Jan Fagerberg (2002) *Technology, growth and competitiveness. Selected essays.* Edward Elgar, Cheltenham, UK

Introduction

This book has it origins in a research project that I undertook in the mid 1980s. The project, the main aim of which was to explain why some countries succeed much better than others in generating growth and competitiveness, was supposed to run from 1983 to 1986. Yet somehow it became a never-ending story. The reason for this, I think, had to do with the scope of the project which I now (with hindsight) can see was too large and ambitious, and the fact that I got severely hooked on this type of research and never really wanted to stop. One important output from this research was my Ph.D.-thesis, 'Technology, Growth and Trade – Schumpeterian Perspectives', submitted to the University of Sussex in 1988. Three of the chapters in this book were also included in my thesis (Chapters 7, 9 and 12). In addition I have included thirteen other articles that, in various ways, supplement and extend the analysis that was presented in the thesis. Hence, what is presented here is a far more complete analysis of the relationship between technology, growth and competitiveness than anything I have published to date. Each chapter is self-contained and may be read independently of the other chapters.[1] In this introduction I try to outline the story of how I came to view global dynamics in the way I do and to summarise the main arguments of the subsequent chapters.

How I got started

The idea of undertaking research in this area came to me in the early 1980s when I was working as a civil servant in the Planning Department of the Norwegian Ministry of Finance. Within the department I became responsible for the analysis of international economic trends and Norwegian trade performance. When I started to work on this I was quickly confronted with the view, widely accepted at the time, that the competitiveness of a country (see Chapter 12) could be measured by the unit labour cost of its manufacturing industry relative to that of its trading partners (in a common currency). The argument behind this view seemed to be that, if unit costs go up relative to that of trading partners, this would harm exports and favour imports, with foreseeable negative consequences for growth, employment and welfare. The policy implication of this was of course wage restraint. Heard the story before?

Although the logic of the argument may seem convincing at first glance, it appeared to me that there was a logical flaw in it: it did not take into account quality differences. Higher prices could equally well reflect higher quality, which, in turn, might justify higher wages. From this perspective higher growth in relative unit labour cost (RULC) could just as well be seen as an indicator of growing quality relative to other countries or increasing - rather than deteriorating - competitiveness. In developing this view I was strongly influenced by two people who had thought much more about this issue than myself, namely Francois Chesnais of the Department for Science, Technology and Industry at the Organization for Economic Cooperation and Development (OECD) (whom I had come to know during a visit to the OECD the year before) and Bengt Åke Lundvall at the IKE-group of the University of

[1] The price to be paid for this is that there is some overlapping, especially in cases where articles use the same data source. This applies for instance to Chapters 7-10 in Part II and Chapters 13-14 in Part III which all use the same data base on international trade.

Aalborg.[2] Both of them argued that competitiveness should be analysed as a dynamic phenomenon, that is, in a growth perspective, and that so-called 'non-price factors' (and, hence, 'technology') were equally if not more important than variations in wage-costs and prices. This I found very attractive.

I do not think I convinced anyone in the ministry when I started to question the then common wisdom, but I convinced myself sufficiently to ask the Director of the Department, Per Schreiner, for permission to do a small project on this during the first months of 1982. Little did I know when I started that the report (based mostly on secondary sources) I submitted a few months later would come to have a lasting influence on my professional life. This had to do with a request from a former colleague, Terje Røed Larsen, who at the time was setting up a 'think-tank' for the Federation of the Norwegian Trade Unions, to publish the report as a working paper. I asked Schreiner if he accepted this, which he did. However, he did certainly not foresee (nor did I) that Larsen would use his great entrepreneurial talent (which later brought him fame as a mediator in the Middle East conflict) to persuade a newspaper in Oslo to use most of its front page on my report under the heading 'Research shows that wage costs do not matter for competitiveness'. The fact that wage negotiations between employers and trade unions were underway at the same time (which was hardly a coincidence) increased the public interest enormously, and the alleged findings of my research became one of the main themes in the media for some time. This made my position in the Ministry quite uncomfortable. I was happy, therefore, when, later the same year, I was awarded a three year fellowship from the Norwegian Research Council for the Humanities and Social Sciences (NAVF) to do research on competitiveness.[3]

In mid 1983 I left the Ministry to start my new career as a researcher. The Research Council did not provide an office, so my first task was to find one. Initially, I got one at the Research Department of the Central Bureau of Statistics in Oslo, a large research organisation with a highly experienced staff and an excellent library (from which I received first class service). While there I entered into a very rewarding collaboration with Gunnar Sollie who – like me – was interested in the relationship between structural changes in international trade and trade performance.[4] In 1985 I moved to a tenured position at the (much smaller) Department for International Economics and Development Economics at the Norwegian Institute for International Affairs (NUPI), also in Oslo. There I was equipped with my own computer, excellent library support and was free to pursue my own research interests.[5] I

[2] I had met Bengt-Åke Lundvall a few years earlier through the Nordic Summer University and had in this way became acquainted with the work he and other members of the IKE-group were doing on competitiveness. Chenais' work I became aware of through internal OECD documents that I gained access to through my job in the ministry (Chesnais 1981).

[3] I am indebted to Professor Tor Rødseth, my former supervisor in Bergen, and Professor Gudmund Hernes who backed the application. I knew Hernes, who had previously been Professor of Sociology in Bergen and one of the lead-researchers in 'Maktutredningen' (a research program on the power-structure in Norway), from his time as junior minister ('statssekretær') in the Planning Department. His intellectual and moral support was very important to me, especially in the in the early phase when my ideas were regularly attacked in the media and I, as a public servant, was prevented from defending them publicly.

[4] Gunnar Sollie and I knew each other from Bergen where we both were graduate students in economics. At the Bureau he was working on a project on intra-Nordic trade with which I became partly associated. Together we developed a new method for assessing the effects of structural changes in international trade on trade performance that put more emphasis on the ability of each country to adapt to changing patterns of demand. Our ideas were set out in a paper which was later published in *Applied Economics* (included here as chapter 8). The method was subsequently used by among others, the OECD. Sadly, Gunnar did not live long enough to experience the appreciation of our joint work as he died prematurely (of cancer) shortly before the paper got published.

[5] I am grateful to the people at NUPI who made this possible. This includes Jens Andvig, Valter Angell, Daniel Heradstveit and the Director at the time, the late Johan Jørgen Holst. At NUPI I also received first-class

stayed at NUPI on a full time basis until 1996 (interrupted, though, by several periods abroad) when I moved to my present position at the University of Oslo.

How I came to think differently

Why did I come to view things so differently from my colleagues in the Ministry and most other Norwegian economists? Actually, when I entered university I had no intention of becoming an economist. My wish was to become a journalist. I had already worked in a newspaper for short periods. When I asked the senior editor what he considered to be the best educational background for a journalist, he said that the most efficient education was learning on the job, but that a few years' exposure to the university might be helpful as well. Following this advice I enrolled as a student at the University of Oslo (my home town) with the ambition of taking an undergraduate course in political science. On the completion of this course in 1973 I moved on to history and to the (recently founded) Regional College in Telemark and one year later to economics and to the University of Bergen.[6]

Compared to studying history, a multi-faceted subject full of life, studying economics was like a cold shower. The approach I was presented with appeared overly static in focus and based on a simplistic view of human behaviour. I still remember the first lecture. The lecturer started by making a number of assumptions about human behaviour and similarly for firms from which he no doubt was going to make a number of deductions for the working of markets and the economy in general. However, he never got that far, because he was repeatedly interrupted by students asking him how these assumptions could be justified. Unfortunately, he was never able to answer these questions. As the weeks passed by we learnt that this practice - making far-reaching deductions from ad hoc assumptions with little if any empirical backing - was common in the discipline. Although this did not make anyone any happier, it calmed down the situation. Teaching continued in the usual way, and we[7] on our side concluded that if we were going to learn something about the working of the real economy (in contrast to its textbook version), it would probably have to be outside the classroom.

My 'out of classroom' activities included, among other things, studying classic texts - and interpretations - on political economy and long run economic and social change. I read widely on the subject, including classics such as Adam Smith, David Ricardo and Karl Marx, and interpretations such as those by Marc Blaug, Maurice Dobb, Luigi Pasinetti and Joseph Schumpeter.[8] Through Rune Skarstein, who at that time worked as a researcher at The Christian Michelsen Institute in Bergen, I became familiar with Sraffa's work and the controversy between the neoclassicals and the neo-Ricardians. However, the most central

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assistance from the administrative staff, particularly Eilert Struksnes and Liv Høivik, and the two librarians, Dagfrid Hermansen and Tore Gustavsson.

[6] The Regional College in Telemark was one of several universities/colleges in Norway offering undergraduate history, and I chose it because I had heard that the course was really excellent. I was not disappointed. As for economics I was left with little choice since at the time the University of Bergen was the only Norwegian University offering standard undergraduate courses in economics.

[7] With 'we 'I am referring to a group of students who shared these views. Central in this group were my two friends from Oslo, Trond Erik Seem and Erik Horn, who also enrolled as economics-students, and with whom I shared an apartment close to the university. This apartment soon became a veritable 'hot-house' for critical thinking about economic, social and political issues with study-groups, meetings and informal discussions in the late evenings. It is a sad fact that both Erik and Trond died prematurely, both in their forties. How I miss their company!

[8] Blaug (1958), Dobb (1973), Pasinetti (1974) and Schumpeter (1954). Other interpretations that influenced me a lot at the time include Bleaney (1976) and Shaikh (1978).

authority in these days was of course Marx and I joined a cross-disciplinary seminar on Marx's 'Capital', led by a philosopher, Hans Ebbing, that lasted three full terms. Like many others in those days I started to ask myself to what extent this theory could be of relevance for understanding contemporary economic developments. What I particularly had in mind was the strong economic downturn that the capitalist world was experiencing at the time. Here, Marx had an interesting theory to offer: (technological) competition, he argued, forced capitalists to invest in new machinery. This would in the short run improve the competitiveness of the individual capitalist but have negative repercussions on long-run profitability (the rate of profit) in the economy as a whole. Eventually this would cause investment to shrink and a crisis to appear. Could the depression of the mid 1970s be understood in this light? I wrote a term paper on the issue and became very excited about it.

I left Bergen for Oslo in the summer of 1976 having completed my undergraduate degree. However, the idea of doing more research on the interaction between growth and crisis became more and more irresistible, so I decided to write a master's thesis on the subject. Initially, my idea was to combine a theoretical and historical approach to the subject and major within (economic) history, but the academic in charge of economic history at the History Department of the University of Oslo at the time was not at all enthusiastic about the idea. So I ended up enrolling as a graduate student in economics at my old department in Bergen but, except for the academic year 1979-1980, I continued to live and work in Oslo (where I had a part time job in the Information Department of the Ministry of Environment). However, working part time on a thesis with little, if any, contact with teachers and students was a lonely process, and it would surely have ended in failure had I not come into contact with the Nordic Summer University (NSU), which hosted study groups in a variety of disciplines and themes (with a common Nordic session every summer). Through my participation in the group on political economy I had the opportunity to discuss my research with others and was introduced to many (for me) new topics and approaches. I also got to know many people with whom I have been in close contact to this very day.[9]

I graduated from the University of Bergen in 1980 with a thesis on 'Theories of Growth and Crises within Classical Political Economy'. The thesis consisted of a series of chapters comparing and discussing the contributions from writers such as Smith, Ricardo, Malthus, various underconsumptionists, Marx (in particular), Keynes and post-Keynesian growth theorists (particularly Domar). In my view the possibility for a 'market glut' caused by a mismatch between production and demand was inherent in capitalism and could be provoked for a number of reasons. It was argued that such incidents ought to be distinguished from long periods characterised by slow growth, increasing unemployment and so on, which were more likely to be caused by fundamental imbalances in the economy of the type discussed by Ricardo, Marx and some of the post-Keynesians. To me it seemed as if there was a common ground among these writers in how they looked at long run economic growth. On the one hand they emphasized the immense potential of the capitalist machine (as an engine of economic progress), on the other that this potential could only be realized if matched by appropriate institutions (such as, for instance, property rights, workers' rights, income distribution rules, patterns/routines/rules of competition etc.).[10] Hence, following this view

[9] This includes, for instance, Aadne Cappelen and Lars Mjøset from the local study group in Oslo with whom I have over the years written several papers on Norwegian economic policy. Our first analysis was published (in Norwegian) as Cappelen *et al*. (1984) as part of an NSU-initiated project on economic policy formation in the Nordic area. For more recent analyses in English see Fagerberg *et al.* (1990, 1992). Mjøset (1987) puts the Norwegian experience in a comparative perspective. On the Nordic level I met, among others, Bengt-Åke Lundvall, Esben Sloth Andersen and Bent Dalum from the newly founded Aalborg University Centre in Denmark.

[10] To support this interpretation I will briefly mention three examples of this way of looking at things. The first is Ricardo's discussion of the Corn Laws, in which the prevailing institutional set up (including the rules for

prolonged periods of economic depression might be seen as a structural imbalance between the conditions for economic progress on the one hand and the prevailing institutional system on the other. I found this way of looking at things very attractive.

Searching for a theory of growth and trade

This is the title of an article I wrote during my first year of research (Fagerberg 1984). The reason why I decided to undertake such a search was that I realised that the kind of 'political economy of growth' framework within which I had worked did not provide sufficient guidance for the type of research I wished to undertake. It provided a focus, that growth and competitiveness problems needed to be analysed in a long run, dynamic perspective, but not much more. Hence, I started to search for theoretical and empirical contributions that shared this focus but went beyond the general analyses I had encountered hitherto. I decided to cast the net rather widely and identified at least four different strands that potentially might be of some relevance for the research I wished to undertake, namely:

- neo-Ricardian theory,
- the regulation school,
- post-Keynesian theory,
- neo-Schumpeterian theory.

The search process consisted of a combination of travelling and reading. In the summer of 1983 I went to a summer school in Trieste in Italy. It was a combined neo-Ricardian/post-Keynesian venture with many of the leading figures within these strands lecturing. My inclination was that there was more promise in the post-Keynesian than in the neo-Ricardian strand. This impression was much strengthened during the summer course. However effective Sraffa (1960) may have been as a critic of neo-classical formalism, it occurred to me the attempts to build a positive theory on the basis of his critique were misguided. To me it seemed to be an abstract scheme totally void of any real understanding of human behaviour/interaction and, as such, not at all superior to the neoclassical theorizing that it wished to replace.

The post-Keynesian strand was more promising in these respects since it placed human behaviour (satisficing rather than maximising) and institutions at the core. However, many of the post-Keynesians whom I met in Trieste seemed to focus on a rather limited set of issues, often related to money/monetary policy in one way or another. Important as this may be in itself I found it to be of little help in my research endeavour. However, this does not extend, it should be said, to all researchers carrying this label, at least not if you include writers such as Nicholas Kaldor, John Cornwall and Anthony Thirlwall. Kaldor (1978, 1981) and Thirlwall (1979) were at the time clearly the most well known critics of the established wisdom of competitiveness, and my work on this topic took their arguments as a point of departure (see Chapters 12 and 16). Cornwall's treatise on 'Modern Capitalism' from 1977 is one of the best books on economics I have ever read and it has influenced me a lot. What I found especially attractive was his strong emphasis on growth as transformation, that is, as a

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allocating income across social classes) was seen as incompatible with the productive potential of the capitalist machine. Hence, if institutional change was not allowed, this would – according to Ricardo – lead to stagnation. A second and more far-reaching example of this way of thinking is Marx' distinction between the productive potential of the capitalist machine, which he called 'forces of production', and the surrounding institutional system, which he called 'relations of production'. He argued that in the long run the latter would constrain the former and hence had to be changed. Third and last, I will point to an example form post-Keynesian growth theory, in which it is argued that an institutional system that divides income between workers and capitalists according to certain rules is a prerequisite for long-run growth in capitalist economies (see Pasinetti 1974, p.86-102).

process of qualitative (and structural) change, in which the success (or lack of such) of individual countries to a large extent depends on their ability to transform economic, social and institutional structures.

In the spring of 1984 I went to Paris as a visiting fellow at CEPREMAP for some months to study the regulation approach.[11] I was very well received by both Robert Boyer and Pascal Petit and I benefited greatly from interacting with them.[12] The regulation approach had a lot in common with the kind of approach I had developed in my master's thesis, for example, it combined Marx's dynamic theory of capitalist development with Keynesian and post-Keynesian features. Compared to Marx the regulationists were more specific when describing the productive forces and the corresponding institutional requirements. Furthermore, while Marx wrote about the type of mechanization that characterised the period following the so-called 'industrial revolution', the regulationists focused on the scale-intensive, mass-consumption production system that developed in the United States about a century ago (and which later diffused to Europe) and its assumed institutional counterparts (such as, for instance, collective bargaining). While I found much of this quite sensible, I could not help recognising that the regulationists essentially took the scale-based production system for given, and that there was no explanation of how such systems emerge, change through time, grow (or decline) and so on.

From reading John Cornwall's work I recognised that Schumpeter heavily influenced his theory of growth as transformation, and I therefore decided to read more of Schumpeter's work in the original.[13] Schumpeter, it occurred to me, was writing in the classical tradition from Smith, Ricardo and Marx, but enriched it by adding a much more refined analysis of the role of technology and innovation for long-run economic growth. The core of his argument, that growth is driven by technological competition, was taken from Marx, as Schumpeter himself readily admitted.[14] However, while Marx exclusively focused on investments in new generations of machinery (and hence on process innovation), Schumpeter combined the idea of technological competition with a broader understanding of innovation as a social and cumulative process. Innovation to Schumpeter was 'a new combination' - developed for a commercial purpose - of existing pieces of knowledge drawn from different sources such as, for instance, basic science, engineering, marketing, procurement, management and so on. Hence, following this view every new innovation does to some extent build on – and adds to - the broader 'knowledge base' of society (to use a more recent term). Thus, following Schumpeter innovation is by its very nature a systemic phenomenon.[15]

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[11] See, in particular, Aglietta (1979) and Boyer (1988a)

[12] At the time Robert Boyer was doing a lot of work on formalising this dynamics based to a large extent on Kaldorian ideas (Boyer and Petit 1981, Boyer 1988b). With my background in growth theories of the 'political economy' type I thought it would be fun to try to do the same within such a framework, so I devoted a lot of time on it while there. It proved to be quite complicated but with some good advice I finally succeeded. The paper, which initially circulated as a working paper from CEPREMAP, was finally published as Fagerberg (1991).

[13] This I have never regretted, and I think that there would be much less confusion about Schumpeter (and other classics as well) if people took the time to read the classic texts in original rather than relying on the secondary literature that in many cases, it has to be said, does more harm than good. The three texts I read at the time, all in English, were *Theory of Economic Development* (1934), *Business-Cycles I-II* (1939) and *Capitalism, Socialism and Democracy* (1943). The *Theory of Economic Development* is generally regarded as the basic introduction to Schumpeter's thinking, and deservedly so. *Capitalism, Socialism and Democracy*, by contrast, was clearly the most amusing read. However, although difficult to comprehend at times, I found *Business Cycles* to be very informative, since in that work Schumpeter offered a good deal of analysis and commentary on actual economic developments.

[14] See, e.g., Schumpeter (1943), pp. 31-2 and pp. 82-3 (cited after the fourth edition, 1954)

[15] In my view this both explains and justifies the many recent attempts to introduce system concepts to the analysis of innovation and diffusion such us 'national systems of innovation', 'regional systems of innovation', 'sectoral systems of innovation', 'technological systems' and the like (for overviews and further references see Freeman 1995 and Edquist 1997).

It struck me that the Marx-Schumpeter model of technological competition was potentially a very fruitful framework for analysing not only competition among firms but also relationships among nations. Just as some firms were innovators while others were imitators, some countries stayed at the technological frontier while others lagged behind. It might be objected, though, that to apply an approach developed for firms to countries may be methodologically suspect (although very common in economics). My argument for doing it in this particular case is that countries, regions and firms can all be viewed as innovation systems with sufficient internal coherence (structure) to qualify as analytical units in the model/approach. In fact, economic historians had played with similar ideas for quite some time (see, in particular, Gerschenkron 1962; Abramovitz 1979) and there had also been some attempts by trade economists to analyse the interaction between growth and trade from this perspective (Posner 1961; Vernon 1966). However, much of this work was based on a pure leader-follower dichotomy, and had difficulties in dealing with dynamic aspects such as, for instance, innovation among followers or 'change of technological leadership'. It occurred to me that by taking advantage of the deeper understanding on innovation diffusion offered by Schumpeter this type of work could be substantially sharpened and refined.

One (systemic) aspect that Schumpeter put a good deal of emphasis on is the tendency towards 'clustering' of innovations in time and space. He saw this as related to phenomena such as business cycles and 'long waves'. While studying Schumpeter's account of this I also consulted some of the other contributions on long waves that were published at that time such as Mensch (1979), Maddison (1982), Clark *et al.* (1982) and van Duijn (1983). However, what I considered to be of importance for my research was not so much the exact regularity or causal structure of such 'waves', as the fact that it was possible to distinguish between different 'technological systems' that were dominant at different times and which differed in terms of internal dynamics, conditions of diffusion and institutional requirements. My hunch was that by taking such differences into account it would perhaps be possible to explore better why some countries were more successful than others in certain types of activities and why some countries succeeded much more than others in exploiting the international process of innovation and diffusion to their benefit.

Putting ideas to work

These were some of the ideas I set out to explore.[16] However, although my ideas were to a large extent inspired by the revival of Schumpeterian thinking, I had been in little direct contact with the advocates of the so-called neo-Schumpeterian theory.[17] In October 1985, I joined a group of economic historians from the Norwegian 'technology history project' (with which I had become partly associated) who were going to visit the Science Policy Research Unit (SPRU) in Sussex, UK. During this visit I was lucky enough to get the opportunity to give a staff seminar there. I remember that Christopher (Chris) Freeman, Keith Pavitt and Luc Soete were all present, and that they were very positive both to the main thrust of my arguments and the empirical techniques I had used to support them. I realised that SPRU was probably the best academic environment around at the time for this type of research, and I began to play with the idea of turning the research I had started into a thesis there. I applied to

[16] I develop these ideas in more detail in Fagerberg (1984) and in the introductory chapter of my thesis (Fagerberg 1988a).

[17] My first direct encounter with the proponents of this approach was, I believe, at a Nordic Summer University workshop in Copenhagen in March 1983 in which Chris Freeman and Carlota Perez gave a joint presentation (published as Freeman and Perez 1984). I was struck by the vigour and fruitfulness of their approach.

and was (after some negotiation) accepted as a part time Ph.D. student at SPRU/University of Sussex.

During the following years I had several shorter stays at SPRU, the longest being in the autumn of 1986 when I stayed one full term. During these years I had a lot of fruitful interaction with several members of the staff, particularly Chris Freeman, Keith Pavitt and Nick von Tunzelmann (who became my supervisor). Chris Freeman also invited me to take part in the big IFIAS project, which was then already well under way, and which in 1988 resulted in the volume *Technical Change and Economic Theory* edited by Giovanni Dosi, Christopher Freeman, Richard Nelson, Gerald Silverberg and Luc Soete. This gave me a chance to write about growth theory and its applications (Fagerberg 1988b) and combine it with my own stuff, and in connection with this I received a lot of stimulating feedback from the other participants, particularly Richard Nelson, who (I now realise) obviously knew most of the growth literature much better than I did.

In the same year the IFIAS book (at the time called 'the bible' by younger scholars) was published, my SPRU-days came to an end with the defence of my thesis in the autumn. The thesis, entitled 'Technology, Growth and Trade: Schumpeterian Perspectives', combined a more macro-oriented approach, based on the application of the Marx-Schumpeter model of technological competition to the study of growth and competitiveness, with a more sectoral focus exploring the changing competition of international trade, its causes and the ability of individual countries to face the challenge. Hence, in contrast to my master's thesis, which ended up as being purely theoretical, the doctoral thesis had a clear empirical basis, drawing on a variety of data sources (including, for instance, national accounts, trade data,[18] R&D data and patent statistics). Many of those data were not easily available and a lot of time had been spent on searching for relevant data and transforming them to a format suitable for my purpose.

The perhaps most important achievement was the formulation of a Schumpeterian growth model ('technology gap model') that could be extended to take into account trade and competitiveness as well and its subsequent application to post-war data for a sample of countries on different levels of development (see, in particular, Chapters 1 and 12 and the appendix on p. 287 in this volume). Following Schumpeter, the model assumes that both innovation and diffusion of technology fuel economic growth. However, in contrast to much previous work in this area, it was emphasised that innovation is not something that occurs only in the technologically leading country, from which it diffuses to other countries at different speeds. Innovation, it was argued, is a pervasive phenomenon in contemporary capitalism, and may be an important source of growth in poorer countries as well (though not necessarily in the same proportion as in a frontier nation).[19] Moreover, it was emphasized that successful imitation (or diffusion) often involves innovation as well, and that innovation and imitation generally draw on many of the same capabilities or resources. R&D capabilities, for instance, were shown to be crucial for both innovation and imitation.

Thus 'catch-up' is not something that comes easy (as traditional neoclassical theory used to suggest, see chapter 6). Successful catch-up, it was shown, normally involves both an extensive upgrading of the technological infrastructure and a rapid transformation of the composition of output and international trade (see chapters 7-9 and 11). Arguably, the historical evidence tells us that policy has had an important role to play in such processes. The

[18] The trade data used in this book are mostly drawn from a database constructed by Bent Dalum, Aalborg University Centre and myself on the basis of trade data supplied by the OECD (on tape). I am indebted to Bent for his help and encouragement in using trade data in analyses of structural change and competitiveness. Without his assistance the analysis presented here would certainly been less rich.

[19] This implies, of course, that knowledge is conceived quite differently from the traditional public good approach of neoclassical economics. For an extended discussion see Chapter 6 and Fagerberg (1994).

most clear-cut examples of this were of course Japan and the Asian NICs (Korea and Taiwan in particular). This evidence remains a matter of considerable controversy, however. For instance, the role of policy in the catch-up of Japan and the Asian NICs has been disputed by the World Bank in its study of the 'The East-Asian Miracle' (World Bank 1993). Aadne Cappelen and I discuss these issues in some detail in Chapter 3 of this book.

As argued above the 'technology gap model of economic growth' is not only applicable to the question of 'why growth rates differ' across countries, but can be used to analyse differences in performance among other entities as well. What matters is that it is possible to identify a 'system of innovation' with sufficient internal structure and coherence to qualify as a unit in the analysis. It has been suggested that such systems are more likely to be found at the regional than the national level, particularly in the case of the larger economies (Braczyk *et al.*, 1998). Chapters 5-6, which result from my close collaboration during the 1990s with Bart Verspagen,[20] put this idea into practice by applying the technology gap model to the growth performance of European regions. As suggested by the theory, the potential for diffusion and R&D capabilities were found to be important factors behind the observed growth differences across European regions. However, the outcome was strongly affected by structural and social factors, such as a large agricultural sector and high long-term unemployment, which were shown to contribute to the failure of many poorer regions to fully exploit the potential for diffusion.

Refining the analysis

After the defence of my doctoral thesis I thought a lot about publishing it as a book but I never came that far. There were two reasons for this. First, I felt that more research was needed in order to arrive at a really comprehensive understanding of the relationship between technology, growth and competitiveness. In particular, I felt that although the thesis to some extent dealt with the consequences of the changing structure of international trade for countries with different patterns of specialization, and the role of trade (and specialization) as a medium for 'catching up', it did too little in terms of explaining how specialization was generated, sustained (or altered) and what the long-run consequences might be. Second, at that time there was a lot happening in this area on the theoretical front, such as the advent of the 'new growth theory', and I wanted some time to consider the possible consequences of this for my work.

The first opportunity to do more research on specialisation came while I was working (lecturing) in Aalborg during the academic year 1987-1988. The IKE-group was at the time under some pressure from fellow economists in Denmark with less heterodox inclinations. One of the suggestions that came up in the internal discussions within IKE, and which I strongly supported, was to do a book in English that presented their research for an international audience. In the end what came out of this was the highly influential volume on *National Systems of Innovation* edited by Bengt-Åke Lundvall (Lundvall *et al.* 1992).

[20] I got to know Bart Verspagen when he was as a Ph.D. student in Maastricht towards the end of the 1980s and was struck by the similarity of our approaches. At the time I was acting as Head of the Department of International Economics and Development Economics at NUPI and I invited Bart to come to NUPI as a guest researcher and stay with us for a few weeks. Bart had, as part of his Ph.D. thesis, developed a non-linear version of the technology gap model that allowed for both 'catching up' and 'falling behind' and which impressed me (Verspagen 1991). This was the start of a very rewarding relationship. In the early/mid 1990s we started on Bart's suggestion to do joint work on European regions. Our common interest in the relationship between technology and growth has also resulted several other papers (see, Fagerberg and Verspagen 2000 and Chapter 2 in this volume). We have also edited two books (Fagerberg *et al.* 1994, 1999) and have organised several workshops and conferences together.

Building on earlier work by Lundvall (1985, 1988) and Andersen *et al.* (1981) I contributed a study on the relationship between learning-based advantages, acquired through domestic user-producer interaction, and the international competitiveness of countries. In fact, I found the impact of such learning on competitiveness to be quite substantial. A revised and extended version of that study is included here as Chapter 13.

However, specialization is not only affected by local learning, important as it may be. It also has to do with differences in competitive conditions across sectors and industries, cross-country differences in capabilities, resources, market size and so on, and the willingness of firms and governments to devote resources to R&D. I include two papers written during the first half of the 1990s which explore these issues in some detail (chapters 14 and 15). The results presented in these two papers emphasize the central role played by technological capability and R&D for international competitiveness in a whole range of industries. This clearly illustrates the crucial fact that innovation is a pervasive phenomenon that matters in most industries/sectors, although the forms it takes and the sources it depends on may, as pointed out by Pavitt (1984, 1988), differ considerably. A particular issue that is explored in these two chapters, and in a different manner in chapters 9-10 as well, is to what extent small countries are at disadvantage in so-called 'high-tech' (R&D intensive) industries, as some neoclassical theorising in this area indeed suggests.[21] This is, of course, an important question for policy-makers, particularly in small countries. The research reported here indicates that there are some industries for which large domestic markets appear to be an important competitive factor and where firms from small countries consequently may face some additional constraints. However, these constraints often tend to be of a rather 'soft' character and may in many cases be overcome through appropriate strategies and/or policies. Arguably, this is an area where policy really matters.[22]

That innovation is a pervasive phenomenon in modern capitalism does of course not imply that all innovations are of equal importance. On the contrary, as pointed out already by Schumpeter, some clusters of innovations (or technological systems) may have a very large impact, what Chris Freeman calls 'technological revolutions' (Freeman and Perez 1984), while others are fairly marginal. The pay-offs to growth are clearly larger in the former case than in the latter. However, the nature of such pay-offs change through time as new technologies develop and expand, while older technologies become progressively less important. It follows that a pattern of specialization that has been very favourable for growth in the past may under changed circumstances have lost much its dynamism. Illustrations of such changes can be found in chapters 7-9, which analyse the structural changes in OECD trade and its impact on specific countries, and in Chapter 11 for the European Union as a whole.

In the mid 1990s the European Union launched its Targeted Socio-Economic Research Program (TSER), the first large scale European research program in this area. Like many others Bart Verspagen and I thought that I might be fun to take part in this new activity. One of the topics that we singled out for research was the relationship between technology, specialization and growth in Europe over the long term. We started to contact others and soon found ourselves at the centre of a large network of enthusiastic people. Some of the participants were also part of other networks that considered applying to the TSER program. So why not join forces? In that way the project grew very large with more than fifty

[21] The logic behind this assertion is the simple one that when there are increasing returns to scale, as is commonly assumed to be the case in these industries, unit costs are a decreasing function of the volume of production. Under such circumstances one might expect that firms with access to large markets would outperform those who operate in smaller markets. The argument is a tricky one, however, because the outcome depends on 'market access' which is notoriously difficult to define and measure in open economies.

[22] See, in particular, the case study on electronics presented in Chapter 10.

researchers involved, and Bart - who became the coordinator of the project (labelled 'Technology, Economic Integration and Social Cohesion', TEIS) – got a hard time trying to keep everything on track. The project soon divided into three working groups, one of these coordinated by me. One of the main outputs of that work was the book *The Economic Challenge for Europe: Adapting to Innovation Based Growth*, edited by Paolo Guerrieri, Bart Verspagen and myself (Fagerberg *et al.* 1999), a summary of which is included here as chapter 11.

It is argued in Chapter 11 that the technological dynamics in the capitalist world has changed considerably in recent decades. In most of the post-war period growth in Europe has been based on the scale-based technological system that originated in the USA around a century ago (and later diffused to Europe). European policies (including integration efforts) have to a large extent been geared to getting the maximum out of this potential. However, more recently the scale-based technological system has been replaced by a scienc-based one with ICT[23] as the most important part as the main engine of growth in the capitalist world. These changes, it is suggested, call for a reassessment of many established practices, institutions and policies. The chapter discusses how policy may be adjusted in order to get the most out of the current technological dynamics.[24]

A broader view?

The opportunity to place my own research in a broader setting came in 1991 with a request from Keith Smith of the STEP-group in Oslo to write an overview paper on technology and growth for Norwegian policy makers.[25] When I sent a draft of the paper to Moses (Moe) Abramovitz, with whom I had been in contact for some time,[26] he reacted by inviting me to submit a substantially revised and expanded version to *The Journal of Economic Literature* (of which he was one of the editors). I was of course very pleased with this suggestion, not the least because it gave me an opportunity to work with Moe, whose work on 'catching up' had been an important source of inspiration for me in my own research. The paper, which was finalized during my stay at the IRPS (Graduate School for International Relations and Pacific Studies),[27] University of California San Diego during the academic year 1993-94, went through several rounds of revisions and was finally published in September 1994 (Fagerberg 1994). Moe's many insightful comments and suggestions substantially improved the content and exposition and contributed to a broadening of my understanding of processes of economic growth and 'catching up' processes. My understanding of the latter was also improved by my stay at the IRPS, which allowed me to follow lectures on Asian catch-up by area-specialists

[23] There is a strong and very robust correlation between high growth in manufacturing productivity growth and an increasing presence in electronics broadly defined. See Fagerberg (2000) and Chapter 2 in this volume.
[24] For an analysis of how the increasing importance of ICT technologies affects global competition and the competitiveness of countries see Meliciani (2001).
[25] This paper was published as a STEP working paper (8/92). An English version was presented to the International Joseph A. Schumpeter Society in Kyoto in August 1992 and published in *Evolutionary Economics* a few years later (Fagerberg 1995).
[26] I received a letter from Moe, whom I did not know at the time, in 1988, expressing strong interest for my research. He rightly saw it as a continuation and extension of his own work on catching up. Thereafter, I continued to send him papers, and on several occasions he replied with comments and encouragements. In 1993 I had the pleasure to receive him in Norway at a conference in part organised by me. The proceedings, including a paper by Moe, were published as *Fagerberg et al.* 1994.
[27] I am grateful to IRPS and to the Dean at that time, Peter Gourevitch, for inviting me and providing me with such excellent working facilities.

such as Chalmers Johnson, Stephan Haggard and others (and to read their accounts of these processes[28]).

When I started to do research in the mid 1980s mainstream growth theory had been a stagnant field for decades, and was no longer able – if it ever had been - to generate a meaningful agenda for applied research. What happened during the 1970s and 1980s was that this agenda was taken over by more heterodox writers, many of whom were inspired by Schumpeterian ideas, my own work being no exception. However I recognised from the reading I then undertook that there were important changes underway in the way mainstream looked at growth. Some of it, it should be said, was not so novel, as it based itself entirely on the idea of 'learning by doing' as a source of growth, an idea that had been widespread among growth theorists at least since the early 1960s. Yet it seemed to me that the approach suggested by Paul Romer in his seminal paper from 1990 (Romer 1990) made a real difference in this regard, since in that paper he incorporated the Schumpeterian idea of R&D-based innovation as the prime source of growth.[29] Hence, what in my view had happened was that at least some mainstream economists adopted parts of the Schumpeterian research agenda. This seemed to me to be a very positive development, as it might open up, I hoped, for more promising discussions between economists with different theoretical inclinations and give new and more meaningful directions for empirical research in this area.

With hindsight, I may have been slightly over-optimistic. Rather than a more open-minded scholarly discourse, what we primarily have got is a swarm of imitators (to use a Schumpeterian term) trying to get academic credit by making relatively marginal changes and refinements in the formal models suggested by Romer and others, often without much concern for the validity of the assumptions they have to make in order to arrive at the desired results. In that respect, much of the recent revival of formal growth theorizing is no better than the kind of economics I was encountered with when I first enrolled as a student in the 1970s. There may be more gains in the applied area but these have taken long to materialise and are (as shown in chapter 6 below) still relatively modest. In fact, the overwhelming majority of applied research that followed in the wake of the first 'new growth' models consisted of testing so-called conditional convergence models (also called 'Barro-equations'). This represents nothing new since similar exercises have been frequent in the applied literature on growth and development since the early 1970s. The reason for this is of course that such empirical tests can be made consistent with different theoretical perspectives. Hence, without further qualifications, such tests are not well suited for discriminating between different theories or increasing our understanding of the growth-inducing mechanisms pointed to by the new growth theories. This is not to say there is no empirical work available that can be used to shed light on the relationship between innovation and growth. In fact, there is by now a lot of work on issues such as science policy, research policy, innovation activities, diffusion of innovations and so on published in books and specialised journals such as *Research Policy*. However, most of this has been going on for a long time and is not specifically related to the new growth theories.

Maybe I expected too much. Perhaps I was so enchanted by the fact that someone in the neoclassical camp finally took innovation and diffusion of technology seriously, that I forgot to ask the crucial question to what extent the way they modelled it was really adequate. In part, this was only natural since the new growth theory of Paul Romer and others and the 'technology gap theory' that I had used as a framework for my own research had a lot in

[28] See, in particular, Johnson (1982), which I really enjoyed reading. I also learnt a lot from the accounts by Amsden (1989), Haggard (1990) and Wade (1990).

[29] Since the publication of Romer (1990) there have been many new theoretical contributions in this area; see Aghion and Howitt (1998) for an extended account. I will not discuss these further here since I intend to focus on arguments that appear to be common to all R&D-based new growth models.

common. Both constructs focused on the interaction between innovation and diffusion of technology as the prime source of growth, and in both cases diffusion – or 'spillovers' in the new growth framework – played a major role. There were, however, also some important differences. While the new growth theory is a typical formal theory, that is, ' an abstract structure set up to enable one to explore, find and check logical connections' (Nelson, 1994 p. 292-3), the technology gap theory is much closer to what Nelson has called 'appreciative theorising', that is, theorising close to the empirical substance which attempts to identify, interpret and understand important empirical relationships without necessarily putting the insights thus obtained into a formal (mathematical) model. Hence, the new growth theory is in a (formal) theoretical sense much more ambitious than the technology gap theory since the former insists on explaining both innovation and diffusion of technology – as well as their mutual interactions – within a unified, mathematical framework.

So, how is innovation explained within the type of new growth theories discussed here? The answer is simple: innovation is just like any other activity. In principle there is no difference between innovation and, say, shoe repair. Both activities are assumed to be carried out by profit-seeking entrepreneurs endowed with perfect knowledge and foresight about all factors relevant for their activities. The reason why the former activity is more conducive to growth than the latter has nothing to do with the activity in itself but with so-called externalities (or 'spillovers') that are assumed to be more frequent in the former case. Is this a valid understanding of how innovation occurs? It was certainly not Schumpeter's view as he described it in his *Theory of Economic Development*. For Schumpeter, innovation was something special, and it required much more than ordinary economic activities. He was careful to distinguish between innovators, which he saw as driven essentially by the joy of creating, a competitive spirit (as in sports) and the pleasure of having success, and the surrounding managerial and financial infrastructure for which standard profit-oriented behaviour could perhaps be assumed. Since the motives of innovators are complex, and only partially related to economic gains, he held it as likely that innovation in principle could thrive under quite different institutional 'arrangements'. The answer to the question of what such arrangements might look like could only be found, he argued, 'by detailed observation of the psychology of entrepreneurial activity (…) for given times and places' (Schumpeter 1934, p. 94). Later in life Schumpeter acknowledged that organised R&D within large firms was one such arrangement (Schumpeter 1943), although he did not go very far in analysing the nature of innovation within this (for him) new setting.[30] However, introducing the organizational aspect arguably adds to (rather than reduces) the complexity of the phenomenon under study, as testified by the rapidly increasing literature on how the organisation of innovation activities differs across nations, sectors, industries and firms.[31]

Does this difference in views really matter? Yes it does. Take for instance the design of policies in this area. If you believe in the neoclassical story, this is only a question of providing the right economic incentives to profit-seeking entrepreneurs. If for some reason the government wants to increase innovative activity, what it has to do is to raise the rewards to it by, say, improving intellectual property rights, providing subsidies and so forth. However, if the motivations of innovators are more complex and dependent on, say, organisational, institutional or cultural contexts or factors, such an 'economistic' approach to policy may totally fail to generate the warranted results.

This is where I would like to end this exposition. To explore what shapes innovation in firms, organisations and society at large requires both empirical investigations and theoretical guidance and interpretation. It should come as no surprise that new growth theorising, based

[30] See, in particular, Schumpeter (1943), ch. XII, pp. 131-4. He thought that this might ease innovation and, hence, increase its speed considerably, but did not analyse the issue in depth.
[31] See, for instance, Malerba and Orsenigi (1997) and Lam (2000).

as it is on a simplistic and one-side view on how innovation occurs, has failed to provide a guide to the development of a really new agenda for applied research in this area. To do that, I will argue, one needs a broader view that draws on insights from not only economics but other disciplines as well, such as – for instance – history, business studies, sociology and cognitive science. Arguably, the extreme division and specialization of the social sciences that we have seen developing in the post-war period may have diverted attention from important factors behind long-run economic development that, like innovation, cannot be properly understood from a narrow disciplinary perspective.

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