Apresentação

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Chris Freeman foi o primeiro professor que tive quando cheguei ao Science Policy Research Unit (SPRU), em agosto de 1981, para cursar o doutorado. Entretanto, a disciplina que eu fazia com ele, intitulada *History, Philosophy and Perspectives of Science*, não era em nível de pós-graduação, mas sim oferecida aos estudantes de primeiro ano de graduação em ciências – química, física e biologia – da Universidade de Sussex, Inglaterra. Durante duas horas por semana, Chris Freeman falava a um auditório com mais de 200 alunos sobre as relações entre ciência e sociedade ao longo da história e em como pensava que seria possível construir essa relação no futuro, de modo que o mundo se tornasse mais justo e um lugar melhor para se viver. As aulas de graduação de Chris eram famosas na Universidade de Sussex, atraindo todos os estudantes de doutorado que chegavam ao SPRU. Logo entendi a razão para essa fama: a qualidade das suas aulas, a incrível combinação de erudição com total ausência de arrogância e a capacidade de ilustrar argumentos cristalinos com casos do mundo "real" e corrente prendiam a atenção e o interesse de todos. Em meio a silêncio absoluto e sem qualquer tipo de nota ou dispositivo, Chris expunha a narrativa que havia resultado de suas reflexões a respeito da extensa e variada literatura sobre os temas tratados, assim como de suas próprias pesquisas. Nesse processo, ficava clara sua posição antielitista no sentido estrito e amplo do termo, algo que certamente fazia parte de suas convicções políticas.

Mais tarde, identifiquei estas mesmas características nas aulas de pós-graduação que Chris ministrava, assim como nos inúmeros seminários e conferências dele que tive o privilégio de presenciar. Entretanto, foi nas oportunidades em que não era o protagonista que ele mais me surpreendeu: Chris tinha essa incrível capacidade de ser aberto e igual com todos, de destacar os pontos positivos da fala e do trabalho de todos, mas particularmente dos estudantes e dos pesquisadores jovens. Sempre me vem à memória o dia em que ele se dirigiu a mim na hora do chá e perguntou, para meu desespero, sobre o que era minha tese. Essa total simplicidade no modo de comportamento e a atenção preferencial aos alunos marcaram profundamente a visão que tenho até hoje de Chris Freeman.

Como alguém que foi para o SPRU na tentativa de incorporar referenciais que pudessem ser aplicados no dia-a-dia de um Conselho de pesquisa como o CNPq, eu notava, com grande satisfação, que Chris defendia a noção de que a pesquisa é uma forma de intervenção, cujo objetivo não é apenas compreender o mundo, mas também mudá-lo, revelando sua aderência à ideia marxista de que é possível combinar teoria e prática. Neste aspecto, como se sabe, ele foi grandemente influenciado pelo trabalho de JD Bernal sobre a natureza da ciência e das ciências sociais.

O foco de Chris na intervenção se revela também na sua decisão de criar uma unidade preocupada em gerar subsídio para a tomada de decisão em Política Científica e Tecnológica, e não uma que apenas realizasse estudos sobre C&T. Chris instituiu, modelou e, por muitos anos – de 1966 a 1982, tendo se aposentado formalmente em 1986 –, dirigiu a Science Policy Research Unit, que, nas décadas de 1970 e 1980, era a principal instituição neste campo no mundo. O SPRU serviu de referência para várias unidades nos mais diferentes países, inclusive para o nosso Departamento de Política Científica e Tecnológica da Unicamp. Nesse processo de institucionalizar a área de estudos sobre pesquisa e inovação e política, Chris também fundou e editou por 30 anos a revista *Research Policy*, estabelecendo-a como o principal periódico da área.

O final dos anos 1970 e a década de 1980 foram tempos difíceis na Europa em geral, mas instigantes no SPRU em particular. Eram muitas as questões controvertidas naquele momento envolvendo as relações entre ciência, tecnologia e sociedade. Uma delas dizia respeito às explicações e medidas para reverter o desemprego crescente em todos os países europeus e era importante entender as razões e origem do problema. A sabedoria convencional e um sem número de estudiosos atribuíam uma "culpabilidade" pelo desemprego à inovação tecnológica, o que era ferozmente contestado por Freeman, que argumentava a ocorrência de uma quebra estrutural no crescimento do pleno emprego.

Muitas ideias novas sobre produção de conhecimento e inovação tecnológica estavam sendo desenvolvidas por Chris e outros pesquisadores que se encontravam no SPRU naquele período. Logo nas primeiras semanas de aula em 1981, em um seminário interno, o prof. Chris Freeman apresentou a teoria que estava desenvolvendo em parceria com Carlota Perez sobre ciclos de crescimento econômico que se iniciam a partir de inovações radicais difundidas na economia, em que tomava emprestadas algumas ideias de Schumpeter e dos ciclos de Kondratiev. Outros pesquisadores que são hoje reconhecidos como "pioneiros" da moderna economia da tecnologia também estavam desenvolvendo seus trabalhos no SPRU naquela época, tal como Giovanni Dosi, um doutorando considerado brilhante que havia aplicado o conceito de paradigma científico desenvolvido por Thomas Khun à inovação tecnológica na indústria. Dosi, na verdade, conseguiu publicar sua ideia central em 1982 (Dosi, G. Technological paradigms and technological trajectories: a suggested interpretation of the determinants and directions of technical change**,** *Research Policy 11(3): 147-162)*, antes que Chris publicasse sua noção de sistema nacional de inovação (ainda que as ideias de ambos sejam complementares e não concorrentes, o fato de Dosi ter publicado e Freeman ainda não foi comentário nos corredores do SPRU naquela época e, de certa forma, revela características de cada um).

O mais relevante aqui é que o fato de que ideias novas e revolucionárias estavam em gestação na instituição no início da década de 1980, quando lá cheguei. Ainda que eu, na época, não tivesse dimensão do que aquela atividade intelectual toda significaria para a área de estudos da C&T, vivi e estudei neste ambiente efervescente e acompanhei os debates nos seminários semanais, o que deixou marcas profundas na minha formação. Usando uma metalinguagem, pode-se dizer que testemunhei o surgimento de novos paradigmas tanto em economia da inovação quanto em sociologia da ciência, e talvez por isso, hoje, sou tão adepta dos estudos de controvérsia em C&T. Mais que tudo, o que ficou claro para mim, e já desde os tempos de doutoranda, é que concepções diferentes sobre como se dá a produção de conhecimento científico e sua incorporação nas atividades de inovação tecnológica informam políticas diferenciadas de C&T. E é exatamente desta questão, ou controvérsia, que trata o artigo de Freeman publicado neste número da *Revista Brasileira de Inovação*.

Chris apresenta a controvérsia sobre os determinantes da direção e escala da atividade científica e inventiva nas sociedades industriais contemporâneas que, em meados da década de 1970, enchia páginas das principais revistas acadêmicas da área. Os proponentes da visão até então dominante e conhecida como *science push* ou "teoria ofertista" argumentavam que são os resultados da pesquisa básica que permitem, em última instância, mudanças nos processos de produção e geram novos produtos. Já os oponentes defendiam a posição de que era o mercado a principal fonte de influência para a atividade inventiva, razão pela qual essa visão é referida como *demand pull*.

Vários autores escreveram sobre este debate, mas Chris Freeman foi provavelmente o único que, já na década de 1970, percebeu e apontou duas importantes características das controvérsias técnico-científicas, em geral, e desta, em particular. A primeira diz respeito ao papel dos interesses dos grupos sociais relevantes no alinhamento destes aos diferentes lados do debate. Neste sentido, ele aponta que não é de se surpreender que estudos coordenados pela *National Science Foundation* tenham encontrado resultados que reforçam a ideia de *science push*, enquanto aqueles desenhados pelos departamentos de governo orientados para missão encontraram evidências para a demanda de mercado. Mais significativa ainda é a conclusão de Chris de que não há manipulação de resultados de nenhum dos lados, mas sim a constatação de que interesses modelam as perguntas que são feitas, a maneira de buscar informações para responder a elas, assim como a interpretação dos resultados encontrados. Tal percepção de Chris é típica dos estudos sociológicos modernos da ciência e evidencia a abrangência de sua análise interdisciplinar.

A segunda observação peculiar de Chris sobre a controvérsia em questão refere-se ao poder de persuasão dos dados quantitativos na nossa cultura científica. Ele argumenta que uma das razões pelas quais os defensores da visão *demand pull* estavam ganhando terreno no debate era porque os estudos que realizavam tinham uma base empírica quantitativa que lhes conferia uma "aparência de [...] apoio estatístico". E isto, segundo ele, a despeito de que "poucos destes estudos apontavam, de maneira não ambígua para a conclusão simples de que a demanda do mercado é a única ou mesmo a principal determinante da escala e da direção da atividade inventiva ou inovativa" (p. 207). Assim, Chris antecipou uma crítica metodológica hoje comum nos estudos sociais da C&T de que resultados quantitativos de pesquisas diferentes, produzidos por bases de dados diferentes e até mesmo incompatíveis quanto às premissas conceituais, são frequentemente citados em conjunto, como se a somatória deles fornecesse evidência sólida e fosse estatisticamente válida.

Situada a controvérsia, Chris Freeman defende o argumento de que as coisas são muito mais complexas do que cada uma das proposições consegue captar. Para dar conta desta complexidade, ele apresenta resultados empíricos que combinam séries históricas de estatísticas de patentes, e séries paralelas de estatísticas de publicação de artigos científicos em diferentes países. A análise revela a fragilidade explicativa de ambos os lados da controvérsia, levando-o a concluir que "a interação entre ciência, tecnologia e economia varia na sua natureza e intensidade com o tempo e entre diferentes indústrias. [...] Isto torna mais difícil qualquer previsão, já que não existe escapatória para a tarefa extremamente complexa de combinar previsão social com tecnológica e nem se pode evitar reconhecer as limitações das técnicas econométricas extrapolativas" (p. 215). Chris Freeman, na década de 1970 – como os novos sociólogos da ciência e da tecnologia hoje –, reconhece que "o acaso (ou as contingências, como preferem os novos sociólogos da ciência) tem um papel muito maior na sobrevivência e no crescimento competitivo do que é confortável admitir" (p. 206)

Em suma, estão delineados no artigo que se apresenta os primórdios do que viria a se constituir em uma das mais influentes contribuições intelectuais de Chris Freeman – a noção de sistemas de inovação. Na visão de Chris, novas tecnologias não são invenções isoladas – elas envolvem um conjunto de inovações tecnológicas e organizacionais inter-relacionadas. Já neste artigo, ele se refere a todos os elementos – empresas, universidades e outros atores, juntamente com tradições, conhecimento (expertise) acumulado e contexto político – que produzem mudança técnica em cada economia nacional. Esta conclusão de Chris Freeman é alentadora para os tomadores de decisão, já que permite um amplo espaço para intervenção, que é exatamente a razão pela qual ele estudava as atividades científicas e inventivas.

O melhor tributo que podemos prestar a Chris Freeman é, parafraseando o que diz neste artigo com respeito a Schmookler, dedicar-nos a ampliar as fronteiras dos estudos de inovação com o mesmo nível de honestidade intelectual, humildade e senso de justiça que ele incorporava.

THE DETERMINANTS OF INNOVATION

Market demand, technology, and the response to social problems

Christopher Freeman

Market demand is not necessarily the sole, or even the principal, determinant of the scale and direction of inventive and innovative activity—still less of scientific activity. Recent research shows that the influence of the market may vary greatly, with cyclic changes (birth, growth, and decline) and discontinuities in industry. In addition, chance plays a far greater role in competitive survival and growth than it is comfortable to admit.

CONTROVERSY still surrounds the determinants of the direction and scale of inventive and scientific activity in contemporary industrialised societies. Participants in the debate have included economists, sociologists, and scientific administrators as well as engineers, inventors, and scientists. A related but more specialised controversy has raged among historians of science, between 'internalists' and 'externalists'.

Crudely speaking, there have been two poles to this rather confused and ill-structured debate. These poles are usually characterised as 'demand pull' and 'science push' (or sometimes 'technology push'). Proponents of the demand theory cite evidence to support their case that market demand is the dominant influence on inventive activity (and in some versions, even on scientific activity). Proponents of the science-push, or supply, theory hold that it is changes on the supply side, ie internal developments within science and technology, which determine (or in weaker versions, permit) changes in the composition of output and the way in which it is produced.

As with the analogous cost-push and demand-pull theories of inflation in economics, it is quite possible to subscribe, at least partially, to both theories.

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0016-3287/79/030206-10 $02·00 FUTURES June 1979

Most of those involved in the innovation debate are 'hybrid' participants, or would at least disclaim the more extreme viewpoints. Nevertheless, some major contributors are clearly identified with one or other pole in the debate.

As with the debate on inflation, this is by no means a purely hair-splitting academic controversy. It raises major issues of contemporary policy towards science and technology, both in industry and in government. Consequently it is not surprising that some of those most actively involved in the discussion have themselves had responsibility for decision making. Nor would it surprise a sociologist of science to find that studies sponsored by agencies responsible for basic science (such as the US National Science Foundation) have come up with findings which by and large might justify a science-push interpretation; whilst studies sponsored by mission-oriented departments (such as the US Department of Defense) have come up with rather a different emphasis.

This does not necessarily justify a cynical view of the debate, although it does provide additional evidence of the extent to which interest groups may influence the choice of subject matter and the emphasis of interpretation. The complexity of the issues and the paucity of the information base mean that it is quite legitimate, as in many branches of social science, to entertain the possibility of several interpretations until further evidence shows more conclusively which of them (if any) is correct.

Evidence favours the demand school

However, during the 1960s it became fashionable to assume that the debate was over and that it had ended in a clear victory on points, if not a knock-out, for the demand school. This tendency was nourished above all by Schmookler's book,[1] but also by the findings of a series of empirical studies of industrial innovation, particularly in the USA and the UK.[2] In fact, as Mowery and Rosenberg have recently shown,[3] few if any of these studies point unambiguously to the simple conclusion that market demand is the sole, or even the principal, determinant of the scale and direction of inventive or innovative activity—still less of scientific activity. Nevertheless, the results are often cited as though they did justify such a simple interpretation, and for this some of the authors of these studies must take part of the responsibility.

One of the reasons that these studies were so influential in strengthening the demand school of interpretation was that they gave an appearance, for the first time, of quantitative, statistical support for this viewpoint—which previously had been argued mainly on purely logical theoretical grounds or on the basis of individual case studies or anecdotes.[4] The studies were in fact very different in scope and methodology, and none of them made any claims to be statistically representative. Nevertheless, they were frequently (if unjustifiably) jointly cited as providing firm evidence that demand was the mother of innovation, if not of invention.

The influence of Schmookler

Some confusion has been caused in this debate by the failure to distinguish clearly between 'need' and 'market demand'. However, these criticisms cannot be sustained in considering the work of Schmookler.[1] He dealt with the influence of market demand on *invention* and took great pains to clarify his methods and definitions. Indeed, of all these studies, Schmookler's book was by far the most scholarly and probably the most influential, at least within the economics profession. It represented his life's work—years of patient research with patent statistics. His own premature death, so soon after the publication of his major book, was a tragic loss and greatly diminished the quality of the subsequent debate. No economist or historian before or since has had the patience or the imagination to make use of patent statistics in the interpretation of long-term trends in economic development and technical change with anything approaching the same success.

Schmookler summarised his own conclusions as follows:[5]

> the most striking and most significant result of the entire study . . . concerns the relation of capital-goods output to the number of capital-goods inventions. The relation is evident in time series involving a single industry, and in cross sections relating to several industries. When time series of investment (or capital-goods output) and the number of capital-goods inventions are compared for a single industry, both the long-term trend and the long swings exhibit great similarities, with the notable difference that lower turning points in major cycles or long swings generally occur in capital-goods sales before they do so in capital-goods patents.
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> The possibility that the results reflect the effect of capital-goods inventions on capital-goods sales is grossly implausible. In the time series comparisons, trend turning points tend to occur in sales before they do in patents and long swing troughs in sales generally precede those in patents. Moreover, trends and long swings in investment in the industries examined are adequately explained on other grounds.
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> The fact that inventions are usually made because men want to solve economic problems or capitalise on economic opportunities is of overwhelming importance for economic theory. Hitherto, many economists have regarded invention—and technological change generally—as an *exogenous*, and some even thought, an *autonomous*, variable. It was exogenous in the sense that it was not controlled by economic variables. According to some, it was exogenous in a particular sense—it was autonomous, its own past entirely determining its future.
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> These views, insofar as they were of a substantive nature rather than merely a methodological convenience, are no longer tenable . . . the belief that invention, or the production of technology generally, is in most instances essentially a noneconomic activity is false . . . the production of inventions, and much other technological knowledge, whether routinised or not, . . . is in most instances as much an economic activity as is the production of bread.

Although the emphasis above is on capital-goods inventions, Schmookler also studied consumer-goods inventions and maintained (although with a little less vehemence) that the same conclusions applied—that market growth and market potential were the principal determinants of the direction and scale of inventive activity.

Nevertheless, despite his remarkable achievement, he would have been the last to discourage critical debate on his findings and his interpretation. As in every branch of science, the successors of a major theorist have the dual responsibility of building on past achievements without being so dazzled that they fail to see weaknesses or check the validity of received propositions in new or different circumstances. Consequently, I am convinced that the best way to demonstrate respect for Schmookler's achievements is to attempt to go beyond his work, and to reassess some of his conclusions.

Schmookler's study concentrated mainly on four major industries (railroads, petroleum refining, agricultural machinery, and paper making), but he also assembled data on "all other" industries and could reasonably lay claim to a fairly comprehensive statistical coverage of the entire US economy over the period 1840–1950. The exact time boundaries of his statistical series varied a little, but most of them covered a period of about a century, ending usually in the 1940s. Schmookler's careful and comprehensive coverage protected his work against the criticisms of bias and selectivity which had been successfully levelled against several of the other contemporary studies where the emphasis had been on innovations rather than inventions. I share with him the belief that patents do provide the most useful, systematic, and comprehensive set of information about inventive activity which is available over a long period.

Study of the postwar chemical industry

Schmookler's work is therefore the point of departure for our own study,[6] rather than the more numerous and undoubtedly important studies of innovation, which lacked any comparable data base. Like Schmookler, we have made extensive use of patent statistics and tried to relate these to various indicators of market growth.

Our coverage, however, is far less comprehensive: it is confined to one industry—the chemical industry—and within that industry to a few major subsectors. We chose this industry for a variety of reasons, but perhaps the principal one was that we wished to test Schmookler's hypothesis not only for a more recent period (the postwar period), but also for an industry which could be described as R and D intensive or even as science intensive. Although in their own time all of the four industries on which Schmookler concentrated were certainly the field of considerable inventive efforts, none of them (with the possible exception of petroleum refining after 1920) could be described as science intensive. Yet the view that technology has recently come to depend more heavily on new developments in science is usually justified with respect to a few industries, notably chemicals and electronics.

Ideally, we would have investigated several such industries; our more restricted coverage was due primarily to limitations of time. As Schmookler and his assistants found, the extraction, classification, enumeration, and analysis of patents is an extremely time-consuming business. However, in some respects, we have gone beyond what Schmookler attempted. We have not only made use of long time series of patent statistics; we have also attempted to use parallel time series of statistics of the publication of scientific papers. The use of these statistics was pioneered by a historian of science, Derek de Solla Price,[7] and again, whilst we recognise that there are serious methodological problems which have been raised by his critics, we nevertheless believe that for certain purposes their use has been vindicated and can provide valuable insights into long-term trends. We have also gone beyond Schmookler in attempting to include countries other than the USA, principally the UK and West Germany.

Would long time series of scientific papers, patents, investment, and production in several sectors of the chemical industry throw some light on the the push–pull controversy (as it applies to this industry)? If the science-push theory were correct, then it might be possible to show that a wave of publication of scientific papers preceded by several years a wave of inventions (as measured by patents), which might in turn be followed by a wave of new investment in the production of a particular group of chemicals. This would be a counter-Schmookler pattern of development.

If, on the other hand, it could be shown that the growth of investment in production facilities for particular groups of chemicals (or a decline in such investment) *preceded* a comparable wave (or decline) of patenting activity, then this would provide further validation for Schmookler's central hypothesis that "the amount of invention is governed by the extent of the market". If it could be further shown that waves of scientific publications *followed* rather than preceded waves of investment and invention, then this would go beyond Schmookler in indicating not merely that the market tends to lead technology, but that technology leads science, rather in the way that Hessen suggested in 1931.[8]

However, we were very much aware in carrying out our research of the dangers of simplistic use of aggregated statistics, which may obscure as much as they reveal. We attempted to gain some knowledge in every subsector of the detailed scientific, technical, and economic changes which occurred and which influenced the development of that sector, whether or not they were reflected in aggregate statistics. We differ from Schmookler in not accepting that aggregate trends in patent numbers also reflect the trend of really important inventions.

The changing pattern of causality

Our results may give some comfort to both poles in the debate but it would be fairly cold comfort—in some instances our work appears to validate Schmookler's results, whilst in others a counter-Schmookler pattern is discernible, and in still others no clear pattern emerges. We regard the results as a refutation of oversimplified schematic views of any variety, and as the basis for a more satisfactory, if slightly more complex, view of the interrelationships between scientific, technical, and economic developments.

The most interesting results are perhaps those which suggest that an early counter-Schmookler pattern, lasting several decades, may later give way in the same branch of industry to a clear-cut Schmookler pattern. This appears to have been the case with plastics, and may now be true of drugs. In plastics too, the shift from *product* to *process* inventions—a feature of the postwar period—is another indication that a satisfactory theory of economic development must take into account the special circumstances surrounding the birth, growth, and decline of each successive new branch of industry.

Our results do lend some credibility to the view of those, like Bernal, who have suggested that the relationships among science, technology, and industry have been changing during the century and that a new (counter-Schmookler) pattern may become more characteristic of the birth and early stages of development of new industries.[9] In these cases, major new developments in science (reflected in a wave of scientific publications) may trigger waves of invention, which in turn lead to recognition of big new opportunities for investment and production. At a later stage in the growth of such new industries, the shifting pattern of consumer demand and the requirements for process improvements may become the principal determinants of the direction and scale of invention, producing the classical Schmookler pattern of demand-led invention and technologically influenced science. There are analogies here with the Kuhnian concepts of paradigm shifts and 'normal' science.[10]

Reasons for rejecting demand-pull theories

The rejection of a pure market theory does not mean the denial of the influence of the market. Indeed, part of the debate on market demand is largely tautological. If innovation is defined (as it usually is, following Schumpeter)[11] as the commercial introduction and exploitation of an invention, then acceptance by the market is a condition which must be met for all innovations, irrespective of their origins.

Our project SAPPHO,[12] a comparison of success and failure, showed that the most successful industrial innovators are those who take a great deal of trouble to learn about the needs of potential users; that is they study the future market in all its complexity. It does not follow that the market is the only factor influencing the outcome, nor does it follow that the failures were managed by fools. All of the innovators were groping in the dark, involved in a complicated research process with a high degree of uncertainty affecting the potential customers, the government, the competitors, the law, the firm's own internal structure, the people working on the project, and the technical and scientific problems involved. The fascination of invention and innovation lies in the fact that *both* the marketplace and the frontiers of technology and science are continually changing. This creates a kaleidoscopic succession of new possibilities and combinations. An unexpected twist of events may give new life to some long forgotten speculations. (Patents for radar were taken out before 1914.) If it were only a question of the market which changed, then innovation would be a much simpler activity than it actually is.

The advance of scientific research in many different fields is constantly throwing up new discoveries and opening up new technical possibilities, which are to a large extent independent of any particular market pressure. If a firm, or a country, can monitor this advancing frontier, by one means or another, it may be able to gain both a technological and a market lead over its competitors by the speed of its response. As the Japanese example has shown, strong in-house R and D, as well as close contact with potential users and markets, will usually be needed to convert the first awareness of the new potential into a competitive advantage.

Innovation is a 'coupling' process, which first takes place in the minds of imaginative people somewhere at the ever changing interface between science, technology and the market. The coupling is far more than an intuitive flash: it is a continuous creative dialogue over a long period of research, experimental design, and development. That is why Schumpeter was so right to insist on the importance of the rare quality of 'entrepreneurship'.[11] Although this interpretation of innovation behaviour in the firm differs greatly from conventional theory, it has recently found recognition and more precise formulation in the work of Nelson and Winter.[13]

The debate in the West on market pull and science push is reflected by a parallel debate between Marxists. Some Marxists, eg Hessen and his colleagues,[8] stress the influence of economic demand even on theoretical physics. Others decry this as 'vulgar' Marxism and stress those passages in Marx's own writings in which he pointed out that man is not simply a tool-using animal, but an imaginative-thinking animal: what distinguishes the worst of architects from the best of bees is that the architect first creates in his imagination that which is subsequently created in reality.

The consequences of rejecting a demand-led theory

The acceptance in economic and social theory of a more complex evolutionary model of firm behaviour than that of the rational profit-maximising entrepreneurs, whether in its neoclassical or its Marxist variant, has far-reaching implications for government policy as well as for management at the firm level. In fact, this is an area where business practice has long since diverged from textbook prescription, as many economists are uncomfortably aware. If information is far from perfect and uncertainty prevails, then the costs and methods of acquiring information become extremely important: if innovation is a complex coupling process of communication, then structural problems both within and outside the firm are of central importance. It is not my purpose here to explore these issues, important though they undoubtedly are. Instead, I will examine some of the macroeconomic implications of the rejection of the demand-led theories of inventions.

A common assumption of market-led theories is that invention proceeds in a vast number of small incremental steps. Such marginalism is indeed an explicit assumption in much neoclassical economic theory. Whilst adopted simply as a methodological convenience or a simplifying assumption, such qualifications are sometimes forgotten; and the world is treated as though it did in fact possess the characteristics of the models. Although there is undoubtedly considerable flexibility in the response of science and technology to the changing pattern of socioeconomic needs, and marginalism is a useful way of thinking about many patterns of change, it would be dangerous to think entirely in these terms. The existence of cycles, the appearance of discontinuities, and the problems of structural change are all reasons for distrusting an oversimplified framework.

If we drop the assumption of 'perfect' information on the part of decision makers and of purely incremental flexible response to markets, and substitute instead a model which takes into account the supply side of science and technology, proceeding independently of the market, although of course interacting with it, then several important conclusions follow.

There will be some major potential developments in technology of whose existence or implications no one (and certainly not the 'market') will be fully aware: there will be many other developments of which even the best informed will be only dimly aware. An important function of policy for science and technology concerns these problems of communication. The introduction and widespread application of any new technology depends partly on its costs and forseeable benefits, and there will often be chicken-and-egg problems:

- Because there are few, if any, people who understand the new technology available to the firm, its introduction would involve enormous costs and risks to that firm in terms of training and recruitment.
- Because potential users are completely unfamiliar with the technology, it is impossible to conduct normal trials by conventional market research techniques, or to predict public reactions.
- Because official government and industrial standards and specifications do not recognise the existence of this new technology, its legal and social acceptance cannot be predicted.
- Because full-scale production has never been attempted, it is almost impossible to predict the effects of batch or mass production. The first one-off, full-size prototype is usually very expensive. The realisation, if this is possible, of any economies of scale often involves not only the innovating firm but also many other organisations (eg component suppliers), as well as a social learning process.

These considerations lead inexorably to the conclusion that chance plays a much greater role in competitive survival and in growth than it is comfortable to admit. They also suggest that a process of intermittent, uneven, or cyclical development is maybe more usual than a smooth incremental process. The bunching of groups of related inventions and the investment needed to bring about their widespread introduction is a more probable pattern of development than the incrementalism associated with run-of-the-mill modifications to established technologies, responding to minor changes in the market.

The evolution of breakthroughs

We might postulate a typical pattern for the major, breakthrough technologies on an *a priori* basis. A series of new scientific discoveries and technical advances, in hitherto unrelated fields, would lead at some point to imaginative inventors and scientists recognising some important and completely new possibilities. The ideas might not necessarily be widely publicised and patented, but they would be discussed and sifted. Gradually, the realisation of potential applications would crystallise, stimulated of course by the awareness of new demands and the expression of social needs.

At this stage, the forecasts would probably still be hopelessly awry, as they most certainly were with electric power, radio, plastics, computers, and lasers. Nevertheless, there would be enough stimulus to enable some pioneering firms to justify R and D projects and programmes.

Some of these would fail, some would succeed, but all would contribute to increased understanding of the potential of the technology. The big successes would stimulate 'bandwagon' and 'me-too' effects: pressures would accumulate to break down the remaining legal, educational, social, and other barriers to the full-scale application of the new technology.

At last, after this prolonged economic gestation process, economies of scale and standardisation would replace the previous diversity of model and design.

The state educational system would accept the new subjects as a regular part of the syllabus and the remaining barriers of standards, customs, and traditions would wither and die. In the mature phase of the fully established technology, the Schmookler pattern of demand-led invention and the Kuhn pattern of 'normal' science would then predominate. Competitive pressure to generate cost reduction through process improvements would become increasingly important.

Let us consider the relevance of this approach to the contemporary problems of industrial societies. It is an interesting hypothesis that Schumpeter's version of Kondratiev's long waves in economic development might be associated with the existence of such mechanisms. Kondratiev has postulated the existence of long waves in economic development, lasting about half a century.[14] In the expansionary phase, investment is buoyant, growth is rapid, and much new investment is generated. In the 'ebb tide', growth is slow, investment is sluggish, and higher levels of unemployment prevail, even during the upswings of the shorter trade cycles. According to this interpretation, the buoyant rapid growth of the 1950s and 1960s has now given way to a quarter century of slow growth and depression.

It was Schumpeter who, whilst largely accepting Kondratiev's notion of long waves, suggested that they might be associated with the appearance of one or several major clusters of innovations: eg steam power, railways, electric power and the automobile.[11]

These ideas often received fairly short shrift from economists, although they have received more sympathetic attention from historians. But the structural problems confronting industrialised societies in the 1970s have now reawakened interest. Schumpeter failed to explain quite why major, radically new technologies should affect the economy in the way which he suggested. As his critics unkindly pointed out, the loss of impetus in the later phase of the Kondratiev wave appeared to be due to the entrepreneurs getting a bit tired after their exertions in the first half of the wave. I have previously attempted to indicate some modifications of Schumpeter's idea,[15] which might give it greater explanatory force.

Schumpeter's ideas on the scale and significance of invention and innovation in the competitive struggle between firms are now very widely accepted and form the basis for Nelson's revisionist theory of the firm.[13] His no less controversial and powerful ideas on technology and long waves in economic development may also prove to be important, both for economic policy and for science and technology policy.

Forecasts and inventions

This article has argued, both from the evidence of our project on the chemical industry and on more general grounds, that simple market-demand models or science-push models are inadequate explanations of the trend of invention in specific branches of manufacturing industry or the economy as a whole.

The interaction between science, technology, and economic influence varies in its nature and intensity over time and between different industries. Schumpeter's ideas on innovation both within the firm and within the system are more helpful than purely incremental models, whether of demand or of invention. This makes forecasting much more difficult, as there is no escape from the extremely complex task of combining social with technological forecasting or from the recognition of the limitations of extrapolative econometric techniques.

Notes and references

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