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THE ECONOMICS OF INNOVATION

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The Economics of Innovation

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The Economics of Innovation

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Summary

Interest in the economics of innovation has been growing very rapidly in the 1980s. Whereas Schumpeter stood almost alone in the first half of the century, there has been a renewed burst of empirical and theoretical analysis in the most recent period. This selection of the seminal papers published in the 1980s starts with Schumpeterian theory but goes beyond his pioneering formulations. About half of the papers were originally published in leading journals of economics but the other half were published in journals dealing with policies for science and technology as this is an area where technology and economics meet. Whereas in the 1960s most of the literature was published in the United States, in the most recent period European economists have made a major contribution. This is reflected in the selection of papers which cover all the recent major developments including evolutionary theory, strategies of firms, path dependency, diffusion of innovations and paradigm change.

Introduction

All schools of thought in economics have always recognized the central importance of technical innovations and of organizational innovations for the competitive performance of firms and of nations and for the long-term growth of the world economy. However, in spite of this consensus, in the first half of this century the vast majority of economists devoted little or no attention to the study of technical change and were content to leave this to historians or to technologists. Schumpeter was almost alone among leading economists in placing innovation at the centre of his theoretical system from his first classic work on the *Theory of Economic Development* (1912) until his death in 1950.

In the second half of this century the climate has changed. At first in the 1950s the change was slow and almost imperceptible but it has been gathering force in the past thirty years. There are many indications of this. One example is in relation to the *diffusion* of innovations through the economic system. When he made his pioneering study of research on diffusion in the early 1960s Rogers (1962) could find only one example of empirical research by economists on the diffusion of innovations in manufacturing industry. A quarter of a century later in his paper on diffusion at the DAEST Conference in Venice he reported on the explosion of research publications in this area (Rogers, 1986). A similar story could be told in relation to the study of research and development activities within enterprises, the origins of innovations, the role of patents, the influence of innovation on competitive trade performance and on productivity growth.

In all these areas and in others too there has been an upsurge of empirical research and of more fundamental theoretical analysis. This growth of interest was clearly apparent already in the 1960s, especially in the United States, when the pioneering work of Mansfield, Nelson, Rosenberg and Scherer in particular stimulated a whole generation of post-graduate students. The Conference on the *Rate and Direction of Inventive Activity* (Nelson (ed.), 1962) convened by the National Bureau of Economic Research was a major landmark in this upsurge. But it was notable that even as late as 1971 in an earlier volume of 'Readings' on *The Economics of Technological Change* (Rosenberg (ed.), 1971) not a single paper came from an economist working outside the United States. This situation has now changed dramatically and in the 1970s and 1980s European economists have contributed substantially to the research in this field. This is now becoming increasingly true of economists from Japan and other countries too.

Because of the huge increase in publications relating to the economics of innovation the task of selection has been extremely difficult. It proved necessary to exclude certain topics almost entirely. The economics of innovation as it relates to international trade, under-development, financial institutions, agriculture, employment, regional development and project evaluations are among the important areas which receive all too little attention. Most of these topics are however the subject of other volumes in this series of 'Critical Writings'.

It was essential to concentrate attention on the 'heartland' areas of the economics of innovation in order to keep the selection within manageable proportions and to focus attention on the most significant advances both in theoretical and empirical work. Moreover because

the field is moving so fast the selection has been made almost entirely from the publications of the most recent period i.e. the last dozen years or so. This has meant the exclusion of much material relating to the period, already referred to, which was published in the 1960s and early 1970s; for example, the work of Nelson on the economics of basic research; of Scherer on firm size, oligopoly and research; of Mansfield on diffusion of innovations; or of Schmookler and others on the role of 'demand-pull' and 'supply-push' in generating innovations. Fortunately this stream of early United States literature has already been amply represented in earlier anthologies on innovation (for example Rosenberg (ed.), 1971) and in major review articles (for example, Kamien and Schwartz, 1975; Kennedy and Thirlwall, 1972; Mowery and Rosenberg, 1979). However, this selection does include one major paper by Dosi which is both a recent and original synthesis of innovation theory and a summary of much of the post-war literature relating both to the origin of innovations (including the R. and D. system) and the diffusion of innovations.

The Dosi paper constitutes the whole of Part II of the book and serves also as an introduction to the main themes discussed in Parts III, IV and V. However, since it concentrates on the *micro*-economics of innovation, it is preceded by a first section devoted to the basic theme of Schumpeterian economics - the evolution of the economic system as a whole. Part III deals with the innovative behaviour of firms and with the diversity of this behaviour in various industrial sectors. Part IV deals with the selection environment confronting entrepreneurs in their efforts to launch innovations. This selection environment consists not only of markets and competitive firms but also a wider institutional framework which includes an 'appropriability regime', industrial standards and other structures which may penalize some and encourage other types of innovation. The patterns of innovation which emerge from this interaction of firm-level activities and strategies and the selection environment are the subject of the final section of the book - Part V. The wheel has then come full circle to the evolution of the macro-system discussed in Part I.

The agenda for much of the research on innovation in the late 1970s and 1980s was set out by Nelson and Winter (1977) in their seminal paper entitled 'In Search of Useful Theory of Innovation'. In this paper they pointed to certain fundamental characteristics of innovation which present a challenge to mainstream economic theory as well as to economists working on a more Schumpeterian approach. The uncertainty inevitably associated with innovation means that some of the central assumptions of neo-classical theory relating to rational profit-maximizing behaviour are untenable. Moreover, disequilibria or multiple equilibria are more characteristic of the system's evolution than the general equilibrium postulated in mainstream theory.

This fundamental challenge to orthodoxy is the subject of the five papers included in the first part of this volume. It starts with Nelson and Winter's own statement of the problem in 1974 which foreshadowed their major contribution to 'An evolutionary theory of economic change' (1982). As they are the first to acknowledge, the attempt to develop an evolutionary theory owes a great deal to Schumpeter (1942) who insisted that the first thing to understand about capitalism is that it is an 'evolutionary process'. Elliott's paper, which follows that of Nelson and Winter, compares Schumpeter's evolutionary theory with that of Marx. Although this is an exploration in the history of economic thought it is essential to a proper understanding of the fundamental issues involved in the contemporary debate. Evolutionary theory necessarily implies the restoration of *history* to a central place in economic thought. This is apparent

above all in the current recognition of the crucial importance of path-dependency in the decision-making of the firm, recognized alike by leading neo-classical theorists such as Hahn (1987) and Schumpeterian economists such as Arthur (see his paper in this volume). As Elliott points out both Marx and Schumpeter were distinguished by their Herculean efforts to integrate economics with sociology and economic history.

This emphasis of evolutionary economics on the importance of history has much in common with some trends in the new institutional economics. They could both be described as attempts to interpret economics as if technical change and institutional change really mattered.

The brief paper by Winter ('Comments on Arrow and Lucas') is a small gem, which is included in order to point out the contrast between the evolutionary approach and the sustained attempts to rescue orthodox theory (described by Winter, following Blaug, as the 'Classic Defence' of the rationality-optimization paradigm). Winter highlights the growing difficulties of the advocates of the 'Classic Defence' in the face of the massive empirical evidence on the *actual* decision-making process within firms, above all in relation to innovation. They now almost all concede that the rationality theory is 'not descriptive of the actual process by which decisions are reached' and that most decisions emerge from adaptive learning processes. They have had to retreat into the increasingly esoteric realms of 'as if' theorizing – firms cannot actually make rational optimizing decisions in the real world because of uncertainty about the future but they nevertheless supposedly behave 'as if' they were managed by super-optimizing, all-knowing agents, as otherwise they would not survive. Winter, like most of the economists represented in this volume finds it hard to understand why so many economists continue their work *as if* Ecclesiastes was right and there is nothing new under the sun. Evolutionary economists take the proposition of Heraclites as self-evident: that you cannot bathe in the same river twice. 'We Heraclitean types find it difficult to understand what the Ecclesiastes types are talking about, what with the universe expanding, the continents drifting, the arms race racing, and the kids growing up.'

However, it is not sufficient in the development of any discipline for the defects of an established theory to be pointed out or its main tenets to be refuted by the empirical evidence and by theoretical arguments. It is also necessary for better models and theories to be developed. No-one would claim that the evolutionary economists have been completely successful in the 1980s in this endeavour. But they have certainly made some significant progress with the agenda set out by Nelson and Winter in 1977. They themselves set the wheels in motion and the 1980s ended with an attempt to synthesize the work of a large group of evolutionary economists in a major book (Dosi, Freeman, Nelson, Silverberg and Soete (eds), 1988). This stream of work is represented here by the two concluding papers in this section by Fagerberg on the one hand and by Silverberg, Dosi and Orsenigo on the other.

Fagerberg's paper offers a 'Technology Gap' explanation of why growth rates differ. It analyses the evidence for 25 countries for the period from 1960 to 1983 and demonstrates a close correlation between the level of economic development and the level of technological development. Finally, the paper by Silverberg *et al* describes an original evolutionary model of the diffusion of innovations, which attempts to take into account the main findings of empirical research on technological and behavioural diversity, uncertainty, learning processes and disequilibrium dynamics.

These empirical research findings are the subject of the next three sections of this volume. In their agenda-setting paper Nelson and Winter (1977) put the main emphasis on the *diversity*

of innovation in various industrial sectors and within industrial firms. They pointed to the need to explain this diversity in any satisfactory theory and (an even more difficult task) to explain how ordered patterns of innovation could emerge despite this diversity and the uncertainty inevitably associated with innovation. How can structure, conformity and order emerge from this chaotic variety?

Dosi's paper in Part II, although a long one, actually condenses the results of an enormous amount of empirical research on the sources of innovation, the search procedures developed by firms in various industrial sectors, the nature of technological trajectories and paradigms, the characteristics of the 'technology accumulation' process within firms and its relationship to publicly available results of basic research, incentives to innovation including appropriability and inducement mechanisms, sectoral patterns of innovation, industrial structures, and patterns of diffusion. Most of the work which follows in Parts III, IV and V is referred to in Dosi's paper. It thus forms both an admirable summary of three decades of research findings and a useful introduction to the papers which follow.

In keeping with the historical approach which has been stressed, the first paper in Part III by Mowery deals with the early history of industrial R. and D. in the United States. The industrial R. and D. laboratory was itself an institutional innovation and its establishment and diffusion raises numerous interesting issues. Among the most important are that raised by Mowery: why did firms prefer to do their own R. and D. rather than sub-contract it to the numerous independent specialized contract research institutes and cooperative associations which were growing up in parallel with the in-house facilities? Mowery's explanation, based on the historical evidence, is that the market mechanism could not work in this area because of the unique characteristics of technology accumulation, the complementarity between research and production activities (especially of course in the case of process innovation), and the need to combine a variety of heterogeneous inputs in the innovative process.

Mowery rejects alternative explanations based on the appropriability of research results and the lack of protection afforded by research contracts. That some of these explanations may be regarded as complementary rather than alternatives is suggested by some of the later papers in this section and in Part IV. In particular the paper by Teece develops the theory of 'complementary' assets in the explanation of strategic innovative behaviour of firms. This highlights the firm-specific advantages of knowledge accummulation within firms especially in relation to production and marketing.

The paper by Gold which follows that of Teece is based on in-depth studies of the actual decision-making processes of firms in adopting innovations. It serves to re-emphasize the importance of 'bounded rationality' – the severe limitations on the capacity of firms to make ex-ante assessment of future changes in the environment, future modifications to innovations which are diffusing and the future streams of costs and benefits. The paper also provides an incisive critique of much of the earlier research on diffusion, in particular the implicit or explicit assumption in many models of an unchanging product diffusing to a stable number of potential adopters in an unchanging environment.

Both this paper and the later paper on diffusion by Metcalfe (Part IV) point to the necessity for diffusion research to take into account the *systems* aspects of innovation and the behaviour of *producers* as well as *users* of innovations. The systemic and cumulative aspects of innovation are also central features of the paper by Rothwell and Gardiner on 'Re-Innovation and Robust Designs' and the paper by Maidique and Zirger on 'The new product learning

cycle'. Just as much research on diffusion in the 1960s and 1970s tended to concentrate on the individual product, so also much research on success and failure of original innovations tended to concentrate on the individual innovation project, rather than families of innovations or a stream of attempted innovations by successful (or unsuccessful) firms.

The original model for much of this research on success and failure was the SAPPHO project at the Science Policy Research Unit designed by Curnow and Freeman in the late 1960s (SPRU, 1972; Freeman, 1982) and continued by Rothwell *et. al.* during the 1970s (Rothwell *et. al.* 1974). But just as it was necessary to exclude the early work of Mansfield and his colleagues in relation to diffusion, so it was possible in this case to include only the more recent critical re-appraisal of this stream of research. The paper by Maidique and Zirger both presents such a critique and also the results of their own SIPRO project at Stanford. While using the original SAPPHO technique of paired comparisons this attempts to cover not just individual projects but a succession of attempted innovations within firms. The authors point out that this has the advantage of taking into account the cumulative learning process within firms and between firms, and in particular learning from *failure* as well as from success. Rothwell and his colleagues had already in the 1970s in the course of their own work in the SAPPHO project moved on from the study of the successful individual innovation to the study of the successful innovating firm and the paper included in this volume takes this approach one stage further by analysing the concept of 'robust designs' – projects which provide the bases for successive profitable generations of a product, 'stretching' its capacity with each 're-innovation'.

The emphasis from empirical research results on knowledge accumulation within firms as the source of innovation success was accompanied by recognition of the diversity of this learning process within different industries and sectors. Both the original SAPPHO project and its various successors were careful to stress that the results applied to particular sectors under investigation, such as chemical processes, scientific instruments or textile machinery. None were so large in scope as to make it possible to generalize about innovations across the entire economy. The paper by Maidique and Zirger is also specific to one industry – electronics – although the results are of wider significance because of the diffusion of micro-electronic technology through the entire system. However, the development of a data bank on innovations at SPRU covering a very wide range of industrial sectors made it possible for Pavitt to develop an original taxonomy of sectors in terms of their characteristic styles of innovative activities. His much-cited paper made use not only of the SPRU data bank but the cumulative results of much other research on individual sectors and firms. His paper not only synthesized the results of this research but contributed substantially to one of the main demands made by Nelson and Winter in their research agenda – the analysis and explanation of diversity in a way which would aid policy formulation. Pavitt's taxonomy points to the need for sector-specific technology policies which however must take into account the inter-sectoral flows between industries. Scherer's (1982) input-output matrix of technology flows makes essentially the same point. Pavitt distinguished three major categories of industry:

1. 'science-based', for example, electronics and chemicals
2. 'production-intensive', for example, steel, automobiles
3. 'supplier-dominated', for example, agriculture, services

As he would be the first to agree, the actual mix of industries in each category changes historically over time with the rise of new industries and the transformation or decline of old ones. It is possible, for example, that a number of service industries and even agriculture may not be so 'supplier-dominated' in terms of their innovative inputs in the 21st century as they have been in the 20th. This point is important in relation to the final paper in this section by Kodama. His analysis of 'technology fusion' was facilitated by the nature of the Japanese R. and D. statistics, which make it easier than in most other countries to analyse those R. and D. activities which cross sectoral boundaries. Diversification into new sectors appears to be particularly important in the Japanese context. As Maidique and Zirger suggest in concluding their paper this may be one of several features distinguishing a Japanese model of strategic innovation behaviour (Imai *et. al.* 1982; Freeman, 1987).

The innovations which emerge and survive in a capitalist economy are shaped of course not only by the searching, re-searching and learning activities of firms and a network of supporting scientific institutions and universities, but also by a selective environment. The interplay between the innovative efforts of firms and this environment is the subject of the papers grouped in Part IV. It is not possible for reasons of space to consider more than a few aspects of this complex environment. Those which have been chosen are however characteristic of the main thrusts of innovation research in the past quarter century. The first topic is the Schumpeterian market structure debate which has continued ever since Schumpeter (1928) first suggested that monopoly and oligopoly provided a more favourable environment to nurture innovations than small firm competition. The second related topic is the appropriation regime which has already been introduced in Part III and in particular the role of patents in this regime. The third topic has also been introduced with Gold's paper and relates to other institutional aspects of the environment on *diffusion* of innovations.

In relation to the first of these topics the papers by Soete and Kaplinsky illustrate the changing nature of the debate on market structure and innovations which has rumbled on for over half a century. In the 1960s there was a strong tendency to question the validity of the Schumpeterian hypothesis on the basis of empirical data relating mainly to patents, and to argue that in the larger firms in particular, R. and D. and inventive activity did not increase in proportion to size. Soete on the other hand claims that better and more recent data for the United States in the 1970s tended to validate the original Schumpeterian position for most industries. In the 1980s, however, the role of new small firms in making innovations was increasingly apparent in such areas as electronic instruments, CAD and software. This led a number of researchers not so much to disagree with Soete's vindication of Schumpeter but to emphasize the changing historical context. Phillips (1971) had already pointed to the contrast between the 'young' Schumpeter with his emphasis on the individual innovative entrepreneur and the 'mature' Schumpeter (1942) extolling the virtues of oligopoly and insisting that 'perfect competition is not only impossible but inferior'. But this contrast could be explained as much by changes in the external environment as by changes in Schumpeter's theory. It would be astonishing if his work has not reflected the increased concentration of industrial R. and D. in the 1920s and 1930s.

Equally, the new wave of small firm innovations in the 1970s and 1980s demands investigation and analysis. As Pavitt, Robson and Townsend (1987) have conclusively shown, this does not mean that large oligopolistic firms have been displaced as the main focus of innovative activities in advanced capitalist societies. What it does mean, as the Kaplinsky

paper demonstrates, is that with the emergence of revolutionary new technologies, a wave of entirely new opportunities opens up for small firms, which are sometimes able to exploit such new opportunities more rapidly. These highly innovative small firms are a very small proportion of the total universe of small firms, but they play an exceptionally important role in the early periods of 'paradigm' change in technology. As Kaplinsky's analysis already demonstrated in the case of CAD, the processes of concentration identified by Marx and Schumpeter have not ceased to operate. The debate on innovation and market structure, therefore, points to the importance both of a long-run historical approach and to the importance of cyclical phenomena of birth, growth and maturity in relation both to technologies and industries.

Schumpeter emphasized that the dynamism of a capitalist economy was due to the profits to be made by innovators and, following Marx, he stressed the tendency for these profit margins to be eroded as a result of the diffusion process. How large their profits are, however, and how rapidly they are eroded will depend partly upon the 'appropriability' of the returns from innovation and this in turn is related to such institutional factors as the patent system, industry standards and the taxation regime as well as the costs of imitation and other barriers to entry.

Mansfield, like Rosenberg, Nelson and Winter, has been so prolific in his contributions to innovation research and has pioneered in so many fields that the problem of editing a volume such as this one is to decide what to leave out rather than what to put in. However, the two papers included here are among the more recent contributions and break entirely new ground. His work and that of his colleagues on imitation costs and patents and his later work on 'How rapidly does new industrial technology leak out?' both illuminate important aspects of the diffusion process, hitherto neglected by most empirical research. They complement the work of von Hippel and taken together these three papers and that of Teece go a long way to explaining some of the variations in innovative behaviour in different sectors of the economy recorded by Pavitt. Von Hippel's combination of theoretical reasoning with analyses of the empirical evidence on appropriability offers the basis for a general theory in this area based on the feasibility (or otherwise) of capturing significant benefit from output-embodied knowledge.

Finally, Metcalfe's paper restores the balance in diffusion research between the factors affecting the potential *adopters* and their behaviour and the hitherto relatively neglected *suppliers* of innovation. Taken together with the Mansfield and von Hippel papers we have some of the main elements of a complete diffusion theory. However, our understanding of the selective environment would still be incomplete if it did not take account of the topics discussed in the last two papers by Paul David and by Arthur. Both of these papers demonstrate that any diffusion is a path-dependent process. In general theoretical terms they show that random fluctuations may determine which of several possible alternatives is 'selected', for example, from competing technological systems. Once the selection has identified the leading candidates, there are then very powerful self-reinforcing mechanisms which serve to 'lock out' the alternatives and strengthen the position of the leading contenders. These mechanisms include scale economies of various kinds, increasing returns from learning by users and producers and their inter-action, availability of skills, components, materials and sub-systems.

Contrary to the assumptions of the 'Classical Defence' of optimizing rationality, the system which is 'selected' is not necessarily the 'best' or most efficient system. 'Events early on can lock the system into an inferior technological path'. That this is indeed the case is beautifully

demonstrated by Paul David in the case of QWERTY. Other examples are the US colour TV system and FORTRAN as a programming language. Clearly, standards, whether set *de facto* by leading suppliers or *de jure* by national and international authorities can have a powerful influence on the diffusion path of any product, or process. Clearly, also, a satisfactory approach to diffusion must take into account the inter-action between competing technologies and the inter-action between the various elements of technological *systems*. As both Gold and Metcalfe have argued and as almost all the papers in Parts III and IV have shown, the study of the *individual* innovation, the diffusion of *individual* products or processes, although an essential starting point, is an inadequate basis for the understanding even of those specific innovations. They have to be located in a wider *systems* context. For this reason innovation research has moved increasingly in the 1970s and 1980s in the direction of analysing *patterns* of innovations, *trajectories* of technology and *paradigms* in the development of technological systems. This stream of research is the subject of the final set of papers in Part V.

Again the historical approach is preferred and the section is introduced by Rosenberg's paper 'On technological expectations'. It recalls Gold's analysis of the problems confronting decision-makers *ex ante* when considering whether to adopt a new process. Rosenberg argues that *expectations* about the future direction and pace of technical change play an important part in such decisions. This could lead in some cases to the paradoxical result that the expectations of a major improvement in an innovation could lead to a *delay* in the diffusion process, rather than an acceleration. Waiting may sometimes be the most sensible decision as during the diffusion process it may often happen that modifications are made which better suit the needs of specific groups of users. Rosenberg quotes numerous examples from the history of technology which confirm and illustrate these points, especially in times of radical change from one major technological system to another, as for example from steam to electric power.

Both the papers which follow – by Sahal and by Utterback and Abernathy – represent attempts to identify characteristic patterns of the evolution of technology. Sahal starts from the rejection of either exclusively demand-pull theories of technological development or of pure supply-push theories, maintaining that 'technology both shapes its socioeconomic environment and is in turn shaped by it. Neither is a sole determinant of the other, the two co-determine each other'. He stresses in particular the influence of scale and size on the evolution of technology: 'one of the most important clues to understanding the process of innovation is to be found in the web of links between the functional performance of a technology and its size and structure'. Ultimately, Sahal argues that the process of scaling up (or of miniaturization) reaches limits and that at this time radical innovations are needed to open up new 'avenues of innovation'. Some of those avenues may be so broad that they afford new opportunities in many sectors – recalling Nelson and Winter's 'generalized natural trajectories'.

Sahal's approach, like that of Metcalfe, points to the importance of cyclical phenomena in the growth of industries and technologies and to the importance of *timing* in relation to public policy. Utterback and Abernathy also stress the cyclical path of evolution of technology and their work is of particular interest in relation to the issue of *process* innovations associated with scaling up. It also indicates an important link between theories of evolution of technology and the problems of firm strategies discussed by Teece and others in Part III.

The last two papers in the book are attempts to place the whole discussion of technological

trajectories, patterns of innovation, and the selection environment in the wider context of the evolution of the economic system as a whole. Thus the wheel turns full circle to the discussion in Part I on evolutionary theories of economic change. Many authors (for example, Dosi, 1982) have drawn a parallel between Kuhn's idea of 'paradigms' in the development of science and the evolution of technology. But the notion of a 'techno-economic paradigm', as put forward by Carlota Perez in her paper on micro-electronics and world structural change, has several original distinguishing features.

In the first place her concept of a change in 'techno-economic paradigm' is one of a change in the basic approach of designers, engineers and managers which is so pervasive that it affects almost all industries and sectors of the economy. Secondly, she argues that the *economic* motivation for such a change of paradigm lies not only in the availability of a cluster of radical innovations offering numerous new potential applications, but also in the *universal* and *low cost* availability of a key factor or combination of factor inputs. She suggests that this key factor was cheap *steel* from the 1880s to the 1930s, cheap *oil* from the 1930s to the 1980s, and cheap micro-electronics (chips) at the present time. Finally, she argues that before a new techno-economic paradigm can generate a new wave of world-wide economic growth, there is a period of adaptation of the socio-institutional framework, corresponding to the recession and depression phases of Schumpeter's 'long waves' of economic development. The old institutions were adapted to a now increasingly obsolete technological style. They tend to 'lock out' alternative systems. There is therefore a period of 'mis-match' between the new technology and the old framework. The need for new institutions is perhaps most obvious in relation to education and training, but if affects almost all institutions, including the capital market, standards, proprietary aspects of technology, government regulation of various sectors of the economy, industrial relations, trade union structure and so forth.

The Perez paper therefore offers a link between the cyclical theories of technological evolution advanced by Sahal, Utterback, Abernathy and others and the theories of path-dependency, structural change and 'lock-out' of alternatives put forward by Arthur, David, Dosi and others in earlier sections of the book. My own paper, which concludes this selection, is again an attempt to place the whole debate on change of paradigm in the wider historical context of the evolution of the capitalist system as a whole. It also addresses the fundamental question of the environmental hazards confronting the global economic systems and argues (perhaps over-optimistically) that it is possible to adapt the specific forms of growth to minimize these hazards. This puts the emphasis, as in the Perez papers, on the social and political choices which are made in the regulatory regime governing the use and abuse of technology. It is these choices, together with the continuing advances in fundamental science and technology which will co-determine the future rate and direction of invention and innovation.

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