to the United States and Great Britain in the early years of the twentieth century. Thus according to one account (see Landes, 1970, for a good exposition), by the late nineteenth century the new chemical product and electrical technology required university trained scientists and engineers for their development, and called for firms that funded and supported industrial R and D laboratories. The U.S. and Germany had educational systems that were responsive to the former need, and were able to support the growth of firms that could meet the latter, whereas Britain was less able. Chandler (1990), and in a different way Lazonick (1990), have placed strong weight on differences between Great Britain, on the one hand, and the U.S. and Germany, on the other, in the way large firms were organized and managed.

The recent literature proposing that Japan is overtaking the U.S. as the leading technological and economic power similarly is concerned mostly with firms and institutions supporting them. The argument is that Japanese firms are more capable than American ones of making long term investments in the physical, but particularly human, capital, that now are needed to be competitive in many industries (Lazonick, 1990). Japanese firms also are more flexible in the specification of jobs, and job assignments, and less compartmentalized organizationally (see e.g. Dertouzos et al, 1989, Womack et al, 1990, and Aoki, 1990.) The relatively rigid hierarchical organization of American firms which served adequately in an earlier era is a handicap in an era of rapid technological change, and Japanese firms are much better able to deal effectively with the new contingencies. In turn, the nature of Japanese capital and labor markets, and the Japanese educational system, provides support for the practice of Japanese firms in making long run investments in broadly training their workers.

I note several common elements to the appreciative theoretic lines of argument sketched above. First, in Abramovitz' terms, they all represent attempts to get "behind" the "immediate" variables influencing production and economic growth. General neoclassical economic theory, and this is what guided most of growth theorizing during the 1950s and 1960s, both formal and appreciative, is focussed on inputs, outputs, prices, their equilibrium configurations, and associated phenomena. It is not well oriented towards considering things like technology as a body of practice and understanding, firms as productive organizations, or national and international institutions, particularly those that shape the evolution of technology and the organization and behavior of firms, which clearly lie "behind" in some sense the immediate determinants. Yet this is exactly what the theoretical stories sketched above are about.

Second, the appreciative theoretical accounts intertwine discussion of tehcnology and technical advance, with discussion of firms and institutions. While standard growth theory has taken on board technology, or at least its reflection in total factor productivity, and the new growth theory has explored various inputs contributing to technical advance, these theories have not yet struggled with the connections and relationships in the stories accounted above.

Yet if the appreciative stories are on the mark, the attempts of the new formal growth theories to better come to grips with technical change may be sorely limited. From another point of view, the developing appreciative theory may be providing strong clues as to where formal theory ought to be trying to go. Or at least this is the theme I will be developing in the following sections.

III. Teachings of Appreciative Theorizing on Technology, Firms, and Economic Institutions

The understandings that long run growth depends on technical progress, that technology is neither a pure public nor a pure private good but is something of both, and that for an economy or firm to tap into public knowledge generally requires significant investment on its part, have been taken aboard by much of the new formal growth theory. However, formal growth theorists have not given much attention to the question "What is technology anyhow?".

There are of course a few simple metaphors about that. One is that prevailing technology is like "a set of blueprints", suggesting the conception of a "technology library", albeit one with some of the "books" proprietary. The notion that technology is "knowledge" also has been around for some time, suggesting something more embodied in humans.

Research by economists on technology and technological change supports the idea that a lot about modern technologies is described in blueprints, texts, pictures, equations. However, in many fields it takes a highly trained professional to make sense of the "blueprints". Further, even for professionals, for most technologies access to the documents provides only a start on what it takes to make a technology work. A lot of learning by doing and using is often required to gain real mastery of a technology. And it is increasingly apparent that for many technologies much of the "knowledge" that is needed to command a technology is "know how", that is in the fingers as well as in the head. (For good discussion see Pavitt, 1987, and Dosi, 1988).

These elements of appreciative theory regarding command over technology carry over to understandings about how technical advances comes about. Scholars of technical advance long have known the importance of investment in R and D, generally involving the employment of professionals trained in the relevant underlying engineering and scientific disciplines, in the generation and development of new technologies. However, they also long have understood that many technologies seem to experience a continuing stream of improvements that reflect understandings gained and changes wrought through learning by doing and using. (See e.g. Rosenberg, 1982.) In many technologies it is apparent that both processes are involved, and that they interact strongly. Thus while what is learned in experience sometimes directly results in changes in practice and design, in many cases this learning has its impact largely by feeding back to influence the problems and targets addressed through R and D.

A number of studies now have documented that broad new technologies tend initially to be brought into practice in crude form, representing a bundle of potentialities, rather than practice that is operationally ready (see e.g. Enos, 1962, and Nelson and Rosenberg, 1993). The automobile, the airplane, the transistor, the computer, and the laser, all surfaced as new technologies of potential wide applicability, but requiring considerable work and ingenuity before they would be worth anything in economic use. It took a long time, and a lot of investments, and a lot of learning, and learning how to learn (Stiglitz, 1987) before these new technologies became major contributors to economic growth.

And to a considerable extent, this learning went on in firms. While economists studying technical advance and economic growth now are coming to understand this, and some of its significance, this understanding has yet to get out very far in the profession at large.

While there are changes in the air, until recently few economists have shown much interest in what actually goes on in firms. There are a number of reasons for the negelect. An important one is that, unlike scholars of business management and strategy, the interest of economists is mostly in variables at a level of aggregation well above that of individual firms, often macroeconomic variables, and even our "microeconomics" is about industry level variables rather than firm level ones. But perhaps a more basic reason is that we economists tend to work with theories that suggest that, at the levels of aggregation we are interested in, what firms do can be presumed to be determined by constraints, opportunities and incentives provided by the environment they are in. Thus, there is no call to look carefully at firms, per se. They are simply puppets dancing to the tune played by the market.

However, under the appreciative theory sketched above, mastery of a technology is more like a skill that needs to be learned whose control requires practice than most neoclassical theorizing is wont to admit, and the entity that learns and practices is the firm. The practice of complex technologies inherently involves organization and management. The way a firm organizes to implement a common broad technology can make an enormous difference. This is a key finding of much of the recent work comparing U.S. and Japanese auto production.

Studies of American and Japanese firms of the sort mentioned earlier has been one important stimulus to new thinking about firms. Another major stimulus, also mentioned earlier, has been Chandler's pioneering historical work on the rise of the modern corporation (1962, 1977, 1990). Particularly Chandler's work has led to the development of a small body of writings that see key firm capabilities as dynamic, rather than static, involving the ability to learn, adapt to changes in the environment, and innovate, and not simply to perform well given prevailing practice and conditions. (See Winter, 1988, and Dosi et al, 1992) This new body of writings on firms is, of course, quite conformable with the new theorizing about cumulative technical advance, sketched above.

While there are signs lately that economists are paying more attention to firms, (e.g. Williamson, 1985, Holmstrom and Tirole, 1989), few yet seem able to see firms as the key actors in economic growth in the sense that Schumpeter did, or more recently Chandler. Recall Abramovitz' citation of Schumpeter's argument that the productivity of capital, or R&D, is largely a function of "enterprise".

Of course what firms do, and the technologies they employ, and develop, are influenced to a considerable extent by the environment they are in. Economists are inclined to define the environment in terms of markets. In turn behind markets are demanders of products and suppliers of inputs (who may be individuals or organizations like other firms), and their preferences, and the constraints they face.

However in recent years at least some economists have become cognizant of aspects of the environment not really considered in the simple treatment. (In a way the new awareness of institutions represents a renaissance of earlier thinking. For a good discussion see Hodgson, 1988) There is increasing recognition among economists that there are entities out there like universities that do research that feeds into technical advance in industry, and whose teaching programs affect the supply of scientists and engineers, government agencies financing certain kinds of R and D, and others setting standards, banks and banking systems, and a variety of organizations and laws which affect labor supply, and demand. Patent, regulatory, and liability law are part of the environment. And so also are a variety of widely shared beliefs and values and customs that affect common expectations about what should be done, and what will be done, in a particular context.

This is an extraordinarily complex bag of things, and it may be foolhardy to give a name to the collection. But as I have noted many scholars have called them all "institutions".

One can question what is common about them. Some economists and other scholars have employed the language of game theory and attempted to define institutions as "the rules of the game" which, given the motivations of the players, constrain the way the "game" will be played. (See e.g. North, 1990) Other economists, stressing that many repeated games have multiple equilibria, have proposed that the concept of "institutions" needs to include not only the formal rules of the game (the "law") but also the particular equilibrium or self-sustaining pattern of play that has evolved (the "custom"). (Schotter, 1981, was among the first to argue this way) This definition seems to fit both statute and common law, and government policies, including the particular ways they have come to be enforced. More generally, it seems to fit the durable parts of "public environment" within which individual actions proceed, and which constrains and frames such actions.

It does not directly seem to fit the "organizations" in the environment, like dominant firms like IBM, and universities and banking systems. However while, according to the definition above, particular organizations would not be considered as institutions, generally accepted forms of governance and structure of kinds of organizations might be. Thus to the extent that Harvard or the University at California are taken as models of what research universities should be, and other universities model themselves after them, one can speak of research universities as institutions. In this same sense one also can see corporate forms widely prevalent in an economy as institutions, to the extent that there is a belief that these forms are right and appropriate. (Williamson, 1985) This clearly is the intended meaning of scholars who have argued that American firms are stuck in their old common ways and beliefs.

Of course institutions are not constant. They do change, if perhaps slowly. The question of how they change would appear to be a fundamental challenge for growth theory.

In the 1970s a few intrepid economists put forth the hypothesis that "institutions evolve optimally". (See e.g. Demsetz, 1967, and Davis and North, 1971). Just how this was supposed to happen was never layed out, however. Since that time economists have become much more aware of major differences across nations in institutions - differences that seem to make a big difference - and thus to be more aware that in some countries the processes that guide the evolution of institutions seem to be more effective than in other countries. They also have come to understand that these processes are very complex, and poorly understood. (see e.g. North, 1990).

In his 1952 article Abramovitz flagged broad national institutions, supporting or constraining industry, as at once something economists need to understand if they are to understand growth, and a topic whose exploration will require them to step over the traditional boundaries of their discipline. In my view getting a good intellectual grip on institutions is going to be harder than getting a better model of technological change, or firm capabilities, and their dynamics, simply because "institutions" are so diffuse. But as Abramovitz said forty years ago, if we are to understand growth we will have to somehow understand institutions.

V. An Agenda

Understanding economic growth better surely should be of very top priority for economic research during the 1990s. There is so much that we don't understand. The slowdown in growth in the United States after the 1960s remains a puzzle. Economists still have little clue regarding whether that slow growth reflects a general falling off of technological progress which will affect productivity growth in all countries as they approach American levels, or whether a good share of the cause is specific to the United States who will soon be surpassed by other economies. We have a somewhat better understanding of why growth rates in Europe and Japan slowed after the late 1960s. However, our understanding of why Japan moved from the bottom of the pack of advanced industrial nations in the early 1960s to close to the top of the pack by 1990 is not very strong.

Limitations on space have prevented me from more than simply mentioning the extremely uneven performance among nations that were very poor as of 1960. Some, like Korea and Taiwan, have grown rapidly and seem to be becoming sophisticated industrial powers. The less developed nations who have not had that successful experience naturally look to Korea and Taiwan, but it is not very clear exactly what went on in those economies that others can readily imitate.

The surge of writings that is coming to be called "the new growth theory" reflects this growing awareness among economists that much about economic growth remains a puzzle. Some of the new models have effectively incorporated a number of the understandings about growth, and particularly about technical advance, that have been gained over the years by those doing empirical research. This is at once a gain for the formal modeling part of the research enterprise, and a gain for the appreciative theorizing part, enriching the former, and bringing new rigor to sharpen the insights of the latter.

However, while the new growth theories have taken on board important elements of recent appreciative theorizing about growth, there are a number of elements that have been missed. I want to conclude this essay by highlighting aspects of technical advance, of firms, and of institutions, that in my view are high priority targets for incorporation in formal growth theory. I also want to discuss the treatment of uncertainty.

Technical Advance: Some of the new growth models focus on endogenous technical advance won through the investments of business firms (e.g. Aghion and Howitt, 1990, Grossman and Helpman, 1989, and Romer 1990) and others on the productivity enhancing effects of the growth of human capital (Lucas, 1989), with the latter argument associated (at least partly) with the proposition that human capital contributes to the advance of technology. However, to date the new models have not taken in the understanding that both available highly trained human capital, and investments by firms to use such capital in creating new technology, are required for technical advance. Modeling R and D as requiring certain kinds of human capital would get some of the key complementarities and interactions expressed in appreciative theorizing into the formal growth models.

Also, the new growth theories treat the dynamics of technical advance rather mechanically. The cumulative learning process whereby a new technology develops towards its potential and finds it range of fruitful uses is almost totally repressed (although Lucas, 1993, captures some elements of it). This limits the ability of the formal theorizing to engage with appreciative theorizing about, for example, long waves, or, the long-time lags that often are involved before a major new technology affects productivity (David, 1991). And, to the extent that cumulative learning is specific to particular technologies, building it in would enable the theory to address a range of phenomena associated with competing technologies, and firms and countries that are specialized in one or another. (See e.g. Arthur, 1988, 1989, and David, 1985, 1992).

Firms: The new formal models continue in the spirit of the older ones in treating the actions taken by firms as determined by the environment they are in, and ignoring anything like Schumpeter's "entrepreneurship" or Abramovitz' "enterprise". Of course formal theories aimed at explaining growth at a macroeconomic level call for a quite parsimonious treatment of firms. However, if the objective is to capture, albeit in stylized form, some of the key insights about, for example the factors behind differences in the growth performance of the U.S. and Japan, that appreciative theorists have highlighted, it seems essential not to have a model in which what firms do is tightly and uniquely determined by the broad economic environment they are in. Rather, one needs a model in which firms have room to develop rather unique characteristics. In turn, the characteristics of firms, as they have developed in a particular era and country, can be treated as a factor influencing economic growth.

In modern economies firms are not only the dominant undertakers of R and D and physical investment. To a considerable extent they are the sources of the funding for these activities, and they determine how these investments are allocated. As the dialogue about Japanese and American firms has brought to our attention, firms provide much of the effective demand for people with various kinds and levels of training and skills, and in some economies provide a good fraction of the training. Firm behaviors that are unprofitable over the long run tend to get extinguished. But both inter- and intra-country comparative analysis reveals that, at any time, there is apparently considerable room for variation. And it is nearly a sure thing that that variation matters. If so, formal growth theory needs to take it aboard.

Institutions: While business firms clearly are the central actors in most areas of technical advance, it seems important to bring other institutional actors into the formal theory. My first candidate is universities. Whereas firms can be viewed as funding themselves, and doing research that leads both to proprietary technology and "public externalities", universities might be modelled as entities that are funded by governments (out of tax revenues) and which create knowledge into which all firms can tap. I note that one advantage of this formulation is that introduction of "basic research" in this way provides another way of seeing why the system as a whole does not seem to be experiencing diminishing returns. Another advantage is that it builds into the formal theory a way of encompassing the public aspects of technological knowledge without having to rely totally on unintended "spillovers".

For the reasons I indicated above, building in "institutions" to our formal growth theories will be a difficult challenge, among other reasons because the things that are called institutions are so diverse. Some models of course already have built in things like tax laws. The introduction of universities would build in another kind of institutional actor. This would seem a useful step forward.

Uncertainty: All of the above discussion calls attention to a more general mismatch between the new formal growth theories, and the appreciative theories discussed above. It lies in the treatment of uncertainty and the behavioral consequences of uncertainty. Several of the new formal models treat the advent of new technology probabilistically. However, in their specification of the knowledge and behavior of the actors, the authors assume that the actors know that probability distribution and make their decisions optimally in the light of that knowledge. In Frank Knight's terms, this reduces the uncertainty to (correctly understood) risk. However, a hallmark of technological advance, as the scholars studying it empirically describe the process, is that the involved actors do not have sufficient information to assign meaningful probabilities to the possible outcomes of their doing one thing or another, or even to list the set of possible outcomes in any detail. Where they do assign probabilities, or act as if they did, with the vision of hindsight it often appears that they had them quite wrong.

Of course the key question is what kind of important limitations or misspecifications in modeling growth are caused by treating uncertainty as if it were correctly understood risk. I would argue that one consequence is that such a treatment makes it difficult (although not impossible) to model technical advance as involving the diverse bets (perhaps invoked by different broad firm strategies) made by a number of competing firms, with ex-post selection determining the technology (and perhaps the firms) that ultimately comes to dominate in a field. Under such a formulation, variables like the number and diversity of competitors, and the extent to which some (at least) propose radical departures from the status quo (which may succeed or fail), become factors influencing technical advance, as well as simply total R and D investments. (This perspective on technical advance is, of course, that taken by "evolutionary" theory. See e.g. Nelson and Winter 1982 and, for a survey, Nelson, forthcoming). In turn, such a perspective could lead to an attempt to model "institutions" that tended to support, or hinder, diversity and creativity.

Over forty years ago Abramovitz flagged technical advance, the role of the enterprise, and the broader cultural and institutional factors surrounding and supporting enterprise as the factors behind the "immediate" sources of growth, and said that understanding all three was a challenge. It still is. Appreciative theorizing for some time has been struggling with the questions about technology and technical advance, and recently has begun to home in on firms. Appreciative theorizing about institutions is experiencing a welcome renaissance. But if my arguments about the relationships between appreciative theorizing and formal theorizing are on the mark, to be fully effective appreciative theorizing needs help from formal theorizing. The "agenda for formal growth theory" that I have sketched above may be regarded as a call for such help.

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