The Economics of Industrial Innovation

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PART TWO

THE MICRO-ECONOMICS OF INNOVATION: THE THEORY OF THE FIRM

INTRODUCTORY NOTE

Part One has shown many instances of dramatic increases in productivity achieved by a combination of technical and organizational innovations as, for example, in cotton spinning, catalytic cracking of oil, scaling up of steel and chemical plants, assembly line production of automobiles, or miniaturization of integrated electronic circuits. It has also illustrated the widening and cheapening of the range of products available to consumers through product innovations in many industries, for example, pottery, consumer durables, new materials, radio and television.

This book now attempts to examine more systematically the conditions which have promoted such successful innovations, first at the level of the individual firm and innovative project (Part Two) and then at the level of the individual nation or region (Part Three). Chapter 8 first discusses those empirical research projects which have sought to identify the pattern of successful innovation in firms. Many innovative attempts end in failure, so that a systematic comparison of success and failure yields some interesting results, as is shown in the case of Project SAPPHO, described in some detail. This leads on to a discussion of the characteristics of those firms which have repeated success with innovations.

The discussion in Chapter 9 shows that size of firm certainly influences what kind of projects can be attempted in terms of technology, complexity and cost but does not in itself determine the outcome. In some areas and in some industries small firms play a very important role in innovation, as indeed the historical account has also shown. They have advantages of speed and flexibility in decision-making and often of lower costs in development work. The historical dimension is again shown to be crucial as the stage of development of a technology and/or an industry is one of the principal determinants of the relative contribution of large and small firms to innovations, and the types of innovation which they are able to make.

Although, as Part One has shown, technologies have certainly changed in rapid succession, and although firms have grown much larger in many industries and introduced entirely new management techniques, such as industrial R&D, nevertheless there are some things which have changed little if at all in their fundamentals. One of these is the prevalence of uncertainty with respect both to future technological change and to future market change. Chapter 10 shows that despite the introduction of numerous sophisticated mathematical techniques into project evaluation and decision-making, generally it has not proved possible for firms to make accurate forecasts of the future costs of development or the time such development will take. Even greater errors are typical of forecasts of future market size and rate of return on investment. Typically firms underestimate cost and overestimate speed of development, sometimes by very wide margins, as in such well-known cases as the aircraft Concorde or the fast breeder nuclear reactor. Errors of estimation in relation to future markets can go in either direction and are often wildly inaccurate. As Part One has shown, the future market for computers, for polyethylene and for synthetic rubber was grossly underestimated. In the case of nuclear power it has been vastly overestimated.

Forecasting errors are greatest in the case of the more radical innovations. With regard to small incremental improvements and new applications of existing products, much greater accuracy is possible and project evaluation techniques can be very useful management tools. The use of such techniques is in any case of some value as a means of mobilizing the necessary combined efforts within the firm and of disclosing potential difficulties at various stages of development and product launch. True uncertainty, where the future simply cannot be known is of course most characteristic of fundamental research and the most radical inventions.

This should not, however, necessarily lead to the conclusion that the most risky and uncertain projects should never be undertaken at all. On the contrary, again as the historical account in Part One has shown, the benefits from such R&D may be very great indeed over the long term. Many of today's most useful technologies owe their very existence to programmes of fundamental research in physics, chemistry and biology conducted over very long periods mainly in university laboratories. Many of today's most valuable products would not exist if determined entrepreneurs and inventors had not been prepared to devote their fortunes, their careers and even their lives to their development.

However, the uncertainty and the risks are such that most firms will not be able to contemplate basic research or the more radical types of innovation. This means that typically in all countries public expenditure has accounted for by far the greater part of basic research and has made a substantial contribution to generic technologies, such as biotechnology, and to information technology and various radical innovations. This type of public expenditure is discussed in Part Four, which deals with public policies for science and technology. In Part Two, the final chapter deals with the strategies of firms, confronted as they are with all the hazards and uncertainties attending technical innovation, whether by themselves or their competitors.

Chapter 11 attempts to classify the strategies which firms adopt as either offensive, defensive, imitative, dependent, traditional or opportunist. Firms which follow an offensive strategy are that very small minority which attempt to make radical innovations, sometimes but not always based on the conduct of fundamental research. A larger number of firms follow defensive strategies, responding fairly quickly to the innovative efforts of others with new products and processes of their own. This is sometimes described as a 'fast second' strategy. Much larger numbers of firms follow a simpler imitative strategy, sometimes on the basis of licensing, franchising or subcontracting from more innovative firms. Imitators may become completely dependent or may start out in a dependent role, as is often the case with firms in developing countries importing technology.

There are some industries where there is little technical change or where there is actually a competitive advantage in making a traditional product

with long-established techniques. Fashion and design innovations may nevertheless be important in such cases but not necessarily technical innovations. Finally, the variety of changing circumstances is so great, both in markets and technology, that there will always be possibilities of identifying product niches and moving into them on a purely opportunist, entrepreneurial basis.

Any attempt to classify firm strategies in this way is necessarily an oversimplification. For example, multi-product firms may follow different strategies in different sectors of their business and these may, in fact almost certainly will, change over time. The effort at classification is nevertheless valuable in bringing out the variety of ways in which firms make use of R&D and STS or, of course in many cases, do not do this. It is particularly valuable to conceptualize the efforts of firms in 'catching up' countries as attempts to upgrade their strategies as they learn to modify the imported technology and make increasing use of their own R&D and STS. This discussion at the end of Part Two thus leads on directly to the further discussion of national systems of innovation and catching up in Part Three.

CHAPTER 8

SUCCESS AND FAILURE IN INDUSTRIAL INNOVATION

8.1 INNOVATION AS COUPLING OF NEW TECHNOLOGY WITH A MARKET

We now consider some tentative generalizations about the technical innovation process in the firms and industries described, and discuss how far it is possible to test the validity of such generalizations, and to relate them to other industries and the economy as a whole.

Jewkes and his colleagues (1958) have argued that the nineteenth-century links between science and invention were much greater than is commonly assumed. Certainly, the classical economists were well aware of the connection between scientific advances and technical progress in industry, in the eighteenth and early nineteenth centuries. The quotation from Adam Smith (1776), with which this book begins, illustrates this point. Nevertheless, the evidence of the previous seven chapters suggests that there were profound changes in the degree of intimacy and the nature of the relationship between science and industry.

As already explained in Chapter 1, this does not imply the acceptance of a linear model of R&D with a simple one-way flow of ideas from basic science through applied research to development and commercial innovation. On the contrary, there has always been and there remains in the modern science-related industries a strong reciprocal interaction between all these activities (Soete and Arundel, 1993) and in particular a powerful influence of technology upon science. Gazis (1979) has given some examples of this interaction in the case of IBM's research laboratories. However, the effectiveness of this two-way movement of ideas depends on the ability of both communities to communicate with each other.

The new style of innovation in the industries which we have considered was characterized by professional R&D departments within the firm, employment of qualified scientists as well as engineers with scientific training, both in research and in other technical functions in the firm, contact with universities and other centres of fundamental research, and acceptance of science-based technical change as a way of life for the firm. Some of the firms we have considered had very strong scientific and technical resources, such as ICI, BASF, Du Pont, IBM, NEC, GM, Toyota, Siemens, GE, Hoechst, RCA, Marconi, Telefunken and Bell. An extreme case was the development of nuclear weapons and atomic energy.

During the twentieth century the main locus of inventive activity shifted away from the individual inventor to the professional research and development (R&D) laboratory, whether in industry, government or academia.

The nineteenth century was the heroic period of both invention and entrepreneurship. Names like Eli Whitney, 'Blacksmith, Nail-maker, Textile and Machine Tool inventor and innovator' ('he can make anything') spring to mind. Henry Thoreau, remembered now as a solitary philosopher, when asked to describe his profession ten years after graduation, replied that he was a Carpenter, a Mason, a Glass-pipe maker, a House Painter, a Farmer, a Surveyor and of course, a Writer and a Pencil-maker. He was in fact responsible for numerous inventions in pencil-making and there was a time when he could think of little else but improving the processes in his little pencil factory (Petroski, 1989). These men were not untypical of American and European nineteenth-century inventors. The British nineteenth-century Industrial Revolution owed much of its success to such men as these.

With an inventive career extending into the twentieth century, Thomas Edison embodied the transition from the 'great individualists', of which he was certainly one, to the large-scale R&D laboratories, that he helped to establish. He made a host of inventions and took out more patents (1,093) than any other single individual, but this was possible partly because he set up large contract research laboratories, first in Newark and later in Menlo Park. Among Edison's staff at these laboratories were some of the outstanding engineers and scientists who later helped to build up corporate inhouse R&D in Germany and Britain, as well as in the United States.

By the first decade of the twentieth century, although Edison was still making inventions, the focus of inventive effort was shifting from the contract laboratory typified by Menlo Park, Tesla's laboratories or that of Edward Weston to the inhouse industrial laboratories established by such firms as Kodak (1895), General Electric (1900), or Du Pont (1902). As Thomas Hughes (1989) shows in his classic study of the 'torrent' of American inventions from 1870 to 1940, by the time of the First World War, corporate R&D had displaced the contract laboratory as the centre of American inventive activities. Even as embryonic military–industrial complex had come into existence with the sponsorship of industrial research by the US Navy and especially the strong links established with Sperry Gyroscope.

Most of the major innovations we have considered were the result of professional R&D activity, often over long periods (PVC, nylon, polyethylene, hydrogenation, catalytic cracking, nuclear power, computers, television, radar, semiconductors). Even where inventor–entrepreneurs played the key role in the innovative process (at least in the early stages) such individuals were usually scientists or engineers who had the facilities and resources to conduct sustained research and development work (Baekeland, Fessenden, Eckert, Houdry, Dubbs, Marconi, Armstrong, Zuse). Some of them used university or government laboratories to do their work, while others had private means.

Frequently university scientists or inventors worked closely as consultants with the corporate R&D departments of the innovating firms (Ziegler, Natta, Haber, Fleming, Michels, Staudinger, Von Neumann). In other cases special wartime programmes led to the recruitment of outstanding university scientists to work on government sponsored innovations (the atomic bomb and radar). Intimate links with basic research through one means or another were normal for R&D in these industries and their technology is science based in the sense that it could not have been developed at all without a foundation in theoretical principles. This corpus of knowledge (macromolecular chemistry, physical chemistry, nuclear physics and electronics) could never have emerged from casual observation, from craft skills or from trial and error in existing production systems, as was the case with many earlier technologies. The same is true of recent biotechnology.

The rise of these new science-related technologies has had major economic and social repercussions over and above the growth of professional industrial R&D. It changed not only the development procedures, but also the production engineering, the sales methods, the industrial training and the management techniques. Quite often the majority of employees in firms in the new industries were not employed in production or handling of goods at all, but in generating, processing and distributing information and knowledge. In the extreme cases the computer software or process plant design and consultancy firm may employ hundreds of people but have no physical output other than paper or computer printout. But even in quite 'normal' electronic or chemical firms, the combined employment in research, development, design, training, technical services, patents, marketing, market research and management may be greater than in production. The complexity of the technical information involved and of the data processing means that specialized information storage, handling and retrieval systems are increasingly necessary. One of the most successful firms in the global telecommunications industry, Ericsson, employed fewer than ten per cent of its workforce in production by the mid-1990s. This proliferation of 'non-production' occupations is often treated as a form of Parkinson's Law or conspicuous waste. Even a scientist–inventor such as Gabor (1964) treated it as unnecessary in economic terms (although perhaps desirable on social grounds). No doubt Parkinson's Law does operate and labour savings can be made in some of these occupations (as they can in production). But it is essential to this analysis that the major part of this growth is due to the changes in technology, and to the new forms of competition which this has brought about.

So far we have discussed the new industries mainly in terms of the scientific basis of their new products and manufacturing technologies, but it is impossible to disregard the pull of the market as an essential complementary force in their origins and growth. In many cases the demand from the market side was urgent and specific.

The strength of the German demand for 'ersatz' materials to substitute for natural materials in two world wars spurred on the intense R&D efforts of IG Farben and other chemical firms. The strength of the military–space demand in the American post-war economy stimulated the flow of innovations based on Bell's scientific breakthrough in semiconductors and the early generations of computers. The urgency of British wartime needs spurred the successful development of radar of all kinds while the German government sponsored the development of FM networks, as well as radar. The Japanese government persuaded Toyota to enter the truck industry for military aims.

Conversely, the absence of a strong market demand for some time retarded the development of synthetic rubber in the USA, the growth of the European semiconductor industry, the development of radar in the USA before 1940, or of colour television in Europe after the war.

This does not mean that only wartime needs and government markets can provide sufficient stimulus for innovations, although they were obviously important historically. A strong demand from firms for cost-reducing innovations in the chemical and other process industries is virtually assured, because of their strong interest in lower costs of producing standard products and their technical competence. The demand for process innovations is related to the size of the relevant industry and here again the American oil industry provided a key element of market pull for the innovative efforts of the process-design organizations. The market demand may come from private firms, from government or from domestic consumers, but in its absence, however good the flow of inventions, they cannot be converted into innovations.

Innovation is essentially a two-sided or coupling activity. It has been compared by Schmookler (1966) to the blades of a pair of scissors, although he himself concentrated almost entirely on one blade. On the one hand, it involves the recognition of a need or more precisely, in economic terms, a potential market for a new product or process. On the other hand, it involves technical knowledge, which may be generally available, but may also often include new scientific and technological knowledge, the result of original research activity. Experimental development and design, trial production and marketing involve a process of matching the technical possibilities and the market. The professionalization of industrial R&D represents an institutional response to the complex problem of organizing this matching, but it remains a groping, searching, uncertain process.

In the literature of innovation, there are attempts to build a theory predominantly on one or other of these two aspects. Some scientists have stressed very strongly the element of original research and invention and have tended to neglect or belittle the market. Economists have often stressed most strongly the demand side: 'necessity is the mother of invention'. These one-sided approaches may be designated briefly as 'science-push' theories of innovation and 'demand-pull' theories of innovation (Langrish *et al.*, 1972). Like the analogous theories of inflation, they may be complementary and not mutually exclusive.

In a powerful critique of demand-pull theories of innovation, Mowery and Rosenberg (1979) pointed to the inconsistent use of the concept of demand in this literature and insist that the results of empirical surveys of innovation cannot legitimately be used (although they often have been) to support one-sided market-pull theories. The example of the electronic computer cited in Chapter 7 is a good example of their point that the market cannot evaluate a revolutionary new product of which it has no knowledge.

It is not difficult to cite instances which appear to give support to either theory. There are many examples of technical innovation, such as the atomic absorption spectrometer, where it was the scientists who envisaged the applications without any very clear-cut demand from customers in the early stages. Going even further, advocates of 'science-push' tend to cite examples such as the laser or nuclear energy, where neither the potential customers nor even the scientists doing the original work ever envisaged the ultimate applications or even denied the possibility, as in the case of Rutherford. Advocates of 'demand-pull' on the other hand tend to cite examples such as synthetic rubber, cracking processes or Whitney's cotton gin where a clearly recognized need supposedly led to the necessary inventions and innovations.

While there are instances in which one or the other may appear to predominate, the evidence of the innovations considered here points to the conclusion that any satisfactory theory must simultaneously take into account both elements. Since technical innovation is defined by economists as the first commercial application or production of a new process or product, it follows that the crucial contribution of the entrepreneur is to link the novel ideas and the market. At one extreme there may be cases where the only novelty lies in the idea for a new market for an existing product.[1] At the other extreme, there may be cases where a new scientific discovery automatically commands a market without any further adaptation or development. The vast majority of innovations lies somewhere in between these two extremes, and involves some imaginative combination of new technical possibilities and market possibilities. Necessity may be the mother of invention, but procreation still requires a partner.

Almost any of the innovations which have been discussed could be cited in support of this proposition. Marconi succeeded as an innovator in wireless communication because he combined the necessary technical knowledge with an appreciation of some of the potential commercial applications of radio. The Haber–Bosch process for synthetic ammonia involved both difficult and dangerous experimental work on a high pressure process and the development of a major artificial fertilizer market, stimulated by fears of war and shortage of natural materials. Despite their early complete underestimation of the market, IBM was for some time the most successful firm in the world computer industry because it combined the capacity to design and develop new models of computers with a deep knowledge of the market and a strong selling organization. Firms such as General Electric and RCA with similar or greater scientific and technical strength, but much less market knowledge and market power in this field, in the end had to withdraw.

We may indeed advance the proposition that 'one-sided' innovations are much less likely to succeed. The enthusiastic scientist–inventors or engineers who neglect the specific requirements of the potential market or the costs of their products in relation to the market are likely to fail as innovators. This occurred with EMI and AEI in computers and with several British firms in radar, despite their technical accomplishments and strong R&D organizations. Professionalization of industrial R&D means that there is now often an internal pressure group which may push 'technologically sweet' ideas without sufficient regard to the potential market, sales organization or costs.

On the other hand, the entrepreneurs or inventor–entrepreneurs who lack the necessary scientific competence to develop a satisfactory product or process will fail as innovators however good their appreciation of the potential market or their selling. This was the fate of Parkes with his plastic comb and of Baird with television. The failures may nevertheless contribute to the ultimate success of an innovation, even though the individual efforts fail. The social mechanism of innovation is one of survival in the market. The possibility of failure for the individual firms which attempt to innovate arises both from the technical uncertainty inherent in innovation and from the possibility of misjudging the future market and the competition. The notion of perfect knowledge of the technology or of the market is remote from the reality of innovation.

The fascination of innovation lies in the fact that both the market and the technology are continually changing. Consequently, there is a kaleidoscopic succession of new possible combinations emerging. What is technically impossible today may be possible next year because of scientific advances in apparently unrelated fields. Although Usher developed the concept mainly in relation to invention rather than technical innovation, this 'Gestalt' theory probably comes close to representing the imaginative process of matching ideas. What cannot be sold now may be urgently needed by future generations. An unexpected turn of events may give new life to long-forgotten speculations or make today's successful chemical process as dead as the dodo. Patents for a float glass process and for radar were taken out before 1914. The stone that the builders rejected is the cornerstone of the arch. The production of polyethylene was nearly suspended after the Second World War because the peacetime markets were thought to be too small. IG Farben offered to sell their synthetic rubber patents to the natural rubber cartel because they thought the synthetic product would not be able to compete in peacetime in price or quality. The early computer manufacturers expected that the market would be confined to government and scientific users. A century after early experiments electric road vehicles were again being seriously investigated by major automobile manufacturers. The apparently random, accidental and arbitrary character of the innovative process arises from the extreme complexity of the interfaces between advancing science, technology and a changing market. The firms which attempt to operate at these interfaces are as much the victims of the process as its conscious manipulators. Innovation works as a social process but often at the expense of the innovators. The implications of this high degree of uncertainty are discussed in Chapter 10.

These considerations lead to three conclusions of fundamental importance. First, since the advance of scientific research is constantly throwing up new discoveries and opening up new technical possibilities, a firm which is able to monitor this advancing frontier by one means or another may be one of the first to realize a new possibility. Strong inhouse R&D may enable it to convert this knowledge into a competitive advantage. Second, a firm which is closely in touch with the requirements of its customers may recognize potential markets for such novel ideas or identify sources of consumer dissatisfaction, which lead to the design of new or improved products or processes. In either case, of course, they may be overtaken by faster moving or more efficient competitors or by an unexpected twist of events, whether in the technology or in the market. Third, the test of successful entrepreneurship and good management is the capacity to link together these technical and market possibilities, by combining the two flows of information and new ideas.

Innovation is a coupling process and the coupling first takes place in the minds of imaginative people. An idea gels or clicks somewhere at the ever-changing interfaces between science, technology and the market. For the moment this begs the question of creativity in generating the inventive idea, except to note that almost all theories of discovery and creativity stress the concept of imaginative association or combination of ideas previously regarded as separate. But once the idea has clicked in the mind of an inventor or entrepreneur, there is still a long way to go before it becomes a successful innovation, in our sense of the term. Rayon was 'invented' 200 years before it was innovated, the computer at least a century before, and aeroplanes even earlier.

The coupling process is not merely one of matching or associating ideas in the original first flash; it is far more a continuous creative dialogue during the whole of the experimental development work and introduction of the new product or process. The one-man inventor–entrepreneur like Marconi or Baekeland may very much simplify this process in the early stages of a new innovating firm, but in the later stages and in any established firm the coupling process involves linking and co-ordinating different sections, departments and individuals. The communications within the firm and between the firm and its prospective customers are a critical element in its success or failure. As we have seen, in many cases the original idea may take years or even decades to develop, and during this time it continually takes on new forms as the technology develops and the market changes or competitors react. Consequently, the quality of entrepreneurship and good communications are fundamental to the success of technical innovations.

Summing up this discussion and the evidence in Part One, we might conclude that among the characteristics of successful innovating firms in the twentieth century in the industries considered were:

1. Strong inhouse professional R&D.
2. Performance of basic research or close connections with those conducting such research.
3. The use of patents to gain protection and to bargain with competitors.
4. Large enough size to finance fairly heavy R&D expenditure over long periods.
5. Shorter lead times than competitors.
6. Readiness to take high risks.
7. Early and imaginative identification of a potential market.
8. Careful attention to the potential market and substantial efforts to involve, educate and assist users.
9. Entrepreneurship strong enough effectively to co-ordinate R&D, production and marketing.
10. Good communications with the outside scientific world as well as with customers.

We might hypothesize that these are the essential conditions for successful technical innovations.

Up to a point, such tentative generalizations about the characteristics of innovation may be tested by analysing and comparing case studies of a large number of innovations. One difficulty about such case studies (many of which have been cited in Part One) is that we do not know how far they are representative of the innovative process. Indeed, much of the literature on industrial innovation falls into two categories: scattered case histories lacking comparability of coverage or theoretical analysis lacking systematic empirical foundations.

As a result, there are many plausible, half-tested hypotheses and many interesting conjectures in innovation theory, but insufficient firm evidence to refute or support them. The historical account in Part One suggests interesting conclusions, but it is difficult to find ways to substantiate them, or to assess their relative importance. Yet such systematic testing of generalizations and hypotheses is essential to advance our understanding.

The remainder of this chapter is therefore devoted to the description of a project which was deliberately designed to test such generalizations about innovation. The project was called SAPPHO and it was carried out at the Science Policy Research Unit during the 1970s. The original project was designed in 1968 by R. C. Curnow, but later stages of the work were led by R. Rothwell.

8.2 PROJECT SAPPHO

The basic idea of the project was to attempt to substantiate (or refute) generalizations about technical innovation, by the systematic comparison of pairs of successful and unsuccessful attempts to innovate in each branch of industry in turn. This method of course rests on the observation that competitive technical innovation is a fairly general characteristic of many branches of industry in industrialized capitalist societies.

Since the introduction of a new product or a new process in any branch of industry may render older products and processes obsolete or uneconomic, firms which wish to survive and grow must be capable of adapting their technologically based strategy to this competition. This does not necessarily mean that every firm has to be research minded or to innovate itself. Various alternative strategies are possible for the firm even in an industry subject to rapid technical change. Some companies may even prefer to disappear rather than to innovate. These alternatives are considered more systematically in Chapter 11. For the time being we are concerned with those firms which do attempt to innovate, whether in products or processes. Some of these firms may attempt to be the first to introduce a new product or process hoping thereby to gain a technological lead and temporary monopoly profits. This strategy is sometimes designated as offensive innovation. Others may act only defensively in response to innovations introduced by competitors. In either case the firm will need some capacity to develop and launch new products or processes (even if under a licensing agreement or by simple imitation). Frequently a firm which attempts to be first may not succeed, and multi-product firms may be offensive in some fields and defensive in others. But in the long run their survival and growth will depend on whether they succeed or fail in their innovations, whether offensive or defensive.

The first stage of project SAPPHO, which is summarized here, was a study of 58 attempted innovations in chemicals and scientific instruments (listed in Table 8.1). Those in the chemical industry were mainly process innovations, whereas the instrument innovations were all product innovations. The instruments were mainly electronic and the chemical processes

Table 8.1 List of SAPPHO pairs

Scientific instruments	*Chemicals (process innovations)*
Amlec eddy-current crack detector	Accelerated freeze-drying of food (solid)
Atomic absorption spectrometer	Acetic acid
Digital voltmeters	Acetylene from natural gas
Electromagnetic blood-flow meter	Acrylonitrile I
Electronic checkweighing I	Acrylonitrile II
Electronic checkweighing II	Ammonia synthesis
Foreign-bodies-in-bottles detector	Caprolactam I
Milk analysers	Caprolactam II
Optical character recognition	Ductile titanium
Roundness measurement	Extraction of aromatics
Scanning electron microscope	Extraction of *n* paraffins
X-ray microanalyser	Hydrogenation of benzene to cyclohexane
	Methanol
	Oxidation of cyclohexane
	Phenol
	Steam naphtha reforming
	Urea manufacture

related mainly to intermediates derived from petroleum (SPRU, 1972). In later work, additional pairs of innovations were studied in these same industries and then, using a somewhat different methodology, paired comparisons were made in various sectors of the mechanical engineering industry (Rothwell *et al.*, 1974; Rothwell, 1976, 1992, 1994).

By pairing attempted innovations it was hoped to discriminate between the respective characteristics of failure and success. The technique had of course been widely used in the natural sciences, especially in biology (McKay and Bernal, 1966). When the two halves of a pair differ with respect to a particular characteristic or set of characteristics, this indicates a possible explanation of innovative success or failure. Where there is a significant and repeated variation between the pattern of success and failure, across a large number of pairs, this provides systematic evidence for the validity of particular hypotheses or groups of hypotheses. Such explanations as appear to have a significant statistical foundation may then be tested again on a new sample of innovations. In this way a structured and tested foundation for theoretical work may be built up.

It was expected that the success and failure halves of a pair would resemble each other fairly closely in many respects, and this proved to be the case. It could be assumed from previous experience that firms attempting to develop a particular new process or product would often have many characteristics in common. The analysis of similarity is complementary to the analysis of divergence for two reasons. First, it enables the identification of some characteristics which are shared by all firms attempting innovation in particular industries. These may be necessary conditions for entry into the race, and may be regarded as such unless other success cases can be found which disprove such tentative generalizations. But second, and more important, they enable us to focus attention on those characteristics in which the pattern does diverge between success and failure. In future research it will be possible to concentrate in greater depth on these significant differences through a process of elimination of unnecessary hypotheses.

The pairs were not 'identical twins'. Their similarity was defined in terms of their market, not necessarily in terms of their technology. For example, two firms might both be seeking a new, cheaper and better way to produce phenol or urea. They might adopt somewhat different technical solutions. It was an assumption of this project that this very choice constituted part of the success, and the wrong choice part of the failure. In a few cases the resemblance was very close, as where several licensees shared access to the same basic technical knowledge. But even here the design varied when two manufacturers attempted to satisfy the same demand. Success depended partly on developing the 'right' design, having regard to the available scientific and technical knowledge and to the potential uses of the innovation.

Concentration on innovation rather than invention has many consequences in terms of method. The most important of these is that the marketing aspects of the process assume much greater importance, whereas the role of that individual, usually described as the inventor, recedes into a wider social context. Our comparisons did not include those numerous experiments by inventors and would-be innovators which are discarded or shelved long before they reach the point of commercial introduction. Such studies are undoubtedly of interest in the management of R&D, but the focus here was on the wider problem of the management of innovation. The failures were products or processes which were brought to the point of commercial introduction, and usually were in fact on the market for some years. Attention was therefore concentrated both on the various stages of development work and also on the preparations for production and sale and the experience of marketing the innovation.

Since the project was concerned with technical innovation in industry, the criterion of success was a commercial one. A failure is an attempted innovation which failed to establish a worthwhile market and/or make any profit, even if it worked in a technical sense. A success is an innovation which attained significant market penetration and/or made a profit. This chapter analyses 29 successes and 29 failures. Often a failure was clear-cut, e.g. a firm went bankrupt, or closed a plant down, or withdrew a product or failed to sell it, whereas success was not always so self-evident. A product might achieve a worldwide market, but take a long time to show a profit. One case (Corfam) which was originally expected to be a success was withdrawn from the market on these grounds (Chapter 5). Even with failures it was not always simple to make an assessment. There were varying shades of grey between the extremes of success and failure. The project deliberately tried to investigate the fairly clear-cut 'black and white' cases of failure and success. In two cases in the chemical industry, and one in instruments, it proved to be feasible to complete two pairs, as there were several commercial successes and several less successful attempts in each case.

Earlier work on the literature survey had shown that there were many possible explanations of success and failure. The project was therefore designed to test a large number of single hypotheses and simultaneously to test a large number of possible combinations of factors. The aim was to

identify a characteristic pattern of failure or success. Altogether about two hundred measurements of each case of success or failure were attempted. Some of the measures were comparative, some absolute. Thus, for example, it was possible to test the hypothesis that large size is generally advantageous for innovation, both by testing in how many pairs the smaller of the two firms failed, and by checking what proportion of firms with fewer than 500 employees succeeded (or 100, 1,000 or 10,000 employees).

But most of the measures were comparisons between the success and failure halves of the pair, enabling statements to be made such as 'successful attempts were characterized by greater . . . or less . . . or smaller . . . or shorter . . . or more . . .' than attempts which failed, but the aim was to link all those comparisons together to derive a pattern of success. The main hypotheses which the project attempted to test related to various measures of size (employment, R&D department and project team); measures of market research, publicity, education of users, involvement of users; modification of the innovation and checks on its progress at various stages; the role of engineers and scientists and of various key individuals, their previous experience, education and background[2], the management, control and planning system in the firm; the communication network with the outside world; degree of dependence on outside technology and familiarity of the firm with the innovation; effectiveness and methods of organizing R&D work, patent policy, competitive pressures, speed in development work and date of commercial launching.

The results of the analysis may be classified under three headings:

1. Factors which were common to almost all attempts to innovate, whether successful or not.
2. Factors which varied between innovative attempts, but in which the variation was not systematically related to success or failure.
3. Measures which discriminated between success and failure.

8.3 RESEMBLANCE BETWEEN SAPPHO PAIRS

Taking the first 29 pairs, involving 58 attempts to innovate, there were many resemblances between both halves of the pairs (Table 8.2). Almost all attempts in these two industries took place within a formal R&D structure which was used to develop the innovation. This confirms the professionalization of R&D described in Part One. Only in the instrument industry were there cases of attempted innovation without such a structure. Most of these were designs for a new product brought from an outside environment.

Since almost all of the firms involved in attempts to innovate had this formal R&D structure, it might be expected that critical differences would exist in the way in which such departments were organized, R&D was planned, projects evaluated, or incentives provided for engineers and scientists. A great deal of the management and sociological literature has concentrated on these aspects of the efficiency of industrial research.

The inquiry did not uncover systematic differences of this kind with respect to R&D organization or incentives. As with previous empirical studies it was found that many supposedly best practice techniques in long-term planning and project assessment were honoured more in the breach than in the observance. But the successful innovators differed only a little from the failures in this respect. In the chemical industry, although not in instruments, there was some evidence that better management and planning techniques were associated more frequently with success. Some of the difficulties inherent in R&D forecasting and project evaluation are discussed in Chapter 10. There was no evidence that successful innovators expected rewards or penalties differing from the less successful nor was there any evidence of unusual incentive schemes for R&D personnel, or greater freedom in successful cases.

One possible explanation of more successful attempts to innovate might lie in patent priority, but again it was not possible to identify differences here. Almost all innovators, both successful and unsuccessful, took out patents and regarded them as important. But the failures did not attribute their lack of success to the patent position of their rivals, except in one case. The results confirm the evidence of the historical account that innovating firms usually take trouble over patents because of their importance as bargaining counters, and to ensure rights of entry into a field, but that patents do not necessarily prevent any competitive developments.

Nor was it found that successful innovators differed from unsuccessful ones in the way in which they organized their project teams. One hypothesis had suggested that the less successful attempts might be characterized by departmental organization on disciplinary lines. But this was not the case. Where firms had a large R&D organization, they sometimes had laboratories working on conventional subdisciplinary lines, but this did not really affect the project development team which was set up in a similar way in both successes and failures.

Another hypothesis for which no supporting evidence was found was the view that business or technical innovators might be less well qualified academically in unsuccessful attempts (or better qualified). There were important differences between business innovators which did distinguish between success and failure, but this was not one of them. Most of the business innovators, and almost all the technical innovators, were qualified scientists or engineers in both halves of the pair. There was a slight tendency for the PhDs to be the more successful in chemicals. Obviously, in these two industries amateurs are rarely chosen to manage innovations. In the two cases when accountants were the business innovators, both failed, but this would be too small a number on which to construct any general theory. It would probably not be possible to find a sufficient number of cases in these industries where innovators were not technically qualified, to test any hypotheses relating to the supposed merits or demerits of amateurism. The difficulty of finding such cases is, however, further evidence of the professionalization of the innovative process.

8.4 VARIATION UNRELATED TO SUCCESS OR FAILURE

Many other measures did show considerable variation between attempts to innovate, but the variance was not closely related to success. Among them were measures relating to size of firm, size of R&D department and numbers of qualified engineers and scientists in R&D. These results need considerable care in interpretation. There was no strong systematic evidence that larger or smaller firms or R&D departments were more or less successful. For example, of the cases involving firms employing more than 10,000 people, six were successes and seven were failures. Where large firms were in competition with smaller firms there was a tendency for them to be more successful, but it was by no means clearcut. At first sight this finding is perhaps at variance with some of the evidence from the historical descriptive account in Part One, which suggested that the heavy costs and long time scale of many innovations would give an advantage to large firms.

However, this result should not be interpreted as implying that size of firm is completely irrelevant in relation to innovation in these two industries. Comparative size measured within a pair did not differentiate between success and failure clearly but in chemicals only 4 out of 34 attempts were made by firms employing fewer than 1,000. Clearly size is relevant to the type of innovations which are attempted at all, and inter-industry differences are very important. The next chapter is devoted entirely to a critical discussion of this problem of size in relation to innovation.

No relationship was found between success and the number of scientists and engineers on the main board of the innovating company, although this proportion varied considerably. However, in almost all cases there were some engineers or scientists on the main board, and it may be that this is the critical threshold factor since the innovation process requires a combination of technical, financial, marketing and management skills.

Perhaps surprisingly, for those who believe in the amenability of innovation to planning techniques, no relationship was found between success and the capacity to set and fulfil target dates for particular stages of the project plan, or in the general approach to planning of the innovators. This finding too needs considerable care in interpretation and is discussed more fully in Chapter 10.

Contrary to some theories, there was no association between failure and the attempt to innovate in areas unfamiliar to the firm. Where firms differed significantly in their familiarity with the field, the outcome was evenly distributed between success and failure.

Another set of measures which did not discriminate between success and failure related to the growth rate of the firm and its competitive environment. There were of course variations between firms in the growth which they had experienced before the innovation, and in the competitive pressures to which they were subject. But these differences apparently did not affect their degree of success in attempting to innovate. Again, it is important not to overstate this finding. This does not mean that competitive pressures or declining growth may not be important in stimulating attempts to innovate, only that they do not ensure success.

A rather surprising finding was that development lead time was not strongly correlated with success. It had been expected that the more successful innovators would be those who found ways of shortening the development phase and telescoping the stages from prototype or pilot plant to commercial launch. But support for this hypothesis came only at the earlier stage of applied research. In the chemical industry successful firms were quicker to get through this early stage. The absence of any evidence of a shorter development stage associated with success provides support for those who have argued that hardware development is a gestation process akin to animal reproduction in that it cannot easily be artificially shortened (Burke, 1970). It may also indicate that successful firms take more trouble at the development stage to get rid of all the bugs, so that later stages are trouble free. There was considerable indirect support for this interpretation from those measures which did discriminate between success and failure.

More recently, there has been some evidence of systematic differences in development lead times between Japanese firms on the one hand and American firms on the other (Mansfield, 1988; Womack *et al.*, 1990; Graves, 1991). Shorter Japanese lead times appear to be related to specific Japanese organizational techniques in the management of innovation, at least in some industries (Baba, 1985; Nonaka and Takeuchi, 1986). Some of these techniques were described in Part One but project SAPPHO did not include any Japanese firms and did not reveal systematic differences between American and European innovations.

8.5 THE PATTERN OF SUCCESS

Of the two hundred measures attempted, only a small number differentiated clearly between success and failure, and these varied a little between the two industries. The principal measures are shown in Table 8.2.

Those which came through most strongly were directly related to marketing. In some cases they might be regarded as obvious, but the case studies showed that even the most obvious requirements were sometimes ignored. Successful attempts were distinguished frequently from failure by greater attention to the education of users, to publicity, to market forecasting and selling (particularly in the case of instruments where it was most relevant) and to the understanding of user requirements.

The single measure which discriminated most clearly between success and failure was 'user needs understood'. This should not be interpreted as simply, or even mainly, an indicator of efficient market research. It reflects just as much on R&D and design as well as on the management of the innovation. The product or process had to be designed, developed and freed of bugs to meet the specific requirements of the future users, so that understanding of the market had to be present at a very early stage. The work of von Hippel (1976, 1978) on 'customer-active' paradigms in new product development, and of Teubal *et al.* (1976) on 'market determinateness' in the Israeli medical electronics industry, both point to the same conclusion. It has been further explored by the interesting work of Mansfield *et al.* (1977) on the integration between marketing and R&D in project selection systems and the ways in which this influences probability of success, and in the work of Lundvall (1985, 1988b, 1993).

This interpretation was confirmed by the strong evidence on the occurrence of unexpected adjustments and bugs after development in the failure cases, and of the need for user adaptations in nearly half the failures. About three-quarters of the cases of failure showed greater after-sales problems. Thus, 'user needs understood' is just as much a discriminating

Table 8.2 Part 1 Measures which did not differentiate between success and failure

Question	*Chemicals*			*Instruments*			*Both industries*			*Binomial test*
	S > F	*S = F*	*S < F*[a]	*S > F*	*S = F*	*S < F*[a]	*S > F*	*S = F*	*S < F*[a]	
Was the innovation more or less radical for the firms concerned?	5	7	5	4	3	5	9	10	10	0.5
At what level was the decision to proceed with the innovation made?	2	11	4	1	10	1	3	21	5	0.363
Was a time limit set?	4	12	1	1	8	3	5	20	4	0.5
Were patents taken out for this innovation by the organization?	—	17	—	3	8	1	3	25	1	0.313
Did one organization accept the innovation as being more in its natural business than the other?	5	8	4	6	1	5	11	9	9	0.412
Did one organization have a more serious approach to planning than the other?	6	7	4	2	7	3	8	14	7	0.5
Was there a systematic and periodically reconsidered R&D programme?	6	7	4	—	10	2	6	17	6	0.613
What was the company's publishing policy?	5	7	5	1	11	—	6	18	5	0.623
Were there any incentive schemes to encourage innovation effort?	2	15	—	—	12	—	2	27	—	0.25
What outcome was the project expected to have on the careers of members of the project team in the event of success?	3	11	3	1	11	—	4	22	3	0.5
Was the innovation part of a general marketing policy?	5	8	4	2	9	1	7	17	5	0.387
What was the degree of coupling with the outside scientific and technological community in general?	2	12	3	2	8	2	4	20	5	0.5
Would the firm have recruited more QSEs if it could have done so at the time of the innovation?	—	17	—	1	10	1	1	27	1	0.75
In each case, when was the decision to innovate formalized on paper?	5	5	7	1	10	1	6	15	8	0.395

continued overleaf

Table 8.2 Part 1 *cont.*

Question	*Chemicals*			*Instruments*			*Both industries*			
	$S > F$	$S = F$	$S < F$[a]	$S > F$	$S = F$	$S < F$[a]	$S > F$	$S = F$	$S < F$[a]	*Binomial test*
How many months elapsed from prototype or pilot plant to first commercial sale?	7	3	7	5	3	4	12	6	11	0.5
Was there a formal R&D department in the organization	1	16	—	2	8	2	3	24	2	0.5
What was the scale of growth of the organization up to the time of marketing (measured by annual growth of turnover in the five years prior to the marketing of the innovation)?	1	13	3	4	4	4	5	17	7	0.387
How many years did the business innovator spend in the educational system?	5	7	5	2	5	5	7	12	10	0.315
Was the R&D department regarded as a profit centre?	6	8	3	1	10	1	7	18	4	0.274
Was there any need to find or use new materials?	—	17[b]	—	—	10	1	—	27	1	0.5

[a] S > F Success more than failure, greater than failure, etc., or in success but not in failure.
S = F No measurable difference between success and failure.
S < F Success less than failure, smaller than failure, etc., or in failure but not in success.
[b] Data not available in one case.
— nil.

Table 8.2 Part 2 Measures which differentiate between success and failure

Question	*Chemicals*			*Instruments*			*Both industries*			
	$S > F$	$S = F$	$S < F$[a]	$S > F$	$S = F$	$S < F$[a]	$S > F$	$S = F$	$S < F$[a]	*Binomial test*
Was the innovation more or less radical for world technology?	10	6	1	2	9	1	12	15	2	0.0065
How deliberately was the innovation sought, comparatively?	7	8	2	6	6	—	13	14	2	0.0037
Was there opposition to the project within the total organization on commercial grounds?	1	9	7	1	7	4	2	16	11	0.0112
Was more use made of development engineers in planning and costing for production in one case than in the other?	5	9	3	4	8	—	9	17	3	0.073
Did one organization have a more satisfactory communication network than the other externally?	5	10	2	5	7	—	10	17	2	0.0193
Was the R&D chief more senior by accepted status in one case than the other?	9	5	3	2	8	2	11	13	5	0.105
Was the sales effort a major factor in the success or failure of the innovation?	7	10	—	9	3	—	16	13	—	0.000015
Were any modifications introduced after commercial sales as a result of user experience?	1	8	8	2	6	4	3	14	12	0.0176
Were there any after-sales problems?	—	4	13	1	2	9	1	6	22	0.000005
Were any steps taken to educate users?	8	9	—	6	5	1	14	14	1	0.00049
If new tools or equipment were needed for commercial production, were any ordered before the decision to launch full-scale production?	8	7	2	2	10	—	10	17	2	0.227

continued overleaf

Table 8.2 Part 2 *cont.*

Question	*Chemicals*			*Instruments*			*Both industries*			
	S > F	*S = F*	*S < F*[a]	*S > F*	*S = F*	*S < F*[a]	*S > F*	*S = F*	*S < F*[a]	*Binomial test*
What was the degree of coupling with the outside scientific and technological community in the specialized field involved?	8	9	—	5	6	1	13	15	1	0.00092
How much attention was given to publicity and advertising?	6	10	1	4	7	1	10	17	2	0.0193
Did the innovation have to be adapted by users?	—	10	7	—	7	5	—	17	12	0.00024
Were there unexpected production adjustments?	1	7	9	1	7	4	2	14	13	0.00636
Did any 'bugs' have to be dealt with in the early production stage?	1	6	10	1	5	6	2	11	16	0.0049
Was any systematic forecasting by the marketing (or sales) department involved in the decision to add the innovation to production lines or to existing processes?	5	7	5	6	5	1	11	12	6	0.166
Were user needs more fully understood by the innovators in one case than in the other?	15	2	—	9	3	—	24	5	—	0.0000001
Did the business innovator have a more diverse experience in one case than in the other?	8	8	1	8	2	2	16	10	3	0.00377
Did the business innovator have a higher status in one case than in the other?	8	8	1	5	4	3	13	12	4	0.0245
Did the business innovator have more or less authority (power) in one case than in the other?	9	7	1	6	4	2	15	11	3	0.000656
To what extent was dependence on outside technology a help or a hindrance in production?	10	6	1	6	4	2	16	10	3	0.00221
How large a team was put to work on the innovation at the beginning of the project?	12	2	3	4	4	4	16	6	7	0.0466
How large a team was put to work on the innovation at the peak of the project?	9	4	4	7	4	1	16	8	5	0.0133
How many years had the business innovator spent in industry?	9	7	1	3	4	5	12	11	6	0.119
Had the business innovator had any overseas experience?	3	14	—	5	6	1	8	20	1	0.0352
Did the business innovator have a greater degree of management responsibility in one case than in the other?	10	7	—	4	5	3	14	12	3	0.00636

[a] S > F Success more than failure, greater than failure, etc., or in success but not in failure.
S = F No measurable difference between success and failure.
S < F Success less than failure, smaller than failure, etc., or in failure but not in success.
— nil.

Source: Science Policy Research Unit (1971).

measure of efficiency in R&D performance as of marketing and overall management.

Size of project team emerged as a clear-cut difference, whereas other size measures did not differentiate. Since in a number of cases the smaller firms deployed a larger team, this implies a greater concentration on the specific project. This consideration is important in considering the relative advantages of the small firm in innovation in Chapter 9. Another measure which strongly suggests the advantages of specialization in R&D is that related to coupling with the outside scientific community.

Carter and Williams (1959a) already emphasized good communications with the outside world as one of the most important characteristics of the technically progressive firm. The most backward firms would not of course be found among those attempting to innovate. But among those who were making such attempts there were significant differences in their general pattern of communications. Better external communications were associated with success, but the strongest difference emerged with respect to communication with that specialized part of the outside scientific community which had knowledge of the work closely related to the innovation. General contact with the outside scientific world did not discriminate between success and failure.

All of these differences may of course be related to the quality and type of management, so that measures relating to the business innovator are perhaps the most interesting. First, it should be noted that the business innovator was hardly ever the same person as the chief executive in the chemical industry, but was frequently so in the instrument industry. The most interesting difference between successful and unsuccessful business innovators, and one which was unexpected, was that greater seniority was associated with success. The successful man (they were all men) had greater power, higher formal status, and more responsibility than the unsuccessful. He was also older and had more diverse experience. Some of these differences were not so clear cut in the instrument industry which may reflect the greater mobility and smaller size of firm, together with more hierarchical structure of management in the chemical industry. Usually the successful chemical innovator had been longer with the innovating firm and in the industry, whereas this was not true in the instrument pairs.

The higher status and greater power of the more successful innovators may be associated with their readiness to take greater risks and to recruit larger teams for their projects. In the chemical industry there was a strong association between success and a more radical technical solution. But taking a variety of measures relating to risk acceptance, there was only very slight evidence that successful innovators assumed greater risks. In the chemical innovations the successful cases were usually the first to market but in the instrument pairs those who came later were usually more successful.

The fact that the measures which discriminated between success and failure included some which reflected mainly on the competence of R&D, others which reflected mainly on efficient marketing, and some which measured characteristics of the business innovator with good communications, confirms that view of industrial innovation as essentially a coupling process, which was suggested at the outset. One-sided emphasis on either

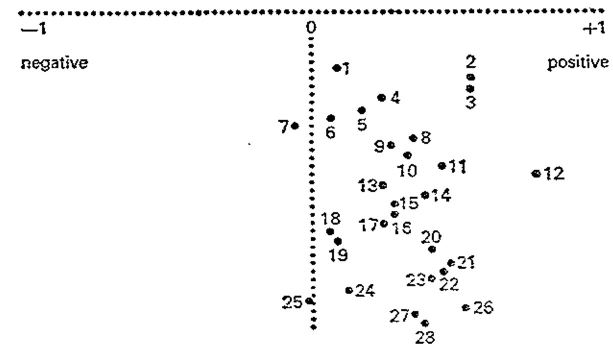

Fig. 8.1 Values of index variable for success points

Source: Science Policy Research Unit (1972).

R&D or sales does violence to the real complexity of the process. This was strongly confirmed by the multivariate statistical analysis illustrated in Figure 8.1. Composite index variables were formed consisting of several measures relating to one factor. The percentage of points correctly classified was greatest by the combination of the following composite measures: R&D strength, marketing and user needs. In the case of chemicals, composite variables relating to management techniques and management strength were also important. Management strength relates mainly to the status and responsibilities of the business innovator.

The critical role of the entrepreneur (whatever individual or combination of individuals fulfil this role) is to match the technology with the market, i.e. to understand the user requirements better than competitive attempts, and to ensure that adequate resources are available for development and launch. This interpretation of the key role of the quality of entrepreneurship is in line with the findings of Barna (1962), Penrose (1959) and the earlier work of Schumpeter on the theory of the firm (1912, 1942).

In the large firm the business innovators must be high enough in the hierarchy to command resources and get things done. They must have enough knowledge of the way the firm works to know *how* to get things done. In the small firm it frequently means that it will be the chief executive, or a person sufficiently close to ensure the necessary concentration of effort. In either case they must be sufficiently powerful and clear about marketing objectives to ensure that the various screening and testing procedures during the course of development and trial production prevent an unsatisfactory product or process coming onto the market. Premature launch may be more dangerous than slowness.

These conclusions will not necessarily be valid for consumer goods innovations where some different mechanisms are at work. In capital goods it is essential to satisfy certain minimal technical performance criteria. The extent to which these generalizations may apply to other industries is discussed in

Chapter 16. Here it is necessary to consider some other limitations of the analysis, before going on to consider in greater depth the question of size of firm in Chapter 9 and the problem of uncertainty, risk and planning in Chapter 10.

Thus, the results of SAPPHO confirm points 1, 3, 8, 9 and 10 among the tentative generalizations advanced on p. 203. Point 2 is discussed more fully in Chapter 11. Point 4 requires great care in interpretation and the whole of Chapter 9 is devoted to this. Points 5 and 7 were not supported by the evidence of SAPPHO. The approach of the innovator to risk again requires much more detailed consideration, and this is attempted in Chapter 10, followed by discussion of the implications for theory of the firm in Chapter 11.

The generalizations so far made about innovation based on the historical descriptive material in Part One thus find some confirmation from this test. Although they may provide a fairly plausible interpretation of some aspects of industrial innovation since the rise of professionalized R&D, it is certainly not claimed that they are securely based statistically or empirically. The sample was not random and the 'universe' is not known. However, a further sample of fourteen pairs confirmed the original results in all essentials. Moreover, the interpretation of innovation as a coupling process is strongly supported by much additional empirical evidence, as well as by logic and common sense. Earlier studies by Carter and Williams (1957, 1959a, 1959b) had led them to formulate the concept of the 'technically progressive firm' embodying many of the combined characteristics of the SAPPHO success cases. Another major series of case studies of industrial innovation in Britain was conducted at Manchester quite independently of SAPPHO at about the same time, and the authors of these concluded:

> Perhaps the highest level generalization that it is safe to make about technological innovation is that it must involve synthesis of some kind of need with some kind of technical possibility. The ways in which this synthesis is effected and exploited take widely differing forms and depend not only on systematic planning and the 'state of the art' but also on individual motivations, organizational pressures and outside influence of political, social and economic kinds. Because the innovation process extends over time, it is important to retain continuous sensitivity to changes in these factors and the flexibility to perceive and respond to new opportunities.
>
> (Langrish *et al.*, 1972, p. 200).

8.6 FURTHER STUDIES OF INNOVATION

Additional important empirical evidence for some of the main SAPPHO conclusions came from a Canadian survey of 47 new small firms, started by technologically oriented entrepreneurs. Like SAPPHO this included the study of failures as well as successes. Litvak and Maule (1972) concluded from their survey that:

> The marketing performance of the entrepreneurs was weak, and was a major factor for the apparent high mortality rate of the projects. Most of the entrepreneurs were unable to see the linkage between product innovation and marketing innovation. . . . Most of the new product development was carried out and implemented before any attempt was made to assess the market potential and the costs of penetrating the market. . . . The point to be made is that the love that the entrepreneur has for his product innovation often blinds him from perceiving his real opportunities and the state of market competition.
>
> (Litvak and Maule, 1972, p. 47)

The point about underestimation of user needs and understanding of the market must be heavily underlined, since the SAPPHO inquiry constantly found in discussion with R&D managers and entrepreneurs that they tend to dismiss the point as obvious, but nevertheless continue to ignore it in practice.

Further confirmation of some of the SAPPHO conclusions came rather unexpectedly from the Hungarian electronics industry (Szakasits, 1974) and from the OECD's international studies of industrial innovation, particularly Pavitt's (1971) cross-country comparisons of the relative innovation success of firms in various member countries, and from the studies of innovation sponsored by the National Science Foundation (NSF) in the United States. An interesting example of the way in which some industrial firms accepted and used the SAPPHO findings in their own management of innovations is given by Leonard-Barton (1995) in her account of the highly successful R&D-intensive American company, Hewlett Packard (pp. 132–3).

However, even when the statistics are relatively good, as in relation to size of firm (Chapter 9), generalizations still need to be heavily qualified. One reason for this is that the 'universe' of innovations or inventions is not known and therefore no strictly random sample can be drawn.[3] Consequently, although attempts may be made to study a representative group of inventions and innovations, as in the Jewkes study or in the research project described in this chapter, we cannot be sure that such a sample is truly representative. This reservation is particularly important when we come to consider so-called secondary or improvement inventions and innovations. There is a tendency in case study and historical work to concentrate on the more spectacular inventions and innovations. But it can be argued as, for example, by Gilfillan (1935) and Hollander (1965), that the myriad of minor improvements and new models are as important for technical progress as the more radical breakthrough innovation. Moreover, it can also be plausibly maintained that non-specialists and non-professionals may make a much bigger contribution to the secondary type of innovation than to the breakthrough. It is also probable that knowledge of the market plays a bigger part in this secondary type of invention and innovation than contact with scientific research or, in the case of process innovations, direct experience of operating the process.

Schmookler (1966) showed that in several American industries, over a long period of more than a century, invention (as measured by statistics of relevant patent numbers) tended to follow behind demand (measured by statistics of investment), with a time lag of a few years. However, another project on the chemical industry at the Science Policy Research Unit showed that in some instances in the chemical industry, there was evidence of 'counter-Schmookler' patterns of growth in the early stages of the emergence of radical new technologies, such as synthetic materials and drugs in the 1920s and 1930s (Walsh *et al.*, 1979). In these periods, surges of inventive activity and of science discovery tended to precede the take-off of sales and investment, as in the analogous case of the electronic computer discussed in Chapter 7. This apparent contradiction may be explained in part if some distinction is made between the more radical inventions and innovations, which are relatively few in number, and the very large numbers of secondary and improvement inventions and innovations, multiplying rapidly as an industry grows and responding directly to market signals and investment behaviour. Thus Schumpeter's theory which puts the emphasis on autonomous innovative activity by entrepreneurs as the mainspring of economic development rather than market demand, can be reconciled with the Schmookler statistics, which measure something rather different.

Insofar as patent statistics capture both minor and major inventions, then Schmookler (1966) showed that professionalized corporate R&D in the 1950s accounted for about half of industrial inventions in the United States and probably for a higher proportion of those which were exploited, i.e. translated into innovations. Many of the other inventions also originated from professional R&D in government and universities. The development and exploitation of inventions emanating from university, government or private inventors probably also involved some professional R&D work in industry in the great majority of cases. But this would still leave a significant number of inventions and innovations which could not be attributed to specialized professional R&D. It is also likely that an even higher proportion of non-patented technical advances are attributable to those outside the professional R&D system.

However, the extent of our ignorance should not be exaggerated. There is firm empirical evidence that most professional industrial R&D is concentrated on product and process improvement and on new generations of established products. What is not known is the relative contribution to technical progress of the R&D work by comparison with the inventions and improvements generated entirely outside the formal R&D system. It is a plausible hypothesis that the proportionate contribution of the formal R&D system is much higher in the research intensive industries, but it also seems likely that technical progress will be most rapid where there is a very strong interaction between the professional R&D groups and all other personnel associated with the process or product who may themselves contribute to the solution of many problems as well as to their identification. This was confirmed by a detailed study of a major technical innovation in the coal-mining industry – the Anderton shearer-loader. Townsend (1976) demonstrated that the highly successful introduction and diffusion of this machine was based on an interplay between a series of more radical inventions and innovations introduced by the machinery makers (co-operating closely with the research establishments of the National Coal Board) and numerous improvement inventions made as a result of operating experience and encouraged by an awards scheme. Both British and German manufacturers contributed to major improvements in the design of this machine, derived in part from their own R&D, and in part from the incorporation of improvements specified by the National Coal Board in Britain. Hollander's work emphasized especially the contribution of the engineering department

and technical assistance groups to technical change, but sometimes in association with R&D.[4]

The later stages of project SAPPHO shifted the emphasis away from the individual cases of success or failure with particular innovations to the study of success or failure of firms over a fairly long period. This enabled the project to take account both of individual major innovations and of incremental innovations. Work was concentrated in sectors of the engineering industries, such as textile machinery (Rothwell, 1976), mining machinery (Townsend, 1976) and agricultural machinery (Rothwell, 1979). Whereas firms could and did succeed for short periods by concentrating on incremental improvements, sometimes even without any formal R&D organization, they were often trapped in the long run by an inability to cope with the more radical types of technological competition (such as the Sulzer weaving machine). The results showed that long-run success depended on an ability to combine occasional more radical innovations with a flow of minor improvements in design, responding to customers' wishes and experience. Strong R&D was increasingly necessary to sustain this combination of technical change in the 1960s and 1970s. The earlier SAPPHO case studies, although oriented towards individual projects, had also pointed in this direction. Especially in the scientific instruments industry, one of the hallmarks of the successful cases was almost always a capacity to incorporate successive design improvements in a series of new models, as for example in the case of the milk analyser (Robertson and Frost, 1978).

One important piece of empirical work lent support to the view advanced here that specialized R&D and other technical services have been increasingly important both for the major radical inventions and innovations and for the minor improvement inventions and innovations. This was the work of Katz (1971) in Argentina. He set out to measure the contribution of technical progress to the growth of a large number of enterprises in several branches of the Argentine economy. He was able to collect very comprehensive time series for a large number of firms (250) and to relate his results to measures of the scale of adaptive R&D and other technical activities carried out by the enterprises. From his preliminary interviews he had ascertained that many Argentine firms, while not making original radical innovations themselves, nevertheless made many adaptations and improvements to the processes and products which they had acquired either from foreign parent companies or by imitation or licensing. He hypothesized that such adaptive R&D would confer important competitive advantages by enabling the firms to meet the peculiar requirements of the Argentine market more satisfactorily, or to adapt to the specialized operating conditions. The conclusion from this work and the research of Martin Bell (1984, 1991) in other developing countries is that 'learning' in whatever country is not simply a function of time but depends on deliberate organized activities, whether preformed in what is nominally an R&D department or elsewhere in the firm.

Katz's results showed conclusively that: (1) the growth of enterprises was closely related to their technical progress; (2) their technical progress was strongly associated with the performance of adaptive R&D, and of specialized technical services, although the professional group responsible for this work might be called process development or technical department rather than research department. His results also suggested the important conclusion that imitative or adaptive R&D is more certain in its outcome than offensive or defensive R&D, since studies of firm growth and R&D intensity in the USA and UK have not shown such a strong association. Hollander goes so far as to claim that many minor technical improvements are virtually risk free.

The Federation of British Industries' comparisons of UK firm growth rates and R&D intensity (1947, 1961) did show positive but weak correlations, and fairly strong association at the extremes. These results, taken together with those of the SAPPHO project, suggest that:

1. Firms performing little or no R&D in industries of rapid technical change are likely to stagnate or disappear.
2. Firms performing a great deal of R&D may sometimes enjoy exceptionally high growth rates through offensive success.
3. In the defensive middle zone, variations in R&D intensity show no statistical association with growth, and uncertainty predominates.

Although the statistical association between R&D intensity and subsequent growth by firms is not very strong, the association between successful innovation and subsequent growth of the firm *is* strong. Both Mansfield (1968a, b) and other economists have provided convincing confirmatory empirical evidence of the conclusions which common sense suggests – that successful technical innovation leads to the rapid growth of the firm. On the other hand, as we have seen, unsuccessful innovation may lead to bankruptcy, however large the scale of R&D. The implications of the high degree of uncertainty associated with radical innovation are discussed further in the succeeding chapters.

8.7 CONCLUSIONS

Following Rothwell's work on project success and firm success in the 1970s and 1980s, he attempted to synthesize all the results of this and other empirical research on innovation in the 1980s (1992, 1994). While maintaining that this work had generally confirmed the findings of the SAPPHO project in other firms and other industries, he indicated a greater emphasis on management planning and control procedures (Table 8.3) without, however, giving strong evidence of the effectiveness of such procedures. At the corporate level he stressed top management commitment and long-term strategy. This is discussed further in Chapter 11. He cited the work of Maidique and Zirger (1985), Dodgson (1991) and Prahalad and Hamel (1990) in support of his contention that repeated success with innovation depended on a process of know-how accumulation over fairly long periods.

Basil Achilladelis, who did the research for many of the SAPPHO project innovations in the chemical industry, has subsequently made some extremely thorough studies of the innovation performance of firms in various sectors of that industry, notably pesticides, petrochemicals and pharmaceuticals (Achilladelis *et al.*, 1987, 1990). Among his many interesting findings, one of the most significant has been his demonstration that

Table 8.3 Success factors

Project execution factors
- Good internal and external communication: accessing external knowhow.
- Treating innovation as a corporate-wide task: effective inter-functional coordination: good balance of functions.
- Implementing careful planning and project control procedures: high quality upfront analysis.
- Efficiency in development work and high quality production.
- Strong marketing orientation: emphasis on satisfying user needs: development emphasis on creating user value.
- Providing a good technical service to customers: effective user education.
- Effective product champions and technological gatekeepers.
- High quality, open-minded management: commitment to the development of human capital.
- Attaining cross-project synergies and inter-project learning.

Corporate level factors
- Top management commitment and visible support for innovation.
- Long-term corporate strategy with associated technology strategy.
- Long-term commitment to major projects (patient money).
- Corporate flexibility and responsiveness to change.
- Top management acceptance of risk.
- Innovation accepting, entrepreneurship accommodating culture.

Source: Rothwell (1992, 1994).

chemical firms which had an original success with a radical innovation were frequently able to follow this with an accumulative series of further successful innovations in the same field (Table 8.4). His work showed further that the success of the large chemical firms in synthetic materials extended also to other sectors of the industry (Table 8.5). These results confirm Rothwell's conclusions on the role of knowledge accumulation in successful firms.

Finally, Rothwell (1992, 1994) studied the influence of ICT on innovation management and innovative success. This led him to an increasing emphasis on the importance of various forms of 'networking' in what he designated as the 'fifth generation' innovation process (Table 8.6). Systemic factors have always been important for successful innovation, as has been clearly demonstrated in the historical evidence on textiles, chemicals, electrical engineering and automobiles, but it is becoming increasingly clear that ICT has redoubled their significance. In the first place, ICT has provided vastly more efficient means for the accumulation and speedy transmission of data within and between individuals and organizations. Second, many innovations now incorporate some electronic devices or elements of computerization which often necessitate some type of collaboration with electronic hardware or software firms. Studies of the rapidly increasing scale of collaborative agreements between firms in the 1980s and 1990s, showed that a high proportion involved firms in ICT, in biotechnology and in advanced materials (Hagedoorn and Schakenraad, 1990, 1992). The complexity of technological development in these and other technologies now often rules out going it alone in R&D and impels firms into collaborative arrangements of one kind or another. A special issue of *Research Policy* on 'Networks of Innovators' (DeBresson and Amesse, 1991)

Table 8.4 Some examples of corporate technological traditions

No.	*Company*	*Technological tradition*	*Radical innovation*	*Year*
1	American Cyanamid	Aminoplasts	Urea melamine resins	1935
2	American Cyanamid	Organophosphorus insecticides	Thimet	1956
3	BASF	Organic chemical intermediates	Ammonia synthesis	1913
4	BASF	Polystyrene plastics	Polystyrene	1928
5	BASF	Magnetic recording tapes	First magnetic tape	1935
6	Bayer	Organophosphorus insecticides	Parathion	1942
7	Bayer	Synthetic rubber	First synthetic rubber	1910
8	Bayer	Polyurethane plastics, foams	Polyurethane	1942
9	B. F. Goodrich	PVC	PVC	1930
10	Celanese	Synthetic fibres	Cellulose acetate	1924
11	Celanese	Organic chemical intermediates	Acetic acid	1933
12	Ciba-Geigy	Insecticides	DDT	1939
13	Ciba-Geigy	Herbicides	Triazines	1957
14	Ciba-Geigy	Vat dyestuffs	Ciba violet	1905
15	Dow	Halogenated hydrocarbons	Chloroform	1903
16	Dow	Polystyrene	Polystyrene	1932
17	Dow	Pesticides	Pentachlorophenol	1930
18	Du Pont	Synthetic fibres	Nylon	1936
19	Du Pont	Fungicides	Nabam	1936
20	ICI	Herbicides	MCPA	1942
21	ICI	Reactive dyes	Procion dyes	1956
22	Monsanto	Herbicides	Randox	1955
23	Montedison	Organic chemical intermediates	Ammonia	1924
24	Montedison	Polypropylene plastics, fibres	Polypropylene	1954
25	Rohm & Hass	PMMA-acrylics	Polymethylmethacryiate	1932

Source: Achilladelis *et al*. (1990).

Table 8.5 Concentration and technological accumulation in chemical innovations, 1930–80*

1 *Type of innovations and companies*	2 *Pesticides*	3 *Pesticides*	4 *Synthetic materials*
	Top 5 companies	*Top 10 companies*	*Top 5 companies*
	Bayer Geigy ICI Dow Du Pont	Col. (2) plus: BASF Hoechst Shell Cyamid Sumitomo	Bayer BASF Hoechst Du Pont ICI
% of all innovating companies	6	12	5
% of all product and process patents	19	27	30
% of all new products	31	44	58
% of all radical innovations	38	54	60
% of major market successes	35	55	66

* Synthetic materials 1930–55

Sources: Achilladelis *et al*. (1987); Freeman *et al*. (1963).

Table 8.6 The fifth generation innovation process: Systems Integration and Networking (SIN)

Underlying strategy elements
- Time-based strategy (faster, more efficient product development).
- Development focus on quality and other non-price factors.
- Emphasis on corporate flexibility and responsiveness.
- Customer focus at the forefront of strategy.
- Strategic integration with primary suppliers.
- Strategies for horizontal technological collaboration.
- Electronic data processing strategies.
- Policy of total quality control.

Primary enabling features
- Greater overall organizational and systems integration:
  - parallel and integrated (cross-functional) development process
  - early supplier involvement in product development
  - involvement of leading-edge users in product development
  - establishing horizontal technological collaboration where appropriate.
- Flatter, more flexible organizational structures for rapid and effective decision-making:
  - greater empowerment of managers at lower levels
  - empowered product champions/project leaders.
- Fully developed internal databases:
  - effective data sharing systems
  - product development metrics, computer-based heuristics, expert systems
  - electronically assisted product development using 3D-CAD systems and simulation modelling
  - linked CAD/CAE systems to enhance product development flexibility and product manufacturability.
- Effective external data links:
  - co-development with suppliers using linked CAD systems
  - use of CAD at the customer interface
  - effective data links with R&D collaborators.

Source: Rothwell (1992).

and the Conference on 'Technological Collaboration' at Manchester in 1993 (Coombs *et al*., 1996) were two of many instances of the rapid growth of research interest in this field. Still lacking were studies of the evolution of networks with a few exceptions such as the brilliant study of imaging networks in Sweden by Anders Lundgren (1991).

Nevertheless, there is now sufficient evidence on the role of networking in innovation to postulate that the typical pattern of nineteenth-century innovation (the inventor–entrepreneur) and of twentieth-century innovation (the inhouse corporate R&D department with good external communications) is now increasingly giving way to a pattern of networking collaborative systems innovation in the twenty-first century. Among the driving forces of this change, two of the most important factors are the increasing complexity of technical change and the systemic nature of many ICT innovations. The example of IBM illustrates this change very well: in the 1950s and 1960s IBM had hardly any collaborative R&D arrangements and came very close to autarchy in its own immense R&D facilities; in the 1980s and 1990s IBM has made dozens of collaborative arrangements with other firms, large or small, in a variety of industries. Parts Three and Four will further explore the growth of networking in both national and international systems of innovation.

NOTES

1. While this may be described as innovation, it cannot be legitimately described as technical innovation. Non-technical organizational innovations are extremely important and often associated with technical innovations as in the case of mass production and lean production or the marketing innovations of Wedgwood.
2. Four key roles in the conduct of innovation were defined as follows:
   1. Technical innovator: the individual who made the major contributions on the technical side to the development and/or design of the innovation. He would normally, but not necessarily, be a member of the innovating organization. He would sometimes, but not always, be the inventor of the new product or process. (They were all male.)
   2. Business innovator: that individual who was actually responsible within the management structure for the overall progress of this project. He might sometimes be the technical director or the research director. He might be the same man as the technical innovator. He could be the sales director, or chief engineer. Occasionally, especially in smaller firms, he could be the chief executive for the organization as a whole. (They were all male.)
   3. Chief executive: the individual who was formally the head of the executive structure of the innovating organization, usually but not necessarily with the job title of managing director. In every case there was an identifiable chief executive, and almost always an identifiable business innovator, but quite often there was no identifiable technical innovator. No attempt was made to force individuals to assume these roles if they were not readily identifiable, since one of the objects of the inquiry was to assess the contribution of outstanding individuals.
   4. Product champion: any individual who made a decisive contribution to the innovation by actively and enthusiastically promoting its progress through critical stages. He might sometimes be the same individual as the technical innovator, or chief executive. Although these roles have been recognized in much of the earlier innovation literature, they are not always identifiable from formal titles used in firms. The job title might vary a good deal, but it was the role which was important. (They were all male.)
3. The second Manchester study of innovation (Gibbons and Johnston, 1972) was based on an ingenious attempt to develop a random sample (see p. 258).
4. This may sometimes be just a question of nomenclature. What is called 'engineering', 'OR' or 'technical department' in one firm may be called 'process development' or 'R&D' in another.

CHAPTER 9

INNOVATION AND SIZE OF FIRM

The historical account in Part One showed that in synthetic materials, in chemical processes, in nuclear reactors and in some electronic systems large firms predominated in launching the innovations. But a blanket hypothesis of 'bigness wins' could not be sustained, either from Part One or from the SAPPHO project. In scientific instruments in particular, new small firms made outstanding contributions. Inventor–entrepreneurs establishing new firms had apparently also been important in the early days of the chemical industry, the automobile industry, the semiconductor and radio industries. They continued to flourish in the microcomputer industry and in computer software. How far is it possible to test generalizations about the relative contribution of large and small firms to industrial innovation?

The evidence from project SAPPHO, so far as it goes, suggests that as between competitive attempts to innovate, size in itself does not affect the outcome very much. However, it is apparent that there is a range of innovations which is not attempted at all by really small firms so that, for example, the competition in the chemical industry or turbine generators is mainly between various large or giant firms. The relative contribution of large and small firms varies a great deal from industry to industry, and investigations such as SAPPHO cannot answer the question of the aggregate contribution of large or of small firms to research and innovation in the economy as a whole.

The size structure of industry and its relationship to problems of monopoly and competition is a problem which has preoccupied economists for a long time (Turner and Williamson, 1969; Cohen, 1995; Scherer, 1992c) and there is now a considerable amount of statistical information. Unfortunately, in our field of interest most of this relates to R&D, or patents rather than innovation, so that there are big problems of interpretation. This chapter aims to provide such an interpretation and concludes by reviewing some attempts at the direct measurement of the numbers of innovations by large and small firms in the manufacturing industry (Freeman, 1971; Kleinman, 1975; Townsend *et al.*, 1982; Pavitt *et al.*, 1987; Acs and Andretsch, 1988; Kleinknecht and Reijnen, 1992b).

9.1 SIZE OF FIRM AND EXPENDITURE ON R&D

Whereas in the 1950s there was very little reliable empirical evidence outside the USA on the degree of concentration in the performance of industrial research and experimental development, such evidence became available in the 1960s. As a result of the efforts of the OECD in standardizing definitions and methods,[1] reasonably comparable data for a dozen countries (OECD, 1967, p. 46) became available. The picture that emerged

Table 9.1 Percentage of total industrial R&D performed in firms ranked by size of R&D programmes

Country	*Number of firms ranked by size*						
	4	8	20	40	100	200	300
USA	22.0	35.0	57.0	70.0	82.0	89.0	92.0
UK	25.6	34.0	47.2	57.9	69.5	75.0	77.0
France	20.9	30.5	47.7	63.4	81.0	91.2	95.6
Japan	—	—	—	47.7[a]	52.1[b]	63.1[c]	71.4[d]
Italy	46.4	56.3	70.4	81.6	92.5	—	—
Canada[f]	30.3	40.8	58.4	71.5	86.2	93.2	—
Netherlands	64.4[e]	—	—	—	—	—	—
Sweden	33.2	43.0	54.0	71.0	85.4	90.0	—
Belgium	38.5	51.8	72.6	82.7	92.8	97.5	99.4
Norway	29.5	38.8	55.7	70.6	88.2	97.9	100.0
Spain	25.2	47.0	73.9	91.5	—	—	—

[a] The first 54 firms.
[b] The first 85 firms.
[c] The first 180 firms.
[d] The first 289 firms.
[e] The first 5 firms.
[f] Current intramural expenditure.

Source: OECD (1967).

was consistent and confirmed the hypothesis of those economists who had postulated a high degree of concentration. The hundred largest R&D programmes accounted for more than two-thirds of all industrial R&D in all countries except one, and for more than three-quarters in most cases. The forty largest programmes accounted for more than half of all industrial R&D in all cases except one, and the eight largest for more than 30 per cent in all countries for which figures were available (Table 9.1). In the Netherlands the five largest programmes accounted for two-thirds of all expenditures (Philips, Shell, Unilever, AKU, DSM).

Since the 1960s, there has been a slight reduction in concentration. This has been associated with the very rapid growth of new, small NTBFs (new technology based firms) in ICT, advanced materials and biotechnology. There is less complete but fairly conclusive evidence that the vast majority of small firms in OECD countries still do not perform any organized research and development. For France, Britain and the United States, and probably most other countries, the proportion of small firms performing R&D is almost certainly less than 5 per cent (if small is defined as fewer than 200 employees).

It is true that the official statistics of research and experimental development expenditures may not capture research or inventive work which is performed by managers, engineers or other staff incidentally to their main work. It may be that this part-time amateur inventive work is very productive, and the evidence will be discussed for the view that small firms account for an exceptionally high proportion of significant inventions and innovations. But so far as specialized professional R&D activity is concerned, there is pretty firm evidence that this is highly concentrated in large firms in all countries for which statistics are available.

Table 9.2 Percentage of R&D, net sales and total employment by companies with largest R&D programmes, USA, 1970

Programme size	*Total R&D*	*Federal R&D*	*Net sales*	*Total employment*
First 4	18	20	6	8
First 8	32	40	9	11
First 20	55	71	16	19
First 40	66	85	23	27
First 100	79	93	38	39
First 200	87	96	50	50
First 300	91	97	63	62

Source: National Science Foundation (1972, pp. 46–7).

The OECD statistics which have been cited (Table 9.1) measured the degree of concentration by size of R&D programme, and not by size of firm in terms of total employment, turnover or assets. However, for the major countries some statistics are available on concentration by size of firm, although not as a consistent classification. Firms with more than 5,000 employees accounted for 89 per cent of all industrial R&D expenditures in the United States in 1970, and for 90 per cent in 1978. They accounted for about 75 per cent in the German Federal Republic in 1979 and probably about the same proportion in the UK. Firms employing more than 3,000 accounted for about two-thirds of Japanese industrial R&D in 1978–9.

However, the degree of concentration was much less marked by size of firm (classified by total employment) than by size of R&D programme. In the United States there were 466 firms with more than 5,000 employees performing R&D in 1970. But many of them had relatively small R&D programmes, whereas some medium-sized firms (1,000–4,999 employees) had rather large ones. Thus the 300 largest programmes were approximately equivalent to the outlays of the 470 largest firms, each accounting for about 90 per cent of the total (Table 9.2). R&D programmes were far more concentrated than sales or employment (Table 9.2). In France the 200 largest programmes accounted for about 91 per cent of total expenditures, but the 200 largest firms (measured by employment) accounted for about 72 per cent. There are some industries in which even the largest firms perform little or no research, and others in which even small firms perform a good deal.

The major source of variations in research intensity between firms is the industry concerned, so that analysis of the relationship with size is best done industry by industry.

In the early debate in the 1960s some economists claimed to have discovered inverse correlations at least for some industries in several European countries and for Canada (Hamberg, 1966; Morand, 1970; de Melto *et al.*, 1980). However, these results have been disputed as more complete evidence became available. Reviewing all the most recent data, both Cohen (1995) and Symeondis (1996) conclude that R&D spending seems to rise more or less proportionately with firm size above a threshold level:

> The most robust finding from the empirical research relating R&D to firm size and market structure is that there is a close positive monotonic relationship between size and R&D which appears to be roughly proportional among R&D performers in the majority of industries or when controlling for industry effects.
> (Cohen, 1995, p. 196)

However, there are exceptions to this generalization, both by industry and by country.

In addition to the points made at the end of the previous chapter, it could be postulated that the small firms who do perform R&D would tend to fall into three categories:

1. Firms which have just begun to develop or exploit a new invention. In this case sales could be relatively low in relation to R&D and a very high research intensity could be expected. For example, Genentech in 1994 had an R&D/sales ratio of over 40 per cent. This might tend to fall in the event of successful commercial exploitation of the innovation and growth of the firm and its sales.
2. Highly specialized firms which have a particular expertise, sustained by an intensive research programme in a very narrow field. Here too, research intensity might often be high. For example, some spin-off firms in science parks have R&D/sales ratios between 10 and 20 per cent over fairly long periods.
3. Firms struggling to survive in industries in which new product competition makes R&D increasingly necessary. A very varied management response might be expected in these circumstances, with some firms trying to scrape by with a subthreshold R&D effort, others relying mainly on co-operative research, and still others taking high risks with an ambitious programme.

From much indirect evidence, such as the growth of science parks and the number of spin-off firms around universities it is reasonable to suppose that the number of firms in the first two categories has been increasing fairly rapidly from the 1970s to the 1990s.

If these suppositions are correct, they would account both for the relatively weak correlation between research intensity and size of firm, found in some studies and for the empirical observations of wide inter-industry variations in the strength of this correlation. In the UK, France, Germany and the USA it has been found that in some industries, small or medium-sized firms had higher research intensity than large firms. Even for all industries taken together, although the US figures showed a consistently higher research intensity for firms employing more than 25,000 it was the federal contracts placed with firms that accounted for the greater part of the difference (Table 9.3). Taking company financed R&D only, the difference by size of firm was not great (remembering that we are dealing here only with those firms that do perform R&D).

Turning to variations in research intensity among large firms, Hamberg (1964) and Scherer (1965a, b) found only a weak correlation with size measured in terms of employment or sales, and still less with size measured in terms of assets. Hamberg's sample consisted of 340 large firms

Table 9.3 Funds for R&D as percentage of net sales in R&D performing companies by size of company, USA

Firm size	*Total R&D (including federal contracts) as % of net sales*			*Company funds for R&D (excluding federal contracts) as % of net sales*		
	1957	*1967*	*1977*	*1957*	*1967*	*1977*
Less than 1,000	1.8	1.7	1.7	1.4	1.6	1.6
1,000 to 4,999	1.8	1.7	1.5	1.2	1.4	1.3
5,000 to 9,999	}	2.1	1.9	}	1.6	1.5
10,000 to 24,999	} 3.9	} 5.2	1.8	} 1.6	} 2.3	1.5
25,000 or more	}	}	4.2	}	}	2.4
All firms	3.4	4.2	3.1	1.5	2.1	2.0

Source: National Science Foundation (1979).

Table 9.4 Concentration of patents, R&D expenditure, and employment and various inventive activity intensity measures for firms with more than 25,000 employees, ranked by employment

Number of firms included	*Percentage of all 130 firms*			*Number of patents per $ bill. sales*	*R&D as % of sales*	*Number of patents per $ mill. R&D*
	Patents	*Employment*	*R&D*			
First 4	9.04	23.98	24.13	11.86	2.69	0.441
First 8	19.89	34.62	38.39	17.98	2.94	0.609
First 12	25.91	40.84	43.87	20.17	2.90	0.695
First 16	35.21	45.98	51.61	20.06	2.50	0.803
First 20	40.71	50.39	54.50	21.41	2.44	0.879
First 30	53.13	59.28	63.88	24.47	2.50	0.978
First 40	58.31	66.25	69.69	23.03	2.34	0.984
First 50	64.81	71.93	75.11	23.55	2.32	1.015
First 75	78.99	83.87	78.75	23.17	2.14	1.085
First 100	91.08	92.77	94.11	22.99	2.02	1.138
All 130	100.00	100.00	100.00	23.03	1.96	1.176

Source: Soete (1979).

from the *Fortune 500* list, while Scherer's sample was 448 firms from the same list. Scherer made the interesting observation that in several industries, research intensity generally rose with size up to sales of $250 million, but began to fall somewhere between $200 million and $600 million. (See also the literature review by Kamien and Schwarz, 1975.) Soete (1979) examined more recent evidence which became available during the 1970s and concluded that the US R&D data did not on the whole support the views of Hamberg and Scherer, although the patent data did still provide some support. Soete maintained that the evidence for the United States showed some tendency for R&D intensity to increase with size of firm, at least in some industries. However, it must be remembered that all the data in Table 9.4 refer to firms with more than 25,000 employees (i.e. very large firms). Evidence in Part One also suggests that the largest firms were sometimes the most research intensive (IG Farben, Standard Oil and Bell).

Thus, summing up the evidence on size of firm and R&D expenditures, which was available in the 1970s:

1. R&D programmes were highly concentrated in all countries for which statistics were available.
2. These programmes were mainly performed in large firms with more than 5,000 employees, but the degree of concentration was significantly less by size of firm than by size of programme.
3. The vast majority of small firms (probably well over 95 per cent) did not perform any specialized R&D programmes.
4. Among those firms which did perform R&D, there was a significant correlation between size of total employment and size of R&D programme in most industries.
5. There was a generally weaker correlation between the relative measure of research activity (research intensity) and size of firm and it was not significant in some industries.
6. In several countries those small firms that did perform R&D had above average R&D intensities.

The most recent evidence for the 1980s and 1990s indicates a reduction in concentration both in terms of size of programme and size of firm; but R&D nevertheless remains more concentrated than employment or sales. Whereas the vast majority of small firms still performs no specialized R&D, the number of R&D performing small firms has increased.

Before attempting to interpret these results it is necessary to consider a little further the relationships between R&D expenditures (inputs into R&D) and R&D output.

9.2 SIZE OF FIRM AND INVENTION

A number of economists has maintained that despite the heavy concentration of R&D expenditure in large firms, it is the small firms that account for most of the important inventions and innovations. As has already been indicated, whereas the measurement of R&D inputs has made significant progress in the past twenty-five years, less progress has been made with measurement of R&D outputs.[2] It was only in 1980 that the OECD devoted a full conference to output (OECD, 1980b). It is generally accepted that the direct output of industrial R&D is a flow of new knowledge and information relating to new and improved products and processes. This may take the form of research reports, technical specifications, operational data and instruction manuals based on experience with pilot plants or prototypes, scientific papers, formulae, oral communications, blueprints, or patents (Table 1.1). No one has found a way to reduce this flow to a common denominator which could be used for inter-firm or inter-industry comparison. The most obvious method would be numbers of inventions and innovations, either unweighted or weighted by some kind of qualitative assessment.

The only statistics of numbers of inventions which are generally available are patent statistics, and ingenious attempts have been made to use these for various forms of comparison, including relative output by size of firm.

However, as already noted in Chapter 5 they are unsatisfactory for a variety of reasons, of which the main one is that firms and industries vary considerably in their propensity to patent. Some firms attach great importance to patents and have large departments with a strong interest in patenting activity, which will tend to inflate their inventive output, when measured in this way. Other firms either do not want to bother with patents or prefer to rely on secrecy. There has been a tendency to assume that large firms would have a higher propensity to patent than small firms and that consequently a measure of output of R&D based on patent statistics would understate the contribution of small firms. Since, in the United States, small firms show a much higher number of patents per dollar of R&D expenditure than large firms, this has been claimed as evidence of superior productivity of small firm R&D (see Rothwell and Zegveld, 1982).

However, Schmookler (1966, p. 33), the leading expert on United States patent statistics, presented convincing evidence for the view that, contrary to general belief, large firms in the United States have a lower propensity to patent than small ones. He based this on the empirically demonstrable effects of anti-trust actions on the patent policies of large firms, on the far greater possibilities of pretesting before filing of applications of large firms, and on the greater security of large firms in relation to patent sharing and know-how exchange arrangements. Small firms usually cannot afford not to patent and cannot afford to wait, so that patent statistics tend to exaggerate the contribution of the smaller firms to inventive output, and that of private individuals. This view was supported in Britain by the work of Pavitt (1982) and the analysis of the workings of the British patent system by Taylor and Silberston (1973).

The other major problem associated with patent statistics is the variability in importance of patents. One way of trying to get round this difficulty is by weighting patents, or by listing major inventions. The difficulty of these methods is that they are very time-consuming, unless they are confined to a small number of really outstanding inventions. In this case the difficulties which arise are those of subjective judgement in selecting the most important inventions, and of rating the relative importance of radical primary inventions, compared with the vast multitude of secondary improvement inventions. By far the best known example of this technique is Jewkes' study, which has already been discussed (Jewkes *et al.*, 1958) and which attempted to show that a majority of 70 major twentieth-century inventions were made outside the R&D departments of large firms. The US Department of Commerce study (1967) adopted an essentially similar view of the importance of private inventors and small firms, but with less empirical supporting evidence, and a tendency to confuse invention with innovation. Similar ideas were propounded earlier by Grosvenor (1929) and Hamberg (1966).

Jewkes' analysis may be criticized on the grounds that some important corporate inventions were omitted or, perhaps more justifiably, on the grounds that the contribution of large firms has become much more important since the 1920s. If his list of inventions is broken down, the share of corporate R&D is weak before 1930, but dominant since (Freeman, 1967, 1992). However, after making allowance for these criticisms, it must be conceded that Jewkes and his colleagues have made a strong case for the view that universities, private inventors and smaller firms have made a disproportionately large contribution to the more radical type of twentieth-century inventions. This was also confirmed by our own historical account.

9.3 SIZE OF FIRM AND INNOVATION

However, it does not necessarily follow that, because smaller firms may score better on numbers of patents or numbers of major inventions in relation to their R&D inputs, they are consistently more efficient in R&D performance than large firms. First, it has already been noted that a number of Jewkes' private inventions were in fact developed and brought to market by large corporations. Of the inventions made outside large firm R&D, perhaps about half were innovated in this way. The final aim of industrial R&D is a flow of innovations, so that efficiency in development is just as important as the earlier stages of inventive work. Indeed, it is often very difficult to say who made an invention because of the tangled chain of claim and counter-claim, but it is usually possible to say more precisely which firms made an innovation, in the sense of first launching a new product or process commercially. The relative performance of large firms is apparently better with respect to innovations than with respect to inventions, and Jewkes accepts that their role in development work (which is usually far more expensive) is much more important.

Thus, it may be reasonable to postulate that small firms may have some comparative advantage in the earlier stages of inventive work and the less expensive, but more radical innovations, whereas large firms have an advantage in the later stages and in improvement and scaling up of early breakthroughs. Moreover, there are significant differences between industries in the relative performance of small and large firms. In the chemical industry, where both research and development work are often very expensive, large firms predominate in both invention and innovation. In the mechanical engineering industry, inexpensive ingenuity can play a greater part and small firms or private inventors make a larger contribution. Patent statistics reflect these differences very clearly, and the point is fully confirmed by the results of the project described at the end of this section. However, it must be noted that in the case of computer software, on which patents could until recently not usually be taken out, the major contribution of small new firms may not be reflected in patent statistics.

As we have seen in Part One there are some types of innovation which are beyond the resources of the small firm. The absolute number of components is one factor which will affect this. The extreme case is Apollo XI, for which more than two million components were required, but there are other more mundane complex engineering products for which more than 10,000 components may be needed, such as advanced jet aero-engines, electronic telephone exchanges, large computer systems, nuclear reactors, or some process plant. Large firms also have a comparative advantage where there are several possible alternative routes to success, with uncertainty attached to all of them, but benefits from the simultaneous pursuit of several. Similarly, they enjoy an advantage where large numbers of different specialists are needed to solve a problem or expensive instrumentation is essential.

Table 9.5 Comparative advantage of types of firms in instrument innovation

Innovation process	*Established large firm*	*Recent small firm on second or subsequent products*	*Entrepreneur, first product*
Motivation to innovate	3	1–	1
Ability to have or develop own knowledge, technology	1	3	1
Cost advantages, using outside knowledge	2	3	1
Resources available to penetrate market	1	2–	3
Resources for new product development	1	3	1 or 2
Advantage in costs and speed of prototype and early model manufacture	3	1–	1
Flexibility to adopt new product or technology	3	2	1+
Cost advantage, large series production and marketing	1	2–	3

1 = highest comparative advantages, 3 = lowest comparative advantages

Source: Shimshoni (1970, p. 61).

Probably the greatest advantage of the small firm lies in flexibility, concentration and internal communications. SAPPHO suggested that greater concentration of management effort is important. Efficient coupling of marketing–production and R&D decision-making may be much more easily achieved in the small firm environment. In the discussion of the electronic scientific instrument industry reference has already been made to Shimshoni's work (1966, 1970). He found that new small firms had played a critical part in innovating several key instruments and postulated that their main advantages lay in motivation, low costs, lead time in development work (from speed in decision), and flexibility (Table 9.5). He also concluded that new firms had a major advantage in external economies in the form of technological expertise brought from elsewhere in the R&D system. In his studies of spin-off instrument firms, Roberts also pointed to the critical importance of technological entrepreneurs bringing with them ideas and half-developed new products from a scientific environment in university and government laboratories. Golding demonstrated this mechanism operating within the American semiconductor industry. The exceptionally important role played by new small firms and spin-off firms in the American semiconductor industry has led some observers to conclude that Schumpeter's 'Mark 1' is a more realistic picture of contemporary reality than his 'Mark 2' (i.e. that large firms do not predominate in the process of innovation – see Chapter 1). However, before jumping to this conclusion, it is important to keep in mind the following points: first that the larger corporations (Bell, GE, RCA, AT&T, etc.) did continue to contribute a large share of the key innovations – perhaps as much as half; second, that they accounted for more than half the key process innovations; third, that in

Europe and Japan, both the imitation process and the innovation process were dominated to a much greater extent by the large corporations. Rothwell and Zegveld (1982), while accepting that small firms enjoy some advantages in the innovation process, have also pointed out some of the disadvantages, such as access to finance, ability to cope with government regulations and lack of specialist management expertise.

How far is it possible to test systematically the relative contribution of small and large firms to innovation in various industries and the economy generally? While the evidence is still incomplete and the measurement problems remain formidable, there were several major advances in the 1970s and 1980s and projects carried out in both Britain and the United States enable us to give a fairly definite answer, even if it is not so detailed and precise as we might wish.

A project carried out at the Science Policy Research Unit in 1971 attempted to measure directly the number of innovations made by each of three size categories of firms in many branches of British industry (Freeman, 1971; Townsend *et al.*, 1981). The inquiry was carried out for the Bolton Committee of Inquiry on Small Firms, which defined small firms as those with fewer than 200 employees. The survey covered the period 1945–70, but later surveys, supported by the Research Councils (SERC and SSRC) added new information for the period 1971–83 (Pavitt *et al.*, 1987). Lists of important innovations were obtained from independent sources for each of a large number of different branches of industry. The innovations were then traced to the innovating firms, 90 per cent of whom were able to supply information on their size in terms of total employment at the time of the innovation.

On the reasonable assumption that the branches of industry included in the survey were representative of British industry as a whole, the most important conclusions were as follows:

1. Small firms accounted for about 17 per cent of all industrial innovations made between 1945 and 1983. This may be compared with their share of production and employment, which in 1963 amounted to about 19 per cent of net output and 22 per cent of employment.
2. The share of small firms in innovation was apparently fairly steady (Table 9.6) from 1950 to 1970 but rose quite steeply after this.
3. The share of the largest firms (10,000 employees and over) in the total number of innovations increased in the 1960s and 1970s but declined in the 1980s.
4. Over the entire period firms with a total employment of more than 1,000 accounted for two-thirds of all innovations.

9.4 INNOVATION BY SIZE OF FIRM AND BRANCH OF INDUSTRY

For the period from 1945 to 1970 the analysis by branch of industry showed big variations in the contribution of small firms to innovation. Industries may be classified into two fairly clear-cut groups:

1. Those in which small enterprises made little or no discernible contribution to innovation, either absolutely or relatively. These included aerospace, motor vehicles, dyes, pharmaceuticals, cement, glass, steel, aluminium, synthetic resins and shipbuilding (Table 9.7), and (in a special category) coal and gas. In this group small firms accounted for only just over 1 per cent of innovations (6 out of a total of 479), but about 8 per cent of net output in 1963.
2. Those in which small enterprises made a fairly significant contribution to innovation in the industry concerned. These included scientific instruments, electronics, carpets, textiles, textile machinery, paper and board, leather and footwear, timber and furniture, and construction. In this group small enterprises accounted for 103 out of 623 innovations, or about 17 per cent, compared with about 20 per cent of net output in 1963.

Table 9.6 Innovation share by size of firm in UK Industry 1945–83

	Size of firm					
Time period	*1–199*	*200–499*	*500–999*	*1,000–9,999*	*>10,000*	*No. of innovations*
1945–49	16.8	7.5	5.3	28.3	42.0	226
1950–54	14.2	9.5	4.5	32.2	39.6	359
1955–59	14.4	10.1	9.1	24.9	41.4	514
1960–64	13.6	9.2	6.0	27.8	43.4	684
1965–69	15.4	8.2	8.5	24.7	43.7	720
1970–74	17.5	9.0	6.3	20.7	46.5	656
1975–79	19.6	9.6	7.5	16.2	46.2	823
1980–83	26.3	12.1	4.3	14.9	41.9	396
Number of innovations	774	411	299	1,004	2,020	4,378
Average percentage	17.0	9.4	6.8	22.9	43.1	100

Source: Rothwell and Dodgson (1994).

If industries are ranked according to the share of small enterprises in the number of innovations for each industry, then this order corresponds fairly well with a measure of concentration based on share of small enterprises in net output (Table 9.7), but the contribution of innovations relative to net output share rises steeply.

In scientific instruments, some types of machinery and paper and board, small enterprises contributed proportionately more than their share of output to innovations. Medium-sized firms (employing 200–999) also contributed substantially to innovation in these industries.

Those industries in which small firms contributed much less than their share of output or nothing at all, correspond broadly to industries of high capital intensity. The major exceptions were aerospace, shipbuilding and pharmaceuticals. In these industries development and innovation costs for most new products are very heavy, although capital intensity is low. Paper and board was again an exception. Although this industry is generally one of relatively high capital intensity, small and medium firms have made important innovations, mainly in speciality products, and in board rather than paper. In these sectors capital intensity is lower.

Table 9.7 Share of small firms in innovations and new output of industries surveyed in UK

1958 SIC MLH number	1958 SIC title of industry	Per cent share of innovation by small firms 1945–70	Number of innovations by small firms 1945–70	Number of innovations by all firms 1945–70	Per cent share of net output by small firms 1963	Value of net output by all firms 1963 (£m)
471–3	Timber and furniture	39	7	18	49	220
351	Scientific instruments	28	23	84	23	154
431–3, 450	Leather and footwear	26	5	19	32	157
335	Textile machinery	23	15	65	21	65
481–3	Paper and board	20	6	30	15	317
339	General machinery	17	18	108	14	409
332	Machine tools	11	4	38	18	100
411–15, 417, 419, 492	Textiles, carpets	10	6	63	18	670
364	Electronics	8	13	160	8	320
211–29	Food	8	3	38	16	814
381	Vehicles, tractors	4	3	64	5	733
276	Synthetic resins and plastics	4	2	52	12	77
370	Shipbuilding	2	1	59	10	215
271(1)	Dyes	0	0	22	7	35
272(1)	Pharmaceuticals	0	0	44	12	124
463	Glass	0	0	13	14	96
464	Cement	0	0	18	0[a]	41
383	Aircraft	0	0	52	2	185
321	Aluminium	0	0	16	10[a]	100[a]
311–13	Iron and steel	0	0	68	9	630
101	Coal	0	0	23	0	655
601	Gas	0	0	15	0	216
500, 336, 337	Construction, earth-moving equipment and contractor's plant	12	4	33	53	1,931

[a] Estimated.

Source: Freeman (1971).

With this exception it seems to be true that in the capital intensive industries both process and product innovations have been mainly monopolized by large firms. This finding corresponds closely with the conclusions which emerge from Part One. The small firms made their contribution mainly in the field of machinery and instrument innovations, where both capital intensity and development costs are low for many products, and entry costs are low for new firms. This again corresponds closely to the historical account given in Part One. Machinery, instruments and electronics accounted for two-thirds of all the small firms' innovations reported. Although small firms made a significant contribution to innovation in such traditional industries as textiles, leather and furniture, the total number of innovations in these industries was relatively small. An important conclusion from this inquiry is that Jewkes was right in believing that the growth of professional R&D in the large corporation has not eliminated the contribution of small firms to industrial invention and innovation. But Galbraith was right, too, in believing that the larger corporation predominated in contemporary industrial innovation.

Since 1970 the increased share of small firms in total innovation in the UK was primarily in those industries where their contribution was already strong, especially electronics, instruments and computers. Research in the United States showed broadly similar results.

Whereas recent empirical research has demonstrated an increased share of total innovations coming from small firms in the 1970s and 1980s, history matters here too. In the past in the early stages of major new technologies, small firms make a disproportionately large contribution, but as a technology matures a process of concentration tends to take place, dominant designs emerge and lock-in may prevail. (See Figure 6.1 and other work of Utterback, 1993). Acquisitions and mergers are often motivated by the desire to spread the growing R&D costs and to gain control of R&D in competitive firms. There is already in the 1990s evidence of a renewed process of concentration both in ICT industries and in biotechnology, where the acquisition of NTBFs by large chemical and drug firms has been especially notable. Kaplinsky (1983) gave a good example of this process of 're-concentration' in the case of computer-aided design (CAD) firms. One other reservation should be made: the contribution of small firms may be exaggerated in some reports by a failure to distinguish clearly enough between small subsidiaries of large multinational firms or establishments and subsidiaries of domestic firms and the truly independent small firm.

But when all is said and done, there is still impressive support for the view that small firm innovations have genuinely increased their share of the total in the final quarter of the twentieth century. Whereas in previous waves of technical change, such an increase has not been sustained and the long-term trend towards concentration has set in again, the situation is somewhat different now, because of the trend towards networking identified in Chapter 8 and the growth in complexity of technology. These trends may be reinforced by the preference of many engineers and scientists to work in smaller and more intimate organizations. An important finding of Autio (1994) in his research on NTBFs in the Cambridge (Mass.) area, Cambridge (England) and Helsinki area was that the majority did not want to become larger or less specialized. The twenty-first century therefore may see a new form of symbiosis between large and small firms, rather than the concentration trend which we have documented in Part One for the nineteenth and twentieth centuries. This will be an important research topic in the twenty-first century.

9.5 CONCLUSIONS

This chapter has shown that statistical generalizations about size of firm, scale of R&D, inventive output and innovation need to be heavily qualified. Industry matters, technology matters and history matters. The role of small firms in the early stages of the evolution of an industry or a technology can be very different from the later stages. Simplistic generalizations about lower or higher concentration leading to better innovative performance cannot be sustained. True enough that competitive pressures may induce more innovation but these pressures can be as strong in a highly concentrated international oligopoly as in a predominantly small firm industry.

This means that competition policy is not so simple as it at one time appeared. An OECD survey by Symeonidis (1996) gives an admirably succinct summary of the conclusions which emerge from his study of 'Schumpeterian hypotheses':

> The present literature survey suggests that there seems to be little empirical support for the view that large firm size or high concentration are factors generally conductive to a higher level of innovative activity. Of course, once it is recognised that all these variables are endogenously determined, the emphasis shifts from causality to mere correlation. Again there is no evidence of a general positive association between innovation and market structure or firm size, although there are circumstances where a positive association exists. This implies that there is no general trade-off between competition policy and technical progress, although in some R&D-intensive industries a high level of concentration may be inevitable. . . . The range of sustainable levels of concentration in any given industry will depend on a number of industry-specific factors. These include technological characteristics, such as technological opportunity, the average cost of an R&D project, the degree of continuity and predictability of technology and the extent of learning economies; demand characteristics, such as the degree of horizontal product differentiation; and aspects of strategic interaction such as the intensity of price competition.
>
> (Symeonidis, 1966, p. 33)

Finally, it is worth remarking that although much of the literature on this topic refers to the 'Schumpeterian' hypothesis on the supposed advantages of large size for innovation, Schumpeter himself did not formulate such a hypothesis in a clear or unambiguous way. It is true that he did refer (1942) in rather a provocative way to large firm advantages but these can be taken to refer mainly to the fact that only large firms could undertake some very complex types of product and process development. In his earlier work (1912) he spoke mainly of the advantages of inventor–entrepreneurs and small firms and we have ourselves (Chapter 1) indicated the differences between his earlier ('Mark 1') model and his later ('Mark 2') model. Although he certainly pointed to the concentration of R&D in large

firms, he also indicated the dangers of bureaucracy in large organizations. Moreover, he did not have the advantages of being able to refer to the statistical evidence which only became available after his death. Consequently, the judgement which is often made that the Schumpeterian hypothesis has been refuted does not do justice either to the historical dimension of his work or to the complexity of the inter-industry and inter-technology differences of which he was aware.

NOTES

1. For definitions of R&D see OECD (1963a).
2. The whole problem of output measurement in R&D is dealt with more fully in UNESCO (1970), Irvine and Martin (1980, 1981), Martin and Irvine (1983), and Proceedings of OECD Conferences on Output Measurement (1980b, 1982); see also the special issue of *Research Policy* on output measurement, vol. 16, nos. 2–4 (1987).

CHAPTER 10

UNCERTAINTY, PROJECT EVALUATION AND INNOVATION

10.1 RISK AND UNCERTAINTY

The power of the giant corporation should not be exaggerated. From everyday observation as well as from Part One, and from the results of SAPPHO and other empirical studies, it is clear that many attempted innovations fail in large firms as well as in small firms. The assertions which are often made about the proportion which fail are rather unreliable for several reasons. Such generalizations are usually based on the experience of one firm or a few firms over a particular period. Moreover, they are usually vague about the criterion of failure. Thus the conventional wisdom of R&D management often refers to a success rate of one project in ten, or even one in a hundred. But everything here depends upon the stage at which such measurements are made. The higher figures often refer to the preliminary selection or screening process by which the less attractive R&D projects or proposals are weeded out before much money has been spent on them, and long before they reach the stage of commercial launch. Shelved research projects or development projects may be regarded as failed innovations in the early stages (Centre for the Study of Industrial Innovation, 1971) but the attrition rate is much higher in the R&D stage than after commercial launch. The SAPPHO project was concerned with attempts which reached this last stage.

Nevertheless, the failure rate is still high when it comes to this stage. This chapter discusses briefly the reasons for the high failure rate and some of the main difficulties confronting the firm in its project selection procedures. Finally, it concludes that it will be difficult to reduce this failure rate by better management of innovation or project selection and control techniques, except for the adaptive and imitative type of project.

This conclusion might appear to be at variance with the findings of SAPPHO and other projects designed to increase our understanding of innovation management. But it is important to recognize fully the limitations of such findings. Even if they are broadly correct in their interpretation of the characteristic pattern of success and of failure this is very far from providing a recipe or formula which will ensure success.

Insofar as the technical and commercial success of other innovators may affect the outcome of each attempt, some failure rate is almost inevitable when there are parallel or competitive attempts. Fuller knowledge about the conditions of success may raise the general standard of management in all attempts but it will not eliminate the possibility of failure where winners and losers are part of the game.

An analogy may be made with the management of football teams. Managers of teams are generally aware of what is needed to win a match. They have a fairly good idea of the pattern of success. So usually have their opponents. But it is by no means so easy to translate the ideal into reality on the field of play for many reasons, including the behaviour of the competition. What can be recognized ex post cannot always be controlled or initiated ex ante. Many of the variables involved are in any case not easy to manipulate.

It is true, of course, that there are some market situations where it is quite possible for several innovators to be successful simultaneously or nearly so. The success of one player does not necessarily mean the failure of another; there are some races where all can have prizes and others which are one-horse races. But even in the case of a monopolist or a socialist system of innovation, failures would persist for three reasons: technical uncertainty, market uncertainty and general political and economic uncertainty, sometimes described as business uncertainty.

The last category applies to all decisions about the future and it is generally assumed that a suitable discount rate applied to estimated future income and expenditure is the appropriate way to handle it in project evaluation. However, it will have bigger implications for innovations than for other types of investment to the extent that innovation projects have a longer time scale before the potential benefits can be realized. These implications are discussed at the end of this chapter.

The other types of uncertainty are specific to the particular innovation project and cannot be discounted, eliminated or assessed as an insurable type of risk. It is true that technical uncertainty can be very much reduced in the experimental development and trial production stages and that is indeed one purpose of these activities. But the outcome of these stages cannot be known before their completion, otherwise the work is not experimental and the activity is not truly innovative. Moreover, even after successful prototype testing, pilot plant work, trial production and test marketing some technical uncertainty still remains in the early stages of the innovation. As we have seen, one of the characteristics of successful innovators is the effort to get rid of bugs in the development stage. But some usually remain even in well-managed innovations, and occasionally they lead to serious setbacks some time after commercial launch. Some very expensive and well-known examples were the Comet jet airliner and Du Pont's Corfam (see Chapter 5).

Technical uncertainty is not merely a matter of 'work' or 'not work', although this is, of course, decisive for success. Indeed, the problem is very rarely reduced to this simple level. Much more usually it is a question of degree, of standards of performance under various operating conditions and at what cost. The uncertainty lies in the extent to which the innovation will satisfy a variety of technical criteria without increased cost of development, production or operation. Several processes for producing indigo dyes synthetically were technically successful without being commercially viable (see Chapter 4). The same was true of many of the hundreds of early cotton-picking machines.

The risk attached to technical innovation differs from normal risks which are insurable. Most economists, following Knight (1965), distinguish

Table 10.1 Degree of uncertainty associated with various types of innovation

1 True uncertainty	Fundamental research Fundamental invention
2 Very high degree of uncertainty	Radical product innovations Radical process innovations outside firm
3 High degree of uncertainty	Major product innovations Radical process innovations in own establishment or system
4 Moderate uncertainty	New 'generations' of established products
5 Little uncertainty	Licensed innovation Imitation of product innovations Modification of products and processes Early adoption of established process
6 Very little uncertainty	New 'model' Product differentiation Agency for established product innovation Late adoption of established process innovation and franchised operations in own establishment Minor technical improvements

between measurable uncertainty or risk proper and unmeasurable uncertainty or true uncertainty (see also Shackle, 1955, 1961). Technical innovation is usually classified with the second category. By definition, innovations are not a homogeneous class of events, but some categories of innovation are recognizably less uncertain than others (Table 10.1), and less risky. As Knight recognized, the classification of risk and uncertainty is a matter of degree except in the extremes. Life and fire insurances and other repetitive, calculable risks are usually cited as instances of the first type of risk which can be dealt with in a fairly straightforward manner by the theory of statistical probability, but uncertainty enters in even here. The second type of risk will not normally be assumed by insurance companies or indeed by banks. Special forms of financial institution have, therefore, been developed to handle this kind of uncertainty involving specific judgement in each individual instance.

Even the lower levels of uncertainty illustrated in Table 10.1 are such that only a very small proportion of R&D is financed directly by the capital market. Internally generated cash flow predominates. Where the risk is not borne by the firm or those fairly familiar with the individual project, usually either some type of cost-plus R&D contract is needed, or outright government ownership and finance of the R&D facility.

Numerous attempts have been made to deal with the uncertainty inherent in innovation by substituting subjective probability or credibility estimates for the relatively objective data used in estimating life insurance tables and other insurable risks (for example, Allen, 1968, 1972; Beattie and Reader, 1971). These attempts raise complex philosophical issues which will not be discussed here, but the empirical evidence will be examined to see how firms actually approach innovation decision-making and how far they are capable of statistically based estimation procedures.

It will be argued that the nature of the uncertainty associated with innovation is such that most firms have a powerful incentive most of the time not to undertake the more radical type of product innovation and to concentrate their industrial R&D on defensive, imitative innovations, product differentiation and process innovation. This proposition will be argued on theoretical grounds and the supporting empirical evidence will then be discussed. The distinction between inhouse process innovation and open market product innovation is very important here. Product innovation involves both technical and market uncertainty. Process innovation may involve only technical uncertainty if it is for inhouse application, and, as Hollander (1965) has pointed out, this can be minimal for minor technical improvements.

The general prevalence of uncertainty in R&D decision-making and the development of innovation strategies is clearly evident from the purely theoretical arguments of Keynes and Knight. However, it is also abundantly confirmed from the empirical evidence on firm behaviour. A particularly interesting example of this evidence is the study by Augsdorfer (1996) on *Forbidden Fruit: An Analysis of Bootlegging, Uncertainty and Learning in Corporate R&D*. He investigated the corporate R&D of more than fifty firms in France, Germany and Britain and found the widespread existence of 'bootlegging', i.e. 'under-the-table' covert research projects conducted by R&D personnel. He refers to many authors in the 1980s and 1990s, such as Roberts (1991), who referred to the existence of 'bootlegging' but Augsdorfer was the first to demonstrate not only its very widespread occurrence but also its significance in terms of the prevalence of uncertainty and the consequent extreme difficulty of 'rational' central planning of R&D by management. The tacit acceptance and even encouragement of bootlegging by some managers is another very interesting phenomenon. The existence of bootlegging can sometimes lead to more radical projects being initiated by adventurous individual researchers without the knowledge of management (Augsdorfer, 1994). Pearson (1990) explicitly relates bootlegging to high uncertainty in both means and ends. Another brilliant confirmation of the prevalence of uncertainty in the inventive and innovative process was the empirical research of Scherer (1997) on High Technology start-up companies.

10.2 PROJECT ESTIMATION TECHNIQUES AND THEIR RELIABILITY

Let us now consider the problems confronting the decision-makers in deciding whether to embark on an innovation project in the firm. Basically, they will be concerned, whatever particular selection technique they may favour or even if they operate purely on hunch, to make some estimate of three parameters:

1. The probable costs of development, production, launch and use of marketing of the innovation and the approximate timing of these expenditures.
2. The probable future income stream arising from the sale or use or the innovation and its timing.
3. The probability of success, technically and commercially.

Ideally, the decision-maker would like a complete cash-flow diagram of the future expenditures and income associated with the innovation

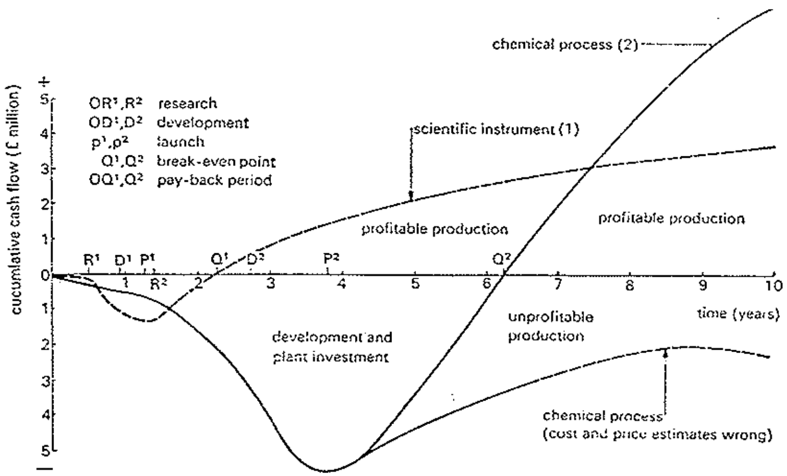

Fig. 10.1 Cumulative cash flow diagram

(Fig. 10.1). Some estimate of the development costs and subsequent launch costs is clearly essential to any kind of assessment of likely profitability, but as we know all too well from the publicized experience of aircraft development, these estimates can often be wildly wrong. Some improvements in estimating techniques can be made and a great deal of effort has gone into this, both in military and civil projects. But it must never be forgotten that estimates can only be really accurate if uncertainty is reduced, and uncertainty can only be significantly reduced either by further research or by making a project less innovative. Those firms who speak of keeping development cost estimating errors within a band of plus or minus 20 per cent are usually referring to a type of project in which technical uncertainty is minimal, for example, adapting electronic circuit designs to novel applications, but well within the boundaries of established technology, or minor modifications of existing designs (categories 5 and 6 in Table 10.1).

This conclusion was strongly confirmed by the empirical work which has been done on project estimating errors at the Rand Corporation (Marschak *et al.*, 1967; Marshall and Meckling, 1962; Mansfield *et al.*, 1971, 1977; Allen and Norris, 1970; Norris, 1971; Keck, 1977, 1970, 1982; Kay, 1979). Mansfield's work is particularly valuable because it permits comparison of different types of innovation. Although it must be conceded that it is very hard to measure just how radical any innovation is, it is nevertheless certain that new chemical entities constitute a more radical class of innovations than alternative dosage forms, and likewise that new products are normally a more radical departure than improved products. The differences in average cost and time overruns, as well as in the variance of estimates, are very striking (Table 10.2).

Table 10.2 Average and standard deviation of ratio of actual to estimated cost by project type and relative size of technical advance, 69 technically completed projects, US Proprietary Drug Laboratory

Project type	*Size of technical advance*		
	Small	*Large and medium*	*Total*
Product improvement			
Average	1.39	1.49	1.41
Standard deviation	1.39	1.64	1.41
Number of projects	28.00	5.00	33.00
New products			
Average	2.21	5.46	2.75
Standard deviation	3.56	5.86	4.11
Number of projects	30.00	6.00	36.00
Actual to estimated time for above			
Product improvement			
Average	2.80	1.74	2.64
Standard deviation	1.28	0.84	1.27
Number of projects	28.00	5.00	33.00
New products			
Average	3.14	3.70	3.24
Standard deviation	1.53	2.19	1.80
Number of projects	30.00	6.00	36.00

Source: Mansfield *et al.* (1971, pp. 102 and 104).

Mansfield's work is also extremely important because it confirms that large errors are not confined to the military sector or the aircraft industry. Moreover, his work on the chemical industry shows that estimating errors cannot be attributed to inexperience, as the firms which he investigated had long experience of project estimation and innovation, and were among the leading R&D performers in the US industry. The results do, however, suggest that there is some trade-off between cost and time, as the average overrun in military projects was much greater with respect to cost than time, whereas the opposite was true of civil projects, both in the USA and the UK. The work of Allen and Norris also suggests that time overruns were greater in research than in development. This trade-off was explored in some depth by Peck and Scherer (1962) in their work on the weapons acquisition process.

In addition to the very large errors involved, the tendency to optimistic bias is notable. This bias is present in other types of investment forecast, but not in such an extreme form. It suggests strongly that the social context of project estimation is a process of political advocacy and clash of interest groups rather than sober assessment of measurable probabilities. This view is confirmed both by historical accounts of individual innovation decision processes and by what little academic research has been done on the subject. Particularly important here was the work of Howard Thomas (1970) on project estimation in two scientific instrument firms. Not only did he find that engineers deliberately made very conservative estimates of development costs, but he also found that they did this in spite of strong financial incentives (including profit-sharing arrangements) to make 'honest' estimates:

> Many engineers in the firm admit quite freely that their estimates of cost and sales volume for projects are often biased in such a way that the resulting return factor estimates appear favourable to the firm. They point out that the procedures themselves are very inaccurate and do not incorporate the technical feeling about a project that an engineer often has, but is not necessarily understood by a finance or marketing man. So the engineer deliberately amends estimates (the means by which evaluations are made) in order to make the return factors acceptable to the firm. They do not do this to make projects personally and technically attractive to them more acceptable to the firm, because they are aware that the firm's financial interests and theirs are in one-to-one correspondence given the profit-sharing and preferential share-purchase plans offered by the firm as part of the remuneration package. Their sole motivation is to make the firm move towards more flexible numerical criteria for differentiating between projects.
>
> (Thomas, 1970)

While it is true that empirical evidence on project estimation is still not as comprehensive as we might wish, it must be regarded as persuasive support for the hypothesis that wide margins of error (with an optimistic bias) are characteristic of the experimental development process. This in itself must make innovation hazardous at least in relation to that part of the decision-making which precedes prototype test and pilot plant work. The evidence on project estimation in former socialist countries also pointed to the same conclusions.

Errors in development cost estimation alone may, of course, be sufficient to bankrupt a firm, if these costs account for a large proportion of its available resources. This occurred in spectacular fashion with Rolls-Royce in the 1970s and several smaller scale examples are cited in Part One. But as we shall see, the market uncertainty is frequently far greater than the technical uncertainty.

Seiler (1965) found that research managements of 100 large US firms rated 'probability of technical success' and 'development costs' as easier to estimate accurately than either 'probability of market success' or 'revenue from sales of product' (Table 10.3).[1] It is easy to think of several reasons why this should be so:

1. The market launch and growth of sales is more distant in time and may be spread over twenty years. A great many things can change during this time. This is partly a question of general business uncertainty relating to the future, but it is also specific to the project so far as it affects forecasts of consumer behaviour.
2. Whereas the development work is largely or entirely under the firm's own control, this is hardly ever true of the market, particularly in a capitalist economy. Economic theory is not capable of predicting the reactions of oligopolistic competitors in the face of innovation by one member of an oligopoly. Nor can the reactions of future customers or the trends of future legislation in relation to new products be safely predicted.

Table 10.3 Percentage of research managements of US firms rating accuracy with which factors affecting R&D can be estimated

	1	*2*	*3*	*4*	*5*	*4+5*
Factor	*Excellent*	*Good*	*Fair*	*Poor*	*Totally unreliable*	
Cost of research	3.5	27.8	52.2	14.8	1.7	16.5
Cost of development	2.6	38.8	46.6	9.5	2.5	12.0
Probability of technical success	3.5	51.3	39.9	6.3	0.0	6.3
Time to complete research	0.9	18.6	50.4	24.8	5.3	30.1
Manpower to complete research	2.6	34.2	53.5	7.0	2.7	9.7
Probability of market success[a]	3.6	33.6	38.2	14.5	10.1	24.6
Time to complete development	1.8	34.5	41.8	17.3	4.6	21.9
Market life of product[a]	4.6	28.0	29.0	23.4	15.0	38.4
Revenue from sales of product[a]	5.3	36.0	28.9	27.2	2.6	29.8
Cost reduction if R&D succeeds	10.7	57.1	14.3	14.3	3.6	17.9

[a] Assuming success of R&D

Source: Seiler (1965, pp. 177–8).

3. The prediction of future sales revenue and possible profit depends not only on forecasting total quantity which can be sold, but also on forecasting future costs of production, price and price elasticity. This is a formidable undertaking for a product not previously used by consumers.
4. Technological obsolescence may kill a new product or process almost as soon as it has been launched.

The empirical evidence, although unsystematic, confirms what theory suggests. Early estimates of future markets have been wildly inaccurate. As suggested in Part One, the major civil innovations in the past 80 years have been in the electronic industry and in synthetic materials. Almost every major innovation in these two industries was hopelessly underestimated in its early stages, including polyethylene, PVC and synthetic rubber in the materials field, and the computer, the transistor, the robot and numerical control in electronics.

As has already been shown in detail in Chapter 7, one of the most interesting cases is the computer. The early estimates almost all assumed that the market would be confined to a few large-scale scientific and government users. Even firms like IBM, as Watson Junior has confirmed, had no inkling of the potential until several years after electronic computers were in use. Optimistic estimates made in 1955 put the total US computer stock at 4,000 in 1965. The actual figure turned out to be over 20,000. Similarly large errors were made in underestimating the future potential applications of numerically controlled machine tools and robots.

Both of these, together with the examples of polyethylene and PVC, are cases of gross underestimation of future market potential for radical innovations. But there are also examples of gross over-optimism, for example in relation to the fuel cell, the airship, Ardil (the synthetic fibre), various nuclear reactors and the IBM STRETCH computer.

It may be said that the forecasting techniques in use before 1980 when many of these estimates were made, were still very primitive and that there are now much more sophisticated techniques which will reduce these errors. This remains to be seen, but the portents are not encouraging. There can have been few cases where more effort and expertise were devoted to market and cost estimation than in the case of Corfam. A computer model of the world market for hides, leather and shoes was developed, and a prolonged programme of manufacturer and customer trials with Corfam uppers. There are few firms with such an impressive record of product innovation as Du Pont, and they probably knew more about the shoe market than any firm in the industry by the time they launched Corfam. Yet they apparently lost about $100 million on this venture before they withdrew the product from the market.

If we consider the various innovations discussed in Chapters 2 to 7, it is difficult to think of any which worked out as originally expected. The gestation period was often far longer than the pioneers had anticipated (PVC, ammonia, TV, synthetic rubber, catalytic cracking, optical character recognition, indigo) and the development costs were frequently very much higher. The Concorde was probably the most spectacular example of gross underestimation of R&D costs and overestimation of the market in the 1960s and 1970s, with the result that, despite intense efforts by the British and French governments, they lost over £1,000 million and had to subsidize production, sales and airline operation as well as R&D. Even so, only a very small number of aircraft entered service.

10.3 ANIMAL SPIRITS AND PROJECT ESTIMATION

All of this is surprising only to those who believe that some new project evaluation technique or simulation technique would resolve the difficulties which are inherent in the very nature of innovation. Keynes was a great deal wiser. Although he was able to make a fortune on the stock exchange, and to write a treatise on the theory of probability as well as to revolutionize economic theory, he had no illusions about risky investments, whether speculative or innovative:

> Most, probably, of our decisions to do something positive, the full consequences of which will be drawn out over many days to come, can only be taken as a result of animal spirits – of a spontaneous urge to action rather than inaction, and not as the outcome of a weighted average of quantitative benefits multiplied by quantitative probabilities. Enterprise only pretends to itself to be mainly actuated by the statements in its own prospectus, however candid and sincere. Only a little more than an expedition to the South Pole, is it based on an exact calculation of benefits to come. Thus if the animal spirits are dimmed and the spontaneous optimism falters, leaving us to depend on nothing but a mathematical expectation, enterprise will fade and die – though fears of loss may have a basis no more reasonable than hopes of profit had before.
>
> It is safe to say that enterprise which depends on hopes stretching into the future benefits the community as a whole. But individual initiative will only be adequate when reasonable calculation is supplemented and supported by animal spirits, so that the thought of ultimate loss which often overtakes pioneers, as experience undoubtedly tells us and them, is put aside as a healthy man puts aside the expectation of death.
>
> (Keynes, 1936, pp. 161–2)

The uncertainty surrounding innovation means that among alternative investment possibilities innovation projects are unusually dependent on 'animal spirits'. But animal spirits are feared and distrusted by cautious decision-makers. As a standard textbook on investment decisions put it: 'Management will show a preference for projects with known outcomes over those whose outcomes are uncertain and the value of an investment proposal will be reduced according to its degree of uncertainty? (Townsend, 1969).

Often the substitution of subjectively estimated expected value for an objectively derived probability estimate rests on a false assumption which Keynes exposed, that 'an even chance of heaven or hell is precisely as much to be desired as the certain attainment of a state of mediocrity'. The risk (or uncertainty) aversion of entrepreneurs varies enormously of course, but we are on fairly safe ground in assuming that not many are prepared to gamble their survival on a fifty-fifty chance. Use of subjective probability estimates is usually acceptable only where possible outcomes are not extreme, and there is some repetition of previous experience.

This means that the acceptance of a high degree of uncertainty in innovation is likely to be confined to the following categories:

1. A few small-firm innovators who are ready to make a big gamble, or who are impelled to do so by some threat to their existence.
2. Large-firm innovators who use careful project selection methods but who can afford to adopt a portfolio approach to their R&D, offsetting a few very uncertain investments against a large number of mediocre projects. The size of the very uncertain investments will not usually be such that failure would threaten the continued existence of the firm.
3. Large-firm innovators who are not closely controlled by any formal project selection system and who are able to use corporate resources with a good deal of freedom, and hence impose their subjective estimates or preferences upon the organization.
4. Large- and small-firm innovators who unwittingly accept a very high degree of uncertainty, through 'animal spirits', because the enthusiasm of inventors, entrepreneurs, or product champions leads them on. In some cases (probably the majority) they may not bother to make any sophisticated calculations of the probable return on the investment. In others they may accept grossly over-optimistic subjective estimates of the probable outcome.
5. Government-sponsored innovators who accept very high risks because of urgent national needs (usually war, or threat of war) or a deliberate national science policy strategy, which creates an assured and profitable market in the event of success.
6. Government-sponsored innovators who accept grossly over-optimistic estimates of future returns for other reasons, where failure does not pose a serious threat to the decision-makers, as in the case of Concorde,

where diplomatic and prestige considerations were allowed for nearly twenty years to overrule commercial judgement and commonsense.

7. 'Bootlegging' individual researchers who initiate unofficial projects within a corporate R&D environment. Some of these may later become official projects.

The empirical evidence relating to the use of project selection techniques confirms that the more advanced portfolio methods which have been developed by statisticians and management consultants are seldom used. In the USA, Baker and Pound (1964) found that a few of the techniques had been used occasionally and then discarded in favour of simpler rule-of-thumb methods or discounted cash flow (DCF) calculations (see also Rubenstein, 1966). These methods are strongly biased towards short-term payback and the system in which they are used is frequently project based rather than portfolio based. Their widespread use probably discourages the more radical type of innovation, which would find more favour either in a fairly sophisticated selection system or without any very formal system. A survey in Sweden in 1971 confirmed that only simple quantitative methods were then used in Swedish industry and indicated some reasons for resistance to sophisticated techniques (Naslund and Sellstedt, 1972). Similarly, Olin's survey (1972) concluded that in the European chemical industry project selection remained a pragmatic and intuitive art.

A partial alternative to a quantitative cost benefit or DCF approach is to use a qualitative checklist method of evaluation. A checklist approach has the advantage of being able to take into account many factors which may be difficult to incorporate in a mathematical formula. For example, a critical factor in the success of any R&D project is the enthusiasm and capacity of the project leader and his or her other commitments. Another is the firm's resources of skilled people and accumulated know-how in the field and the possible spin-off from other R&D projects. A third may be the firm's relationship with potential customers and so forth. While all these factors may be taken into account by a research manager or an entrepreneur in calculating probability factors for technical or commercial success, a checklist procedure has the merit of compelling fairly systematic attention to be paid to each point. The actual checklist may be varied in accordance with the peculiar circumstances and characteristics of the firm (or other innovating organization), but the kind of questions that would tend to appear in most checklists would include the following (Dean, 1968; Seiler, 1965):

1. Compatibility with company objectives.
2. Compatibility with other long-term plans.
3. Availability of scientific skills in R&D.
4. Critical technical problems likely to arise.
5. Balance of R&D programme.
6. Interaction with other R&D projects.
7. Competitors' R&D programmes.
8. Size of potential market.
9. Factors affecting expansion of the market.
10. Influence of government regulations and control.

11. Export potential.
12. Probable reaction of competitors.
13. Possibility of licensing and know-how agreements.
14. Possibility of R&D co-operation with consultants or other organizations.
15. Effect on sales of other products.
16. Availability and price of materials needed.
17. Possibilities of spin-off exploitation of innovation.
18. Availability of production skills and equipment.
19. Availability of marketing skills and experience.
20. Advertising requirements.
21. Technical sales and service provision.
22. Effects on company image.
23. Risks to health or life.
24. Probable development, production and marketing costs.
25. Possibility of patent protection.
26. Scale and timing of necessary investment.
27. Location of new or extended plant(s).
28. Attitude of key R&D personnel.
29. Attitude of principal executives.
30. Attitude of production and marketing departments.
31. Attitude of trade unions.
32. Overall effect on company growth.

Some firms may also take into account additional external costs and benefits, such as problems of waste disposal or employment effects, retraining requirements and contributions to research outside the company. The consideration of some environmental effects has become far more widespread in the 1980s and 1990s and indeed has become a legal obligation in some countries (see Chapter 18). Firms may also find even more valuable those types of technique which attempt to foresee and avoid all the conceivable bugs and blockages which could frustrate the future progress of the project (Davies, 1972). Finally, still other types of checklist and portfolio approaches go even wider and relate the solution of the project to the overall strategy of the firm, both with respect to technology and other objectives (see, for example, Patterson, 1996; Kanz and Lam, 1996).

Although the checklist approach permits consideration of many factors which may be disregarded or overlooked in a quantitative analysis, it too has serious limitations. It does not permit easy comparison between alternative projects or ranking of a list of projects, nor does it provide any indication of the likely absolute size of the pay-off. Since most firms have a backlog of projects from which to choose, these are serious defects. Consequently, the ideal method of project selection is probably a combination of a quantitative cost–benefit approach with a qualitative checklist approach. Several such scoring systems have been developed. Figure 10.2 illustrates that originally worked out for project evaluation at Morganite by Hart (1966). Hart's system was based on calculating a project index value, which takes account of estimated peak sales value, net profit on sales, probability of R&D technical success and a time discount factor in relation to future R&D costs according to the formula:

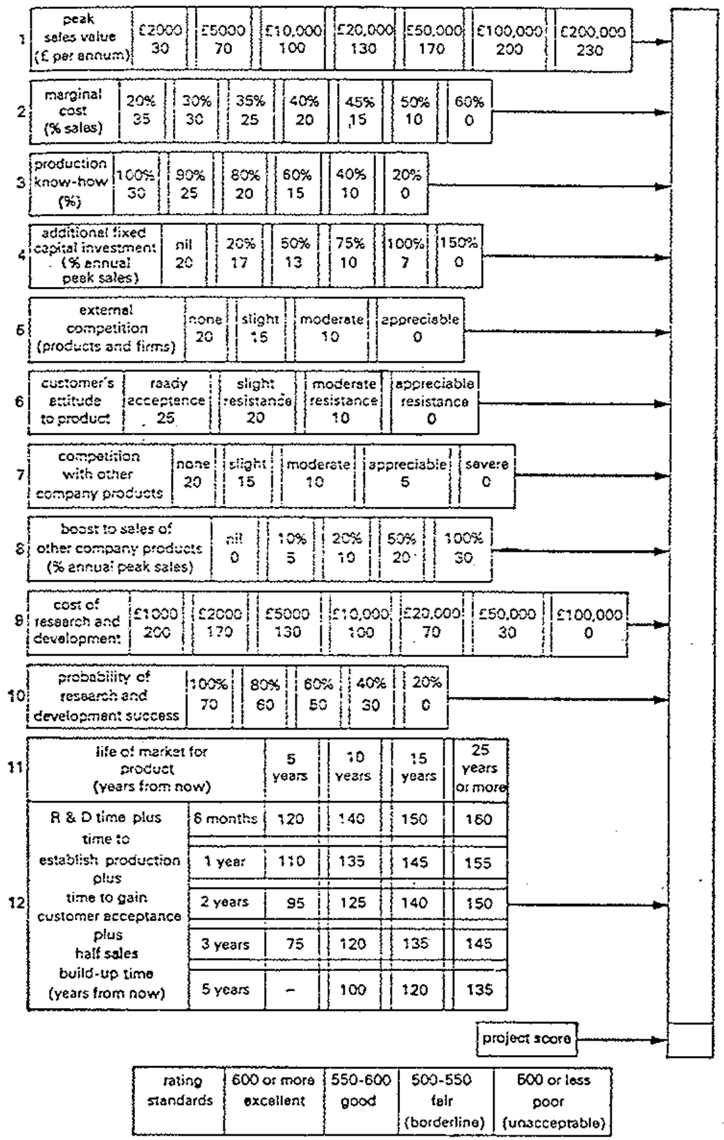

Fig. 10.2 Evaluation chart for product research and development projects

Source: Hart (1966).

$$I \text{ (index value)} = \frac{S \times P \times p \times t}{100\ C}$$

where S = peak sales value £ per annum,
P = net profit on sales (per cent),
p = probability of R&D success on a scale 0 to 1,
t = a time discount factor,
C = future cost of R&D (£).

Estimates for the variables are obtained by answering a checklist of questions. Figure 10.2 shows a score for each possible answer to the question and permits scoring by addition rather than multiplication by using logarithmic functions of the answers. The method can be varied by using different questions and different scores to suit the circumstances of a particular firm.

The advantage of this method is that it permits the firm to take into account such factors as external competition and customer attitudes, yet at the same time ranks projects on some systematic basis, on the assumption of sales growth and high profitability as major company objectives. Another advantage of the technique is that it can be used to involve all departments of the firm in discussion and evaluation, thereby contributing to mobilization and integration of the firm's resources. This is probably the main benefit of any formal evaluation technique. Indeed, some type of formal technique is usually necessary simply to monitor and control the progress of a project. The periodic revision of estimates and reconsideration of projects is essential for effective management of technical innovations.

However, it is apparent that one of the major factors affecting the selection of a project is the balance of work in the R&D department and in the firm as a whole. Consequently, project selection must be related to programming. What management is looking for is a portfolio of projects rather than a series of separate projects. By thinking in terms of a portfolio rather than a project it is possible to select a blend of safe and high risk projects, so that the more long-term and radical advances are not ignored as they would tend to be if selection were based entirely on a scoring system or rate of return system (Kay, 1979, 1982, 1984).

The empirical evidence confirms that industrial R&D is heavily concentrated on the less uncertain types of project. Only a few firms perform any basic research and this accounts for less than 5 per cent of all industrial R&D expenditures in most OECD countries. Several surveys have confirmed that the bulk of R&D expenditures are devoted to minor improvements and quick payback projects, rather than the more long-term radical innovations (FBI, 1961; Schott, 1975; Kay, 1979, 1984; Nelson *et al.*, 1967). Table 10.4 shows the results of an Italian industrial R&D survey based on data supplied by individual researchers. This confirms the involvement in technical service activity and the relatively low proportion of time spent on fundamental research. It must be remembered that in terms of expenditure (as opposed to time of researchers) the share of experimental development would be higher and that of research lower.

Although not directly related to the distribution of R&D expenditures, the study of Gibbons and Johnston (1972) provided additional interesting

Table 10.4 Distribution of the working time devoted by researchers to various activities, broken down by industry, type of researcher and level of responsibility

	Researchers no.	% of time devoted to							
		Individual R&D	Group R&D	Studying and monitoring	Teaching	Organization, administration	Technical services	Advertising, other functions	Other
Food and textiles	187	18	21	11	2	25	16	6	1
Metallurgy	264	11	29	15	2	11	26	7	0
Mechanical eng.	1,669	23	20	10	2	17	20	6	2
Electrical eng.	644	17	24	12	2	22	15	6	1
Electronics telecom.	3,056	20	27	12	4	15	16	6	1
Transport vehicles	1,209	22	23	11	3	11	19	8	2
Pharmaceuticals	2,139	22	28	19	3	13	10	3	0
Chemicals	2,328	17	29	14	2	15	14	7	1
Rubber and fibres	868	18	32	12	1	16	15	6	0
Research companies	1,083	22	33	15	3	10	10	5	0
Other manuf. sectors	1,564	16	22	14	2	8	25	9	4
Total	15,010	20	26	13	3	14	16	6	1
% researchers who perform the activity	—	72	83	81	18	60	55	42	6
Scientists	3,248	33	39	14	1	6	4	3	0
Engineers	11,762	16	23	13	3	16	20	7	2
Junior researchers	5,959	27	26	14	2	8	15	5	3
Project leaders	7,154	16	29	13	3	15	17	7	0
Directors	1,808	10	18	13	4	27	18	9	1

	Researchers no.	Distribution of R&D time (%)						Type of researcher		
		Fundamental research	% of researchers who perform the activity	Applied research	% of researchers who perform the activity	Experimental development	% of researchers who perform the activity	Scientists (%)	Engineers (%)	Average age
Food and textiles	187	9	53	34	94	57	94	0	2.4	40.4
Metallurgy	264	2	18	49	94	49	94	1.4	2.0	42.2
Mechanical eng.	1,669	10	41	56	100	35	83	6.9	6.0	37.0
Electrical eng.	644	8	28	55	88	37	67	4.1	6.7	31.8
Electronics telecom.	3,056	8	30	41	86	51	85	10.1	20.8	36.2
Transport vehicles	1,209	8	44	51	91	41	82	12.4	11.3	35.9
Pharmaceuticals	2,139	17	62	55	94	28	68	31.2	15.9	36.2
Chemicals	2,328	11	43	43	86	45	83	11.9	14.8	40.5
Rubber and fibres	868	10	39	51	93	39	80	6.0	3.9	38.3
Research companies	1,083	10	47	53	93	38	77	13.8	10.1	35.1
Other manuf. sectors	1,564	8	35	43	98	49	96	2.3	6.0	38.4
Total	15,010	11	43	49	91	41	79	100.0	100.0	37.4
% researchers who perform the activity	—	—	—	—	—	—	—	—	—	—
Scientists	3,248	16	57	69	98	15	58	100.0	—	35.4
Engineers	11,762	9	39	43	89	48	86	—	100.0	37.8
Junior researchers	5,959	12	46	49	88	38	76	61.5	33.7	34.1
Project leaders	7,154	9	40	48	93	43	82	34.2	51.4	38.1
Directors	1,808	11	45	50	93	39	83	4.3	14.9	43.4

Source: Sirilli (1982).

Table 10.5 Distribution of R&D activities in Italy

	Percentage
Expansion of range without technical modification	17
New application of existing product	2
Standard product with new specification appropriate to particular application (e.g. portable)	29
Conforming to new standard (e.g. metric)	2
Standard product made easier to use	6
Standard product, new marketing	2
Standard product, new design	2
Products developed outside UK	23
New products, involving technical change and developed by UK firms	18

evidence. They attempted to derive a random sample of innovations by listing all new product announcements which appeared in UK technical journals on a selected date in 1971. They found that when they examined the list of 1,317 products, after eliminating 258 duplications, 32 process or service innovations, and 16 of non-industrial origin, the remaining 1,000 new products broke down as shown in Table 10.5.

When they came to examine in greater detail a sample of the last 18 per cent, which were those relevant for their purpose, they found that half of this restricted sample could be described as modifications of existing products of the company.

There are grounds for believing that firms are more ready to attempt radical innovations in relation to their own processes than in relation to their products. This includes, of course, the adoption and modification of the product innovations of the capital goods industries. The market uncertainty is very much reduced with inhouse process innovations as the firm controls the application. For similar reasons, much more radical product innovations may be expected in response to an assured market (whether government or otherwise) than on a competitive market. This was clearly evident in the development of radar and synthetic materials as well as in military aircraft.

If the process which is developed cannot be used by the firm itself, then the uncertainty is, of course, greater. This difference accounts largely for the respective approach to process innovation of the chemical plant contractor, as compared with the chemical firms themselves. As we have seen in Chapter 4, the contractors, who cannot use the process themselves but have to face an extremely uncertain market, tend to concentrate on improvements in design and scaling up. They usually attempt completely new processes only in association with a chemical or oil firm. The chemical and oil firms, on the other hand, have a strong incentive not only to make improvements in their inhouse processes, but to explore radically new processes for use in their own establishments, hoping that they can keep the innovation sequence largely under their own control. The introduction of such a new process may be used to lower production costs and increase profitability by comparison with competitors, or to lower product prices and expand markets. In some cases introduction may be retarded to preserve the profitability of existing investment. If the process can ultimately be licensed to other firms, this may be regarded as an additional bonus, but the successful marketing (licensing) of the process is not usually essential, as it is in the case of the contractor-originated process. Indeed, there may be a deliberate preference for secrecy and not licensing.

Research in the 1980s and 1990s has demonstrated that potential appropriability of the revenues from a new product or process plays an important role in project evaluation but that this varies very considerably between industrial sectors. This emerged both from the systematic Yale survey of innovations (Levin, 1988; Levin *et al.*, 1987) and from the later even more comprehensive PACE Survey (Arundel, 1995) in several European countries.

10.4 R&D BUDGETING AND THE STRATEGY OF THE FIRM

By undertaking R&D work mainly with a relatively low degree of uncertainty (Table 10.1), the firm is in effect using its R&D budget as a form of insurance against the risks of technical change. Or, as Arrow (1962) puts it, 'the Corporation acts as its own insurance company'. Management often actually bases its R&D budget on a percentage of sales calculation. This insurance premium varies in different branches of industry depending upon the intensity of technological competition, but the level of expenditure is often fairly uniform among many firms in each branch. Although management cannot calculate accurately the return on any individual project or piece of R&D, it has learnt from experience and from observation of competitors that this 'normal' level of R&D spending will probably help it to survive and grow. However, there is room for a variety of alternative strategies and these are discussed more fully in the next chapter. Some firms may spend much more heavily on R&D than is usual for their industry branch and follow a high-risk offensive strategy. Others may try to get by with very little R&D, or none at all, relying on other sources of competitive advantage. Threshold factors complicate the budgeting problem still further, and the factors discussed in Chapter 7 illustrate some of the dangers of R&D budgeting by an industry average. Naslund and Sellstedt (1972) produced evidence to show that in Swedish industry many firms allocated funds to R&D on an ad hoc project basis, rather than as a stable regular budget. But Kay (1979) argues persuasively that these differences in behaviour may be largely a function of size of firm. The Swedish firms were mainly small ones but the larger European and American firms typically follow a more long-term budget strategy.

Between industries wide variations in research intensity continue to exist, and it may be postulated that they are attributable on the one hand to historical circumstances (new technological opportunities), and on the other to the varying pressures of competition. In an industry in which new processes or new generations of products emerge every ten years or so, a moderately high level of research intensity would be necessary to avoid obsolescence of the product range or excessive costs (drugs, instruments, machinery, vehicles). Although the individual firm in such an industry might increase profitability for a few years by cutting back on R&D, this would be at the expense of long-term profitability and survival. A very low level of R&D activity, or none at all, would be a viable strategy in those branches of industry where technological obsolescence is not a problem, or where changes in product range are mainly fashion based. An extraordinarily high level of R&D activity might be necessary for survival in industries such as aircraft and electronics where an artificial stimulus to obsolescence derives from military R&D and procurement as well as rapid changes in world science and technology. In the extreme case, if it works, it's obsolete. The fact that one-third of the net output of the aircraft industry in the United States and the United Kingdom was actually R&D for long periods can only be explained in these 'Alice in Wonderland' terms. Thus the main determinant of research intensity is the branch of industry, and the ranking of industries by intensity is similar in all industrialized countries. Nelson (1991) and Pavitt (1984) are among the authors who have most strongly emphasized the importance of this variation by industry.

The outcome of the individual projects with a high degree of technical and market uncertainty cannot be precisely foreseen, either by the firms or by anyone else. Otherwise they would make fortunes more easily and enjoy a high and relatively stable rate of growth. But within an industry branch it is much more likely that someone will succeed in making the big advances, even though we cannot predict exactly which firm. Thus the uncertainty attached to R&D would lead one to expect a stronger statistical association between R&D spending and the growth of an *industry* than between R&D spending and the growth of the individual *firm*. This is in fact what the empirical evidence does show.

The most research intensive industries are, by and large, those with the highest growth rate (Fig. 7.1), whereas industries with little R&D are on the whole relatively slow-growing or stagnant. But within a fast-growing, research intensive industry, such as electronics or pharmaceuticals, there is not such a strong association between high growth and research intensity by firm. Some of the empirical data suggest a weak correlation and some suggest none at all. This result could be expected not only on grounds of the high degree of uncertainty surrounding the outcome of expensive offensive projects in any individual firm, but also on grounds of externalities which continually arise in R&D. Many firms in an industry may benefit from the technical progress made in only one or two or in a different industry altogether. The whole electronics industry benefited from Bell's work on semiconductors, but only a small part of this benefit was recovered by Bell in the form of licence and know-how payments or indeed in sales. Whereas the patent system strengthens the possibility of appropriating the benefits of knowledge gained through inhouse R&D, it cannot and does not prevent the diffusion of this information through a variety of channels, particularly the movement of people as in the American semiconductor industry.

A much stronger association might be expected between firm growth and some combined measure of R&D with Scientific and Technological Services (STS). This would capture the important productivity growth attributable to minor productivity improvements of the type described by Hollander. The empirical results of Katz confirm this hypothesis and so too does some work on the distribution of qualified personnel and growth of firms.

Firms recognize the need to perform R&D in order to stay in business or retain their independence, but there is no recipe for successful innovation.

This is one of the main factors contributing to 'higgledy-piggledy' or 'Tolstoyan' patterns of growth. Among the characteristics of successful innovation are the capacity of the innovator to couple efficient R&D with knowledge of the market requirements. But this is more obvious ex post than ex ante. Burns and Stalker in a classic study (1961) showed the internal difficulties within the firm in achieving the necessary degree of integration of these functions. Since innovation is often a complex of events extending over several years, the coupling process is a continuous one and is liable to be severely strained by internal problems within the firm as well as by extraneous events. The process is one of groping, searching and experimenting and even the best laid plans may come to grief. A firm with an efficient R&D set-up is more likely to survive, but it is by no means sure to do so. Even its own innovations may increase the general instability and uncertainty, so that they will often be unwelcome within the firm itself. Project SAPPHO showed that typically there was opposition to an innovation within the firm both on commercial and technical grounds. A greater degree of opposition on commercial grounds was quite strongly associated with failure of the innovation.

The existence of conflict in relation to R&D decision-making and the uncertainty inherent in the process mean that selection and forecasting procedures are not always what they appear from formal descriptions of the methods. A great deal has been written about various modes of technological forecasting and their application in American industry (Jantsch, 1967). Such TF techniques can undoubtedly be very useful and Bright (1968) in particular demonstrated their value in company strategy in identifying new technological opportunities and threats. However, as with other management techniques, the reality differs from the impression given by enthusiastic advocates. In view of the importance of recognizing what really happens in industry, and distinguishing this from idealized abstract concepts, the conclusions of one survey are quoted at some length:

> Since some companies could probably benefit from formal technological forecasting but do not practice it, we searched within the organization for factors that inhibit its use. We found these common management-oriented obstacles to the use of this technique:
>
> 1. *Failure to integrate technological forecasting into the organization's regular plans.* Whereas most managers support the viewpoint that the most critical factor in implementing any forecasting technique is its integration into a long-range planning program, including the selection of research projects and the allocation of resources consistent with overall corporate objective, this is most often not the case in practice.
>
> More typical is the experience of the executive who was transferred into the advanced planning group of his company with the task of instituting a formal forecasting program to simplify the planning process. . . . There had been no attempt to apply forecasting to the technological future of the company's major product line, and hence his efforts had had no impact on planning.
>
> In another company, one individual with a technical background developed an interest in sophisticated technological forecasting and, with the support of the corporate vice-president for research, had been developing descriptive reports for more than a year. In addition to preparing reports on techniques, he also

addressed the R and D planning process, with special attention to the problems of integrating technological forecasting with planning. Yet we found no evidence that anyone was using these techniques for decision making contrary to reports by Jantsch on the same organization. The company's efforts represent the work of one man who had hopes for the future, but has met with little success to date in selling his ideas.

2. *Failure to objectively select research and development projects.* In most of the companies we studied, the planning and control of R and D expenditures appears haphazard at best. An objective, factual assessment of the economic benefits, direct and indirect, of R and D investments seems to be very much the exception. While part of this is a natural outgrowth of the inevitable uncertainty of the task and the necessary flexibility and informality that characterize most research activities, it also represents management's failure to deal adequately with the planning and control process. The R and D project-selection process observed was primarily one of 'advocacy', based on the personal interests of researchers, the pet projects of key administrators, and a variety of other criteria which could be at odds with the strategic interests of the company.

In one company major R and D decisions are determined by internal power dynamics, which has led to a considerable amount of 'hobby work', or unauthorized research on pet projects. In another, funds are allocated by function or discipline on the basis of advocacy and power, even though it is recognized that individual product allocations might provide a more solid basis for planning. Despite the need for an objective cost-justification of research projects, we found little incentive among R and D decision makers for either planning or forecasting of technology.

3. *Failure to understand the role of sophisticated management techniques.* A further aspect of managerial resistance to technological forecasting (and to other management techniques) results from a fear of the unknown, a concern that decision-making prerogatives are being pre-empted, and/or the fear that systematic decision-making techniques may uncover incorrect decisions made in the past. In addition, the adoption of sophisticated forecasting is likely to further complicate the planning task rather than simplify it.

4. *Failure of top management to support forecasting efforts.* The support of top management is a requisite for many major changes, but we found few top managers supporting technological forecasting, and none initiating it. Initiation generally came from one man with the right background, interest and motivation, but without the influence necessary to establish his technological forecasting ideas.

The staff-line barrier is another aspect of this problem, manifested by a corporate staff trying to sell the technique to a divisional planning group, which, in turn, is asked to sell it to the divisional management.

5. *Failure of divisional management to look far enough ahead.* A final management impediment to technological forecasting is the short time perspective of line decision makers in profit-controlled divisions. The pay-off from technological forecasting is often in the long run, and, as one director of technology planning noted, 'The big corporation has no memory for the long-term investment'.

(Dory and Lord, 1970; Roberts, 1968)

Many other empirical surveys could be cited to confirm these conclusions, notably Olin (1972). They support the general arguments of Nelson and Winter (1977), Dosi (1984, 1988), Downie (1958), Gold (1971, 1979) and

Marris (1964) on the theory of the firm. Downie was concerned to explain why it was that the process of concentration in more efficient firms did not proceed more rapidly and more 'rationally' since big inter-firm differences in efficiency were clearly apparent. His explanation was in terms of 'unexpected' success of innovations in firms which had fallen behind. Thus the 'innovation mechanism' offset the efficiency 'transfer mechanism', constantly changing the relative position of competitive firms. Marris was arguing that growth maximization was a more realistic explanation of firm behaviour than profit maximization. He postulated, however, that such growth policies were subject to a profits constraint. Insofar as R&D is regarded mainly as a force contributing to growth and survival, its spread may be associated with the type of professional management attitudes which he identified as characteristic of the modern corporation, but in so far as they are unable to estimate likely profitability, it will remain higgledy-piggledy, with the profit/survival constraint sometimes weeding out the unlucky as well as the inefficient.

Moreover, the empirical evidence confirms that decision-making in relation to R&D projects or general strategy is usually a matter of controversy within the firm. The general uncertainty means that many different views may be held and the situation is typically one of advocacy and political debate in which project estimates are used by interest groups to buttress a particular point of view. Evaluation techniques and technological forecasting, like tribal war dances, play a very important part in mobilizing, energizing and organizing.

Although a defensive R&D strategy may be regarded as the typical response of the firm in research intensive branches of industry, it is by no means the only possible response. Particularly in countries where science is in any case underdeveloped, such a strategy may not be a realistic possibility. Even in the case of firms in advanced economies, managements of some firms may prefer a strategy of imitation, dependence or even suicide. These may be the only realistic alternatives where strong military or civil demands from government provide a powerful artificial stimulus in particular sectors of the world market, or where multinational corporations enjoy overwhelming advantages in scale of R&D, dynamic economies and market power.

The possibilities open to the firm will be considerably affected by national innovation policies. From the historical account in Part One it was evident that the growth of synthetic materials in Germany and of electronics in the USA were intimately related to government policies. By greatly reducing both the technical and the market uncertainty, governments provided a very powerful stimulus to industrial innovation. The profitability constraint (or profit maximizing behaviour for those who prefer this assumption) means that the time horizon of most firms in their decision-making is relatively short. This inevitably militates against long-term strategies, so that the advocates of long-term R&D policies in the firm will usually be at a disadvantage, unless they have external support of this kind. This is of great importance in considering such problems as pollution, energy conservation and resource depletion (see Part Four).

For this reason (the uncertainty and long-term nature of radical R&D), it must be expected that there will be a tendency in a private capitalist economy to underinvestment in long-term research and innovation, in spite of the potential advantages which the individual firm may gain. This underinvestment will be greatest in fundamental research and the more radical types of innovation (Nelson, 1959; Arrow, 1962). It is largely for this reason that in capitalist and the former socialist economies alike, governments finance most fundamental research and a certain amount of radical innovation (see Chapter 16). Conversely, there may well be overinvestment in short-term R&D associated with product differentiation and brand image. In conditions of oligopoly the firm strives to reduce market uncertainty by differentiating the market for its own products through a combination of advertising and minor technical changes. It does not necessarily follow, however, as Arrow and other economists have suggested, that a more perfect innovation system would separate R&D from the firm. The evidence from SAPPHO as well as most other innovation studies confirms the view that the coupling entrepreneurial function of the firm can be most efficiently performed if it is active in R&D itself. Successful innovation depends on combining technical with market knowledge. There is also the negative evidence that those socialist economies which initially separated industrial R&D from the enterprise were generally revising these policies in the 1970s in the direction of greater emphasis on enterprise-level R&D. The debates on the 'core competences' of firms (Teece, 1986; Prahalad and Hamil, 1990) generally led to the conclusion that there were major advantages for those firms which were able to integrate their manufacturing know-how with their R&D. Some economists explained this in terms of transaction cost theory (Williamson, 1975; 1985) but sociological explanations also carry conviction (Mowery 1983).

The overall picture which emerges from this survey of uncertainty and project selection in relation to innovation is rather more Tolstoyan than the neoclassical theory of the firm tends to assume. Most firms are unable to make very rational calculations about any one project, because of the uncertainty which is inherent in the process, because they lack the information necessary for rational behaviour and because they lack the time and the inclination to get it or to use very complex methods of assessment. This means that growth is higgledy-piggledy and that no one foresees very clearly the outcome of their own or their competitors' behaviour. If anyone doubts this let them consider the behaviour of the firms involved in the United States and European computer industries between 1950 and 1990, or in the radio industry between 1900 and 1930. Nevertheless, the social benefits and costs arising from this untidy innovative process can be very great. We now turn to a consideration of the various strategies which are open to the firm in the face of this degree of technical and market uncertainty, and in Part Four we consider the problem from the standpoint of national policy.

NOTE

1. One can only agree with Mansfield's ironic comments on the apparent optimistic self-deception of US research managements in believing that 'good' estimates could be made of many of these factors. The point here, however, is the relative accuracy.

CHAPTER 11

INNOVATION AND THE STRATEGY OF THE FIRM

11.1 THE RANGE OF INNOVATION STRATEGIES

Even though the survival and profitability constraints are obviously of the greatest importance in explaining firm behaviour, we conclude from Chapter 10 that rational profit maximizing behaviour (or growth maximizing) is seldom possible in the face of the uncertainties associated with individual innovation projects. This is not to deny that neoclassical short-run theory is a valuable, precise, abstract model of firm behaviour, but it means that this model has limited relevance, and that other ways of interpreting and understanding innovative behaviour are needed (Nelson and Winter, 1977, 1982; Dosi *et al.*, 1988). One possible approach to such a theory (and it is no more than a first approach) is to look at the various strategies open to a firm when confronted with technical change. Such an approach does not look to an equilibrium which is never attained, but does take into account the historical context of any industry in a particular country. This chapter classifies some possible strategies, and discusses them in relation to R&D, and other innovative activities of the firm.

Any classification of strategies by types is necessarily somewhat arbitrary and does violence to the infinite variety of circumstances in the real world. The use of such ideal types may nevertheless be useful for purposes of conceptualization, just as the use of the concepts of extrovert and introvert is useful in psychology. In practice there is an infinite gradation between types, and many individuals possess characteristics of both types. Moreover, individuals (and firms) do not always behave true to type. Finally, people and firm strategies are always changing, so that generalizations which were true of a previous decade will not necessarily be true of the next. For example, information and communication technology is leading to many changes in the behaviour of firms, especially with respect to their external networks of relationships and their collaboration with other firms (Coombs *et al.*, 1996; Hagedoorn and Schakenraad, 1992).

Any firm operates within a spectrum of technological and market possibilities arising from the growth of world science and technology and the world market. These developments are largely independent of the individual firm and would mostly continue even if it ceased to exist. To survive and develop it must take into account these limitations and historical circumstances. To this extent its innovative activity is not free or arbitrary, but historically circumscribed. Its survival and growth depend upon its capacity to adapt to this rapidly changing external environment and to change it. Whereas traditional economic theory largely ignores the complication of world science and technology and looks to the market as the environment, changing technology is a critically important aspect of the environment for firms in most industries in most countries.

Within these limits, the firm has a range of options and alternative strategies. It can use its resources and scientific and technical skills in a variety of different combinations. It can give greater or lesser weight to short-term or long-term considerations. It can form alliances of various kinds. It can license innovations made elsewhere. It can attempt market and technological forecasting. It can attempt to develop a variety of new products and processes on its own. It can modify world science and technology to a small extent, but it cannot predict accurately the outcome of its own innovative efforts or those of its competitors, so that the hazards and risks which it faces if it attempts any major change in world technology are always present.

Yet not to innovate is to die. Some firms actually do elect to die.[1] Firms which fail to introduce new products or processes in the chemical, instruments or electronics industries cannot usually survive, because their competitors will pre-empt the market with product innovations, or manufacture standard products more cheaply with new processes. Consequently, if they wish to survive despite all their uncertainties about innovation, most firms are on an innovative treadmill. They may not wish to be offensive innovators, but they can often scarcely avoid being defensive or imitative innovators. Changes in technology and in the market and the advances of their competitors compel them to try and keep pace in one way or another. There are various alternative strategies which they may follow, depending upon their resources, their history, their management attitudes, and their luck (Table 11.1).

They differ from those which are normally considered in relation to the economist's model of perfect competition, since two of the assumptions of this model are perfect information and equal technology. Both of these assumptions are completely unrealistic in relation to most of the strategies we are considering, but they are perhaps relevant for the traditional strategy which may be followed by firms producing a standard homogeneous commodity under competitive conditions. Such firms can concentrate all their ingenuity on low-cost efficient production and can ignore other scientific and technical activities or treat them as exogenous to the firm. Some products are still produced under conditions which may sometimes approximate to traditional competitive assumptions but they are only at one end of the spectrum. The traditional strategy is essentially non-innovative, or insofar as it is innovative it is restricted to the adoption of process innovations, generated elsewhere but available equally to all firms in the industry. Agriculture, building and catering are examples of industries which in some respects approximate to these assumptions, although all three are now quite strongly affected by information technology and by organizational innovations.

We consider six alternative strategies, but they should be considered as a spectrum of possibilities, not as clearly definable pure forms. Although some firms recognizably follow one or other of these strategies, they may change from one strategy to another, and they may follow different strategies in different sectors of their business.

Table 11.1 Strategies of the firm

Strategy	*Inhouse scientific and technical functions within the firm*									
	Fundamental research	*Applied research*	*Experimental development*	*Design engineering*	*Production engineering quality control*	*Technical services*	*Patents*	*Scientific and technical information*	*Education and training*	*Long-range forecasting and product planning*
Offensive	4	5	5	5	4	5	5	4	5	5
Defensive	2	3	5	5	4	4	4	5	4	4
Imitative	1	2	3	4	5	3	2	5	3	3
Dependent	1	1	2	3	5	2	1	3	3	2
Traditional	1	1	1	1	5	1	1	1	1	1
Opportunist	1	1	1	1	1	2	1	5	1	5

Range 1–5 indicates weak (or non-existent) to very strong.

11.2 OFFENSIVE STRATEGY

An 'offensive' innovation strategy is one designed to achieve technical and market leadership by being ahead of competitors in the introduction of new products.[2] Since a great deal of world science and technology is accessible to other firms, such a strategy must either be based on a special relationship with part of the world science–technology system, or on strong independent R&D, or on very much quicker exploitation of new possibilities, or on some combination of these advantages. The special relationship may involve recruitment of key individuals, consultancy arrangements, contract research, good information systems, personal links, or a mixture of these. But in any case the technical and scientific information and knowledge for an innovation will rarely come from a single source or be available in a finished form. Consequently the firm's R&D department has a key role in an offensive strategy. It must itself generate that scientific and technical information and knowledge which is not available from outside and it must take the proposed innovation to the point at which normal production can be launched. A partial exception to this generalization is the new firm which is formed to exploit an innovation already wholly or largely developed elsewhere, as was the case with many scientific instrument innovations. The new small firm is a special category of offensive innovator. The remarks here apply primarily to already established firms, but we may recall the conclusion of Chapters 8 and 9 that the importance of the new small innovating firm is related to the reluctance and inability of many established firms to adopt an offensive strategy.

The firm pursuing an offensive strategy will normally be highly research intensive, since it will usually depend to a considerable extent on inhouse R&D. In the extreme case it may do nothing but R&D for some years. It will usually attach considerable importance to patent protection since it is aiming to be first or nearly first in the world, and hoping for substantial monopoly profits to cover the heavy R&D costs which it incurs and the failures which are inevitable. It must be prepared to take a very long-term view and high risks. Examples of such an offensive strategy which have been considered in Part One are RCA's development of television and colour television, Du Pont's development of nylon and Corfam, IG Farben's development of PVC, ICI's development of Terylene, Bell's development of semiconductors, Houdry's development of catalytic cracking, and the UK Atomic Energy Authority's development of various nuclear reactors. It took more than ten years from the commencement of research before most of these innovations showed any profit, and some never did so.

The extent to which an offensive strategy requires the pursuit of inhouse fundamental research is a matter partly of debate and partly of definition. From a narrow economic point of view it is fashionable to deride inhouse fundamental research, and to regard it as an expensive toy or a white elephant. Certainly it can be this, and the advice of many economists and management consultants to leave fundamental research to universities has a kernel of good sense, but it is too narrow. Certainly some of the most successful offensive innovations were partly based on inhouse fundamental research. Or at least the firms who were doing it described it as such, and it could legitimately be defined as research without a specific practical end in view (the definition of applied research). However, it was certainly not completely pure research in the academic sense of knowledge pursued without any regard to the possible applications. Perhaps the best description of it is oriented fundamental research or background fundamental research. A strong case can be made for doing this type of research as part of an offensive strategy (or even in some cases as part of a defensive strategy).

The straightforward economic argument against inhouse fundamental research holds that no firm can possibly do more than a small fraction of the fundamental research which is relevant, and that in any case the firm can get access to the results of fundamental research performed elsewhere. This oversimplified 'economy' argument breaks down because of its failure to understand the nature of information processing in research, and the peculiar nature of the interface between science and technology. There is no direct correspondence between changes in science and changes in technology. Their interaction is extremely complex and resembles more a process of mutual scanning of old and new knowledge. The argument that 'anyone can read the published results of fundamental scientific research' is only a half-truth. A number of empirical studies which have been made in the Unites States indicate that access to the results of fundamental research is partly related to the degree of participation (Price and Bass, 1969; Steinmueller, 1994). In trying to answer the question: 'Why do firms do basic research with their own money?', Rosenberg (1990) described basic research investment as a 'ticket of admission' to scientific and knowledge-building networks. Many case studies of innovation show that direct access to original research results was extremely important, although the mode of access varied considerably (Illinois Institute of Technology Research Institute, 1969; Langrish *et al.*, 1972; Wilkins, 1967; Gibbons and Johnston, 1974; Industrial Research Institute Research Corporation, 1979; Hounshell and Smith, 1988). More systematic studies by Mansfield (1991) of 76 major American firms showed that most of them believed that a significant proportion of their new products and processes introduced between 1975 and 1985 could not have been developed without the results of fundamental university research in the fifteen years prior to the innovations.

Inhouse fundamental research was obviously important in some of the cases considered in Part One (e.g. nylon and polyethylene), and its role in relation to Bell's discovery and development of the transistor is discussed in a classic paper by Nelson (1962). It was also important in a significant proportion of the American case studies, for example in GE and Dow. The results of SAPPHO, although not strongly differentiating between success and failure on the basis of fundamental research performance, did suggest a marginal advantage to fundamental research performers (Science Policy Research Unit, 1972). It may sometimes be a matter of hair-splitting as to whether research is defined as background, oriented basic or applied research. It must always be remembered that all schemes of classification are to some extent arbitrary and artificial.

Price and Bass (1969) attempted to measure the relative importance of direct participation as one of the modes of access to original research. They classified 244 coupling events in 27 innovation case studies. A coupling event is one which links developments in basic science with technological

Table 11.2 Frequency of use of coupling method

Category of coupling	*Suits and Bueche*	*Frey and Goldman*	*Tannenbaum (MAB)*
Indirect[a]	8	5	25
Passive availability[b]	28	17	43
Direct participation[c]	38	18	40
'Gatekeeper'[d]	14	2	6
All 'coupling events'	88	42	114

[a] No direct dialogue between originators and users of new scientific knowledge.
[b] Scientists are open to approach but do not initiate a dialogue. Technologists request assistance.
[c] Includes inter-disciplinary teams, exchanges and consultants.
[d] Gifted individuals assigned the specific function of promoting communication between scientists and engineers.

Source: Price and Bass (1969).

advances. The results shown in Table 11.2 indicate that direct participation was involved in 40 per cent of the events, and passive availability of scientists outside firms was also very important. It is not unreasonable to postulate that here too the effectiveness of communication is to some extent a function of the degree of involvement in basic research.

Most of these studies relate to innovations made by firms which would probably be classified as offensive, and tend to confirm the view that inhouse oriented fundamental research combined with monitoring activities and consultancy are important modes of access to new knowledge for firms pursuing such a strategy. Price and Bass conclude that:

> 1. Although the discovery of new knowledge is not the typical *starting point* [our italics] for the innovative process, very frequently interaction with new knowledge or with persons actively engaged in scientific research is essential.
> 2. Innovation typically depends on information for which the requirement cannot be anticipated in definitive terms and therefore cannot be programmed in advance; instead key information is often provided through unrelated research. The process is facilitated by a great deal of freedom and flexibility in communication across organizational, geographical and disciplinary lines.
> 3. The function of basic research in the innovative process can often be described as meaningful dialogue between the scientific and technological communities. The entrepreneurs for the innovative process usually belong to the latter sector, while the persons intimately familiar with the necessary scientific understanding are often part of the former.
>
> (Price and Bass, 1969, p. 804)

These findings are extremely important, because it has often been concluded from individual case studies that technical innovations bear no relation to basic research or the advance of scientific knowledge. The results of the US Department of Defense Project Hindsight (Sherwin and Isenson, 1966) and of the Manchester Queen's Award study (Langrish *et al.*, 1972) were often wrongly construed in this way, because they suggested that most of the new products were based on an 'old' science. Any major

innovation will draw on a stock of knowledge, much of which is old in this sense. But the capacity to innovate successfully depends increasingly on the ability to draw upon this whole corpus of structured knowledge, old and new (Steinmueller, 1994).

The availability of external economies in the form of a highly developed scientific and technological infrastructure is consequently a critical element in innovative efficiency. Although these external economies are to some extent worldwide, and to this extent it makes sense to talk of a world stock or pool of knowledge, access to many parts of it is limited. Cultural, educational, political, national and proprietary commercial barriers prevent everyone from drawing freely on this stock as well as purely geographical factors. The ability to gain access to it is an important aspect of R&D management and bears a definite relationship to research performance and reputation. Pavitt's inter-country comparisons of innovative performance (1971, 1980) also bear out this conclusion and so too does the study by Gibbons and Johnston on the interaction of science and technology (1974).

We may conclude, therefore, both from the results of Price and Bass and other empirical studies and from our own survey, that the performance of fundamental research, while not essential to an offensive innovation strategy, is often a valuable means of access to new and old knowledge generated outside the firm, as well as a source of new ideas within the firm. While ultimately all firms may be able to use new scientific knowledge, the firm with an offensive strategy aims to get there many years sooner. Even if it does not conduct oriented fundamental research itself it will need to be able to communicate with those who do, whether by the performance of applied research, through consultants or through recruitment of young postgraduates or by other means. This has very important implications for training policy as well as for communications with the outside scientific and technological community.

However, although access to basic scientific knowledge may often be important, the most critical technological functions for the firm pursuing an offensive innovation strategy will be those centred on experimental development work. These will include design engineering on the one hand, and applied research on the other. A firm wishing to be ahead of the world in the introduction of a new product or process must have a very strong problem-solving capacity in designing, building and testing prototypes and pilot plants. Its heaviest expenditures are likely to be in these areas, and it will probably seek patent protection not only for its original breakthrough inventions but also for a variety of secondary and follow-up inventions. Since many new products are essentially engineering systems, a wide range of skills may be needed. Pilkington's were successful with the float glass process and IG Farben with PVC, largely because they had the scientific capacity to resolve the problems which cropped up in pilot plant work and could not be resolved by rule of thumb. The same is even more true of nuclear reactor development work.

There has been a great deal of confusion and misunderstanding over expenditure on R&D in relation to the total costs of innovation. It became fashionable to talk of R&D costs as a relatively insignificant part of the total costs of innovation – at most 10 per cent. This view is not supported by any empirical research and is based on a misreading of a US Department of

Commerce report frequently quoted and requoted. The small amount of empirical research which has been done on this question indicates that R&D costs typically account for about 50 per cent of the total costs of launching a new product in the electronics and chemical industries. As in so many aspects of industrial innovation it was Mansfield and his colleagues (1971, 1977) who got down to the hard task of systematic empirical observation and measurement, rather than plucking generalizations from the air. Their results were confirmed on a larger scale by the Canadian surveys of industrial R&D and by German work (OECD, 1982).

This is not to minimize the importance of production planning, tooling, market research, advertising and marketing. All of these functions must be efficiently performed by the innovating firm, but its most important distinguishing feature is likely to be its heavy commitment to applied research and experimental development. As we have seen, this was characteristic of IG Farben, Du Pont, GE, RCA, Bell and other offensive innovators. In the case of the new firm established to launch a new product, the inventor–entrepreneur is the living embodiment of this characteristic.

However, in order to succeed in its offensive strategy the firm will not only need to be good at R&D, it will also need to be able to educate both its customers and its own personnel. At a later stage these functions may be socialized as the new technology becomes generally established, but in the early stages (which may last for some decades) the innovating firm may have to bear the brunt of this educational and training effort. This may involve running courses, writing manuals and textbooks, producing films, providing technical assistance and advisory services and developing new instruments. Typical examples of this aspect of innovation are the Marconi school for wireless operators, the BASF agricultural advisory stations, the ICI technical services for polyethylene and other plastics, the IBM and ICL computer training and advisory services, UKAEA's work on isotopes, and technical education of the consortia and the CEGB. As we have seen, many observers (e.g. Brock, 1975) believed that the efficient provision of these services was the decisive advantage of IBM in the world computer market at the time of its greatest dominance.

The offensive innovator will need good scientists, technologists and technicians for all these functions as well as for production and marketing of the new product. This means that such firms are likely to be highly education intensive in the sense of having an above average ratio of scientifically trained people in relation to their total employment. The generation and processing of information occupy a high proportion of the labour force, but whereas for the traditional firm this would represent a top heavy and wasteful deployment of resources, these activities are the lifeblood of the offensive innovating firm. It is the conversion of information into new knowledge of products and processes which is its most important feature.

11.3 DEFENSIVE INNOVATION STRATEGY

Only a small minority of firms in any country are willing to follow an offensive innovation strategy, and even these are seldom able to do so consistently over a long period. Their very success with original innovations may lead them into a position where they are essentially resting on their laurels and consolidating an established position. They will in any case often have products at various stages of the product cycle – some completely new, others just established and still others nearing obsolescence. The vast majority of firms, including some of those who have once been offensive innovators, will follow a different strategy: defensive, imitative, dependent, traditional, or opportunist. It must be emphasized again that these categories are not pure forms but shade into one another. The differences assume particular importance in relation to industry in the developing countries, but they are also important in Europe and the United States.

A defensive strategy does not imply absence of R&D. On the contrary a defensive policy may be just as research intensive as an offensive policy. (The example of Sloan's approach to R&D at GM described in Chapter 6 is a good illustration.) The difference lies in the nature and timing of innovations. The defensive innovators do not wish to be the first in the world, but neither do they wish to be left behind by the tide of technical change. They may not wish to incur the heavy risks of being the first to innovate and may imagine that they can profit from the mistakes of early innovators and from their opening up of the market. Alternatively, the defensive innovator may lack the capacity for the more original types of innovation, and in particular the links with fundamental research. Or they may have particular strength and skills in production engineering and in marketing. Most probably the reasons for a defensive strategy will be a mixture of these and similar factors. A defensive strategy may sometimes be involuntary in the sense that a would-be offensive innovator may be outpaced by a more successful offensive competitor.

Several surveys (Nelson *et al.*, 1967; Schott, 1975, 1976; Sirilli, 1982) have shown that in all the leading countries, most industrial R&D is defensive or imitative in character and concerned mainly with minor improvements, modifications of existing products and processes, technical services and other work with short time horizons. Defensive R&D is probably typical of most oligopolistic markets and is closely linked to product differentiation. For the oligopolist, defensive R&D is a form of insurance enabling the firm to react and adapt to the technical changes introduced by competitors. Since defensive innovators do not wish to be left too far behind, they must be capable of moving rapidly once they decide that the time is ripe. If they wish to obtain or retain a significant share of the market they must design models at least as good as the early innovators and preferably incorporating some technical advances which differentiate their products, but at a lower cost. Consequently, experimental development and design are just as important for the defensive innovator as for the offensive innovator. Computer firms which continued to market valve designs long after the introduction of semiconductors could not survive. Chemical contractors which attempted to market a process which was technically obsolescent could not survive either. The defensive innovator must be capable at least of catching up with the game, if not of leap-frogging. This means that in industries such as semiconductors and software, all innovators must be extremely agile since the life of each new generation of components and products is so short (Hobday, 1994).

In an interesting study of the computer market, Hoffmann (1976) maintains that IBM has mainly followed a defensive innovation strategy, although with some offensive elements, while Sperry Rand (Univac) pursued a more consistently offensive strategy and Honeywell an imitative strategy. Since IBM spent far more on R&D than Sperry Rand in absolute terms, this illustrates the point that the defensive innovator may well commit greater scientific and technical resources than the offensive innovator. A certain amount of slack may be necessary in order to cover many new possibilities and to retain the flexibility needed to move very fast in catching up with the technical advances first introduced by competitors. However, even with heavy R&D spending a defensive strategy may lead to a firm being outflanked by more agile competitors. The classic case of an R&D-intensive firm being outmanoeuvred in this way was IBM's late development of the personal computer. In the end, they had to do this using external sources and an R&D team which stood outside their main R&D facilities (Chapter 7).

Patents may be extremely important for the defensive innovator but they assume a slightly different role. Whereas for the pioneer patents are often a critical method of protecting a technical lead and retaining a monopolistic position, for the defensive innovator they are a bargaining counter to weaken this monopoly. The defensive innovators will typically regard patents as a nuisance, but will claim that they have to get them to avoid being excluded from a new branch of technology. The offensive innovators will often regard them as a major source of licensing revenue, as well as protection for the price level needed to recoup R&D costs. They may fight major legal battles to establish and protect their patent position (RCA with television, ICI with polyethylene, La Roche with tranquillizers, EMI with their scanner, Telefunken with PAL), and typically their receipts from licensing and know-how deals will far exceed expenditure. (In 1971, ICI had receipts of £13 million and expenditure of £3 million.)

The defensive innovators will probably find it necessary to devote resources to the education and training of their customers as well as their own staff. They will also usually have to provide technical assistance and advice and these functions may be just as important for the defensive as for the offensive innovators. On the other hand, advertising and selling organizations, the traditional weapons of the oligopolist, will probably be more important, and to some extent technical services to customers will be bound up with this. The oligopolist may well attempt to use a combination of product differentiation and technical services to secure a market share not attainable by sheer originality (Brock, 1975; Hoffmann, 1976).

Both the offensive and the defensive innovator will be deeply concerned with long-range planning, whether or not they formalize this function within the firm. In many cases this may still often be the vision of the entrepreneur and his immediate associates, but increasingly this function, too, is becoming professionalized and specialized, so that product planning is a typical department for both offensive and defensive innovators. However, the more speculative type of 'technological forecasting' is more characteristic of the offensive innovator, and as we have seen in Chapter 10, still has considerable affinities to astrology or fortune-telling. It should probably still be regarded as a kind of sophisticated war dance to mobilize a faction in support of a particular project or strategy, but increasingly serious techniques have been developed (Bright, 1968; Beattie and Reader, 1971; Encel *et al.*, 1975; and Jones, 1981).

The defensive innovator, then, like the offensive innovator, will be a knowledge intensive firm, employing a high proportion of scientific and technical personnel. Scientific and technical information services will be particularly important, and so will speed in decision-making, since survival and growth will depend to a considerable extent on timing. The defensive innovators can wait until they see how the market is going to develop and what mistakes the pioneers make, but they dare not wait too long or they may miss the boat altogether, or slip into a position of complete dependence in which they have lost their freedom of manoeuvre. R&D will be geared to speed and efficiency in development and design work, once management decides to take the plunge. Such firms will sometimes describe their R&D as advanced development rather than research.

Most commonly, the large multi-product chemical or electrical firm will contain elements of both offensive and defensive strategies in its various product lines, but a defensive strategy is more characteristic of firms in the smaller industrialized countries, which cannot risk an offensive strategy or lack the scientific environment and the market.

The strategy which a firm is able or willing to pursue is strongly influenced by its national environment and government policy. Thus, for example, European firms since the war have generally been unable or unwilling to attempt offensive innovations in the semiconductor industry and their role has been almost entirely defensive (Chapter 7 and Hobday, 1991). French chemical firms have often followed a defensive strategy while German chemical firms have often been offensive. The complex interplay of national environment and firm strategy cannot be dealt with in detail here. But it is important to make the simple but fundamental point that many firms in the offensive group are United States firms, while most firms in the developing countries are imitative, dependent or traditional, with Europe in an intermediate position. In the eighteenth and early nineteenth centuries many British firms were following offensive innovation strategies, although without formal R&D departments. An over-simplified interpretation of Japanese experience since 1990 would be in terms of the movement of an increasing proportion of firms from traditional to imitative strategies, and then to defensive and offensive innovations. Japanese national policy has been designed to facilitate this progression. The extent of this shift is clearly observable in the statistics of the Japanese technological balance of payments since the war. In the early post-war period Japanese firms were spending far more on buying foreign licences and know-how than they were receiving from the sale of their own technology. At this time it was customary to regard the Japanese as 'superb imitators' and the long-term elements in their strategy were often overlooked. During the 1970s and 1980s on the new contracts which they signed, Japanese firms received more from the sale of their own technology than they paid out.

Hobday (1995) has shown in his research on the innovative strategies of East Asian firms in the 'Tigers' that there has been similar progression in their strategies from dependence, imitation and joint projects with foreign firms to increasingly independent innovations. National policies have tried to facilitate this (see Chapter 12). A technology policy of this sort involves a gradual change in the mix of STS in the direction of a more R&D-intensive mix. The type of R&D also changes from adaptive to increased originality, but it may require a long period in which most enterprises follow a dependent or imitative strategy, while slowly strengthening their technical resources, on the basis of a carefully conceived long-term national policy, involving protection of 'infant technology' as well as the build-up of a wide range of government-supported STS. The main elements of this long-term national strategy have been well described by Allen (1981) and are analysed further in Chapter 12. The precise balance of STS must vary with the size, resource endowment and historical background of each country. But in many developing countries STINFO (Science and Technical Information Services), survey organizations, standard institutes, technical assistance organizations and design-engineering consultancy organizations capable of impartial scrutiny and feasibility studies for projects involving imported technology are all of critical importance. They can provide the essential science and technology infrastructure which enables the STS at enterprise level to function effectively, despite the inevitable limitations in trained scientific and technical personnel. Only a few enterprises will gradually be able to develop first an adaptive and later an original innovative capacity. However, even in the United States the vast majority of firms are traditional, dependent or imitative in their strategies. We now turn to a consideration of these alternatives.

11.4 IMITATIVE AND DEPENDENT STRATEGIES

The defensive innovators do not normally aim to produce a carbon-copy imitation of the products introduced by early innovators. On the contrary, they hope to take advantage of early mistakes to improve upon the design, and they must have the technical strength to do so. At least they would like to differentiate their products by minor technical improvements. They will try to compete by establishing an independent patent position rather than simply by taking a licence, but if they do take a licence it will usually be with the aim of using it as a springboard to do better. However, their expenditure on acquisition of know-how and licences from other (offensive and defensive) firms may often exceed their income from licensing. For the imitative firm it will always do so.

The imitative firm does not aspire to 'leap-frogging' or even to keeping up with the game. It is content to follow some way behind the leaders in established technologies, often a long way behind. The extent of the lag will vary, depending upon the particular circumstances of the industry, the country and the firm. If the lag is long then it may be unnecessary to take a licence, but it still may be useful to buy know-how. If the lag is short, formal and deliberate licensing and know-how acquisition will often be necessary. The imitative firm may take out a few secondary patents but these will be a byproduct of its activity rather than a central part of its strategy. Similarly, the imitative firm may devote some resources to technical services and training but these will be far less important than for the innovating firms, as the imitators will rely on the pioneering work of others or on the socialization of these activities, through the national

education system. An exception to this generalization might be in a completely new area (e.g. in a developing country) when neither imports nor the subsidiary of an innovating firm have opened up the market. The enterprising imitator may aspire to become a defensive innovator, especially in rapidly growing economies. This will mean an upgrading of STS and the strengthening or commencement of R&D activities, leading often to joint ventures or collaborative agreements with foreign or domestic firms. Examples of this are discussed in Part Three.

The imitator must enjoy certain advantages to enter the market in competition with the established innovating firms. These may vary from a captive market to decisive cost advantages. The captive market may be within the firm itself or its satellites. For example, a large user of synthetic rubber, such as a tyre company, may decide to go into production on its own account. Or it may be in a geographical area where the firm enjoys special advantages, varying from a politically privileged position to tariff protection. (This was the typical situation in many developing countries in the period of import substitution and still today in many cases). Alternatively or additionally, the imitator may enjoy advantages in lower labour costs, plant investment costs, energy supplies or material costs. The former are more important in electrical equipment, the latter in the chemical industry. Lower material costs may be the result of a natural advantage or of other activities (e.g. oil refineries in the plastics industry). Finally, imitators may enjoy advantages in managerial efficiency and in much lower overhead costs, arising from the fact that they do not need to spend heavily on R&D, patents, training, and technical services, which loom so large for the innovating firm. The extent to which imitators are able to erode the position of the early innovators through these advantages will depend upon the continuing pace of technological change. The early innovators will try to maintain a sufficient flow of improvements and new generations of equipment, so as to lose the imitators. But if the technology settles down, and the industry becomes mature, they are vulnerable and may have to innovate elsewhere. Du Pont's decision to move right out of the rayon industry despite their technical strength is a good example of strategic planning of this kind. Several other more recent cases in the chemical industry are discussed in Quintella (1993) in his book on strategic management. In some industries and technologies (but by no means all) the growth of an industry may be represented as following a cyclical pattern – a product cycle from 'birth' to 'maturity'. Sometimes the pace of technical change and the rapid succession of new generations of products may delay 'maturity', while in others apparently 'mature' industries may be rejuvenated. However, in those cases where it is a useful approximation to the growth pattern Hirsch (1965) has summarized the characteristics of the product cycle which may permit imitators to compete (Table 11.3 and Fig. 11.1). The extent to which they are actually able to do so, particularly in developing countries, is strongly influenced by institutional factors and government policies (see Chapters 12 and 15).

Unless the imitators enjoy significant market protection or privilege they must rely on lower unit costs of production to make headway. This will usually mean that in addition to lower overheads, they will also strive to be more efficient in the basic production process. They may attempt this by

Table 11.3 Characteristics of the product cycle

Characteristics	*Cycle phase*		
	Early	*Growth*	*Mature*
Technology	Short runs Rapidly changing techniques Dependence on external economies	Mass production methods gradually introduced Variations in techniques still frequent	Long runs and stable technology Few innovations of importance
Capital intensity	Low	High, due to high obsolescence rate	High, due to large quantity of specialized equipment
Industry structure	Entry is know-how determined Numerous firms providing specialized services	Growing number of firms Many casualties and mergers Growing vertical integration	Financial resources critical for entry Number of firms declining
Critical human inputs	Scientific and engineering	Management	Unskilled and semi-skilled labour
Demand structure	Sellers' market Performance and price of substitutes determine buyers' expectations	Individual producers face growing price elasticity Intra-industry competition reduces prices Product information spreading	Buyers' market Information easily available

Source: Hirsch (1965).

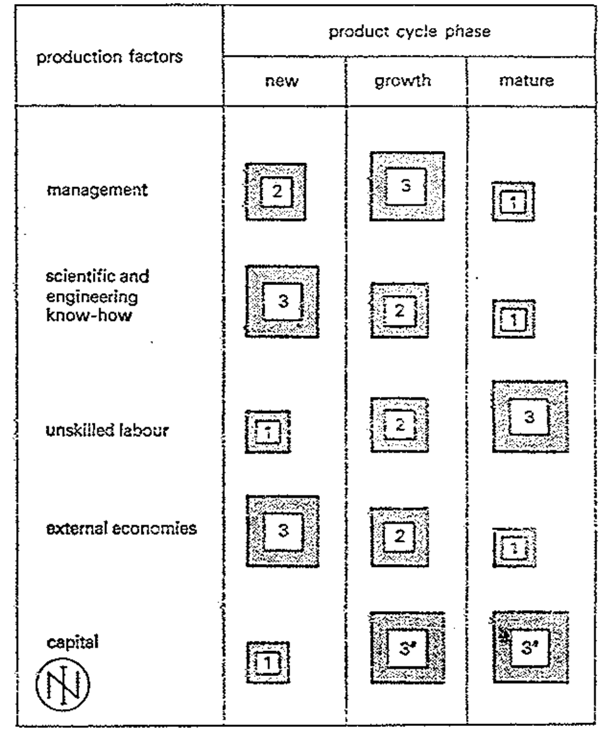

Fig. 11.1 The relative importance of various factors in different phases of the product cycle.

Note: The purpose of the blocks is simply to rank the importance of the different factors, at different stages of the product cycle. The relative areas of the rectangles are not intended to imply anything more precise than this.
[a] Considered to be of equal importance.

Source: Hirsch (1965).

process improvements, but both static and dynamic economies of scale will usually be operating to their competitive disadvantage, so that good adaptive R&D must be closely linked to manufacturing. Consequently, production engineering and design are two technical functions in which the imitators must be strong. Even if they are making carbon copies under licence, the imitators cannot afford to have high production costs unless they have high tariff protection. They will also wish to be well-informed about changes in production techniques and in the market, so that scientific and technical information services are another function which is essential for the imitator firm. The information function is also important for the selection of products to imitate and of firms from which to acquire know-how. It is clear that in all of this the would-be imitator in the typical developing country may be severely handicapped by local circumstances, unless national policies are carefully designed to facilitate technical progress.

If national policies and firm strategies are strongly oriented towards catch-up, for example, with respect to training, education, finance for investment and the import of technology, then it may be possible for latecomers to turn their lateness into a competitive advantage. Gerschenkron (1962) developed this theory of latecomer advantages mainly with respect to scale of plant in the steel industry in the nineteenth century. He pointed to the importance of financial institutions which could bear the costs of heavy investment and argued that given this 'social capability' latecomer firms could enjoy the advantages of the existence of an established world market and the availability of skills and technologies which could be imported quickly and more cheaply than those which were available to the early innovators. Jang-Sup Shin (1996) has extended Gerschenkron's analysis with the example of the South Korean steel industry and the South Korean semiconductor industry.

The examples cited in Chapter 2 on the British cotton industry show, however, that the British offensive innovators in the late eighteenth and early nineteenth century were able to reinforce their competitive advantages by economies of agglomeration and networking advantages, as well as by organizational and marketing innovations. In the end, they were overtaken by catch-up firms in latecomer countries, but it was a very extended process. The balance is a delicate and complicated one and the extent to which latecomer firms and countries can overcome their disadvantages by determined and intelligent catch-up strategies is one of the key questions discussed in Part Three, especially in the chapters on national systems of innovation (Chapter 12) and development (Chapter 15).

A dependent strategy involves the acceptance of an essentially satellite or subordinate role in relation to other stronger firms. The dependent firm does not attempt to initiate or even imitate technical changes in its product, except as a result of specific requests from its customers or its parent. It will usually rely on its customers to supply the technical specification for the new product, and technical advice in introducing it. Most large firms in industrialized countries have a number of such satellite firms around them supplying components, or doing contract fabrication and machining, or supplying a variety of services. The dependent firm is often a subcontractor or even a sub-subcontractor. Typically, it has lost all initiative in product design and has no R&D facilities. The small firms in capital intensive industries are often in this category and hence account for rather few innovations (see Chapter 9). For the special role of such firms in the Japanese economy, see Clark (1979); Sako, (1992); Womack *et al.*, (1990).

The dependent small subcontract firm may, however, also seek to upgrade its technology and in some instances its major customers may help it to do so. In Chapter 9, we have seen that the most dynamic small firms are the so-called NTBFs (new technology based firms). These will often be offensive innovators in very specialized niche markets. However, small subcontract firms may also move from a dependent status to the category of innovative firms by the upgrading of their specialized knowledge in a narrow field. They may also lessen their dependence by enlarging their customer network once they strengthen their own innovative competence.

As we saw in Chapters 6 and 7, the Japanese automobile and electronic industries are often cited as examples of changing subcontractor relationships. The large assembly firms may extend technical assistance to their 'first-tier' suppliers, lend them engineers, and collaborate with them in upgrading the specifications of the components and materials which they supply. The same strategy is being increasingly imitated in Europe and North America and indeed some large firms in both retailing and manufacture already pursued it decades ago. Marks and Spencer is a good example of this. The case of Chrysler is another interesting example. For a long time this firm was distinctly the weakest of the three top US automobile firms and was actually saved from bankruptcy by the federal government. It deliberately attempted to emulate the Japanese strategy of working closely with suppliers in order to emerge from its difficulties. From 1989 to 1996 it reduced the number of suppliers from 2,500 to 1,140 but established a new relationship of collaboration in design and manufacturing with the remaining companies. Dyer (1996), who studied the supplier companies, described this transformation as the creation of an 'American *Keiretsu*' and showed that Chrysler's profit per vehicle leapt from an average of $250 in the 1980s to a record for all US automobile firms of $2,110 per vehicle in 1994. From being the least profitable, Chrysler became the most profitable of the US auto firms in the 1990s and the supplier firms were able to make numerous suggestions for improvements, many of which were implemented. Although the main benefits in this case appear to have gone to Chrysler, there were clearly also gains for suppliers and a changed form of dependence based more on mutual trust and co-operation.

The pure dependent firm is in effect a department or shop of a larger firm, and very often such firms are actually taken over. But it may suit the larger firm to maintain the client relationship, as subcontractors are a useful cushion to mitigate fluctuations in the work load of the main firm. In the 1980s and 1990s, there has been a fairly strong worldwide trend towards outsourcing by large firms of activities which were once performed inhouse. The dependent firm may also wish to retain its formal independence as the owners may hope they will ultimately be able to change their status by diversification or by enlarging their market. They may in any case prize even that limited degree of autonomy which they still enjoy as a satellite firm. In spite of their apparently weak bargaining position, they may enjoy good profits for considerable periods, because of low overheads, entrepreneurial skill, specialized craft knowledge or other peculiar local advantages. Even if they are 'squeezed' pretty hard by their customers, they may prefer to endure long periods of low profitability rather than be taken over completely. Although bankruptcies and takeovers may be common, there is also a stream of new entries.

11.5 TRADITIONAL AND OPPORTUNIST STRATEGIES

The 'dependent' firm differs from the 'traditional' in the nature of its product. The product supplied by the 'traditional' firm changes little, if at all. The product supplied by the 'dependent' firm may change quite a lot, but in response to an initiative and a specification from outside. The traditional firms sees no reason to change its product because the market does not demand a change, and the competition does not compel it to do so. Both lack the scientific and technical capacity to initiate product changes of a far-reaching character, but the traditional firm may be able to cope with design changes which are essentially fashion rather than technique. Sometimes indeed, this is its greatest strength.

Traditional firms may operate under severely competitive conditions approximating to the perfect competition model of economists, or they may operate under conditions of fragmented local monopoly based on poor communications, lack of a developed market economy, and pre-capitalist social systems. Their technology is often based on craft skills and their scientific inputs are minimal or non-existent. Demand for the products of such firms may often be very strong, to some extent just because of their traditional craft skills (handicrafts, restaurants and decorators). Such firms may have good survival power even in highly industrialized capitalist economies. But in many branches of industry they have proved vulnerable to exogenous technical change. Incapable of initiating technical innovation in their product line, or of defensive response to the technical changes introduced by others, they have been gradually driven out. These are the 'peasants' of industry.

An industrialized capitalist society includes some industries which are predominantly traditional, and others characterized by rapid technical innovation. It has been argued that an important feature of the twentieth century has been the growth of the research intensive sector. But it is a matter of conjecture and of policy as to how far this change may continue. It is a complex process, since sometimes the very success of a technical innovation may lead to standardized mass production of a new commodity with little further technical change or research for a long time. Usually, however, the industries generated by R&D have continued to perform it, so that the balance has gradually shifted towards a more research intensive economy, and a higher rate of technical change. It is the contention of this book that this has been one of the most important changes in twentieth-century industry, but it must be seen over a long time perspective.

This change is now extending to service industries and may prove to be an even more important structural change in the economies of the twenty-first century, both in the presently industrialized countries and in the developing countries. Mainly as a result of the pervasiveness of ICT, some service industries, such as the financial services, entertainment and information services are becoming both more capital intensive (through their heavy investment in computers and communication equipment) and more research intensive (through their employment of software and electronic engineers). The telecommunication industry which was already fairly R&D-intensive and capital intensive is now even more so and is rapidly extending its linkages to the world of entertainment and multimedia services. Moreover, the old boundaries between manufacturing and services, which were already being bridged in the 1950s and 1960s are now often completely eroded.

It was always hard to classify a firm like IBM, either to services or to manufacturing since it was extremely strong in both. As the share of software and consultancy in the total output of the computer industry has been rapidly increasing, IBM (and similar computer firms) has become more and more like a service firm, even though manufacturing remains a

vitally important activity. The same tendency can be seen in the case of firms manufacturing telecommunication equipment: Ericsson, one of the most successful European firms in this industry, now employs fewer than 10 per cent of its employees in manufacturing. Most of its personnel are engaged in software, design of systems, R&D, worldwide consultancy and marketing, technical services, management and networking. It subcontracts a great deal of manufacturing. Smart (1996) has described how even a classical manufacturing firm like General Electric in the United States is moving into service areas as a matter of deliberate strategy. Manufacturing accounted for 56 per cent of total revenue in 1990 but this had declined to 44 per cent by 1995 and was projected to decline still further to 33 per cent by the year 2000. Financial services, medical services, consultancy and 'after-market services' were all expanding rapidly.

An even more extreme example is that of Benetton, which most people would think of as a firm belonging to the clothing industry. But, as Belussi (1993) and others have shown, Benetton actually has hardly any employees in manufacturing, except in experimental operations designed to keep abreast of new technological developments. Almost all manufacturing is subcontracted to a network of small firms in North-Eastern Italy, whereas Benetton itself concentrates on design and worldwide marketing through hundreds of franchised retail outlets all over the world. ICT is extremely important for Benetton since the daily sales data from these retail outlets are processed and co-ordinated by a computerized warehouse near Venice and form the basis for the manufacturing orders to the subcontractors.

All these examples are illustrative of the fundamental changes affecting the economy through the worldwide diffusion of ICT. Firms everywhere are being obliged to rethink their strategies as a result. Of course, this rethinking may not take the form of deliberate sophisticated new management strategies. Only in a minority of rather large firms will sophisticated management tools such as technological forecasting be deployed. The latest management fashions, such as re-engineering or technological audits, lean production or bench-marking will be more common. But in the vast majority, the rethink will take the form of chief executives, entrepreneurs and other managers responding to ideas or pressures from competitors, from the media, from suppliers and other external sources of information and knowledge, and adapting their own hunches or visions of the future accordingly. The shift in thinking related to the diffusion of the ICT techno-economic paradigm is everywhere apparent (see Chapter 17).

This shift has been less the result of any conscious central government strategy (although government policies have increasingly tended to favour this change) than the outcome of a long series of adaptive responses by firms to external pressures at home and abroad, and of attempts to realize the dreams of inventors. The efforts of firms to survive, to make profits and to grow have led them to adopt one or more of the strategies which have been discussed. But the variety of possible responses to changing circumstances is very great, and to allow for this element of variety one other category should be included, which may be described as an opportunist or niche strategy. There is always the possibility that entrepreneurs will identify some new opportunity in the rapidly changing market, which may not require any inhouse R&D, or complex design, but will enable them to prosper by finding an important niche, and providing a product or service which consumers need, but nobody else has thought to provide. Imaginative entrepreneurship is still such a scarce resource that it will constantly find new opportunities, which may bear little relation to R&D, even in research intensive industries.

Those firms which adopt a strategy of offensive or defensive innovation have gradually learned how to innovate. But there is no recipe which can ensure success and intense controversy still surrounds the important ingredients. The fact that they are often innovating on a world market increases the uncertainty which they confront, and has often led to the involvement of government to subsidize R&D, to create appropriate infrastructures and to diminish market uncertainty. Economic policy inevitably becomes enmeshed with policy for science and technology. These problems are particularly acute for the developing countries and are discussed further in Part Three.

11.6 CONCLUSIONS

In Part One it was argued from historical evidence that the professionalization of the R&D process was one of the most important social changes in twentieth-century industry. In Part Two it has been argued that the requirements of successful innovation and the emergence of an R&D establishment within industry have profoundly modified patterns of firm behaviour. This means that it is no longer satisfactory (if it ever was) to explain behaviour exclusively in terms of response to price signals in an external environment, and adjustment towards an equilibrium situation. World technology is just as much a part of the firm's environment as the world market, and the firm's adaptive responses to changes in technology cannot be reduced to predictable reactions to price changes. This makes things difficult for economists. It means that they must pay much more attention to engineers and to sociology, psychology and political science. Economists have an elegant theory which is confronted with a very untidy and messy reality. Their theory was and is an important contribution to the explanation and prediction of many aspects of firm behaviour, but it is not self-sufficient and attempts to make it so can only lead to sterility.

The chapters in Part Two offer little support to those theories of the firm which have postulated either perfect knowledge or optimizing behaviour with respect to the future. A more sophisticated modern defence of these theories is the 'as if' version. It is conceded that firms are incapable of making the type of calculations about the future, which are assumed in the neoclassical story, but it is argued that the outcome is nevertheless the same because competition ensures the survival and growth of those firms who have behaved 'as if' they could. However, as Hodgson (1992) and Winter (1986) have convincingly argued, this story is barely more credible than the original version. Neither biological evolution, nor the evolution of firms and industries, leads to optimality.

Far more plausible, in the light of the evidence in Parts One and Two, are those theories of the firm, such as those of Nelson and Winter (1974, 1982) and of Dosi *et al.* (1988) which take full account of bounded rationality, imperfect information, market and technical uncertainty. Moreover, any

satisfactory theory of the firm must also take account of the variety of behaviour in different industrial sectors and over different historical periods.

The late Edith Penrose (1959) pointed economics in this new direction with her 'resource-based' theory of the firm, with various combinations of 'competences' and accumulated skills and knowledge. Most recent theorizing has followed Teece (1986) in his development of these concepts in relation to various functions within the firm, i.e. competence in R&D, manufacturing and marketing. At one extreme this can lead to the notion of the 'hollow corporation' which subcontracts all manufacturing. However, the examples which have been discussed above do not point unequivocally in this direction. Even Benetton recognized the importance of retaining some minimal competence in manufacturing, if only to check the work of subcontractors and to avoid being overtaken by sudden new developments in technology.

An interesting and original alternative to this type of thinking is that advanced by Christensen (1995). He proposes a conceptual distinction between four generic categories of assets for innovation: (1) scientific research assets; (2) process innovative assets; (3) product innovative application assets; (4) aesthetic design assets. The last of these is too often forgotten in theorizing about competence but is of the greatest importance in many industries and services. Whereas innovation may sometimes depend on only one or two of these four assets, more commonly a constellation of assets has to be mobilized. However, these assets may be located in a variety of organizational settings, which are moreover often regrouping. Christensen's theory indicates many exciting avenues of research both for economists and for sociologists and organization theorists.

The discussion in this chapter is not intended as an alternative theory of firm behaviour. Such a theory requires a greater integrative effort in the social sciences. But it is intended to indicate the kind of issues which must be embraced by any theory which seeks to explain the firm's innovative and adaptive responses to technological change, as well as to price changes in its factor inputs and the market for its products. There are encouraging indications that social scientists from several disciplines, including economists, are beginning to tackle the development of a more comprehensive and satisfactory theory of the firm (MacKenzie, 1990; Stirling, 1994).

NOTES

1. Metcalfe's study (1970) on Lancashire cotton firms showed that a large number were not willing to purchase a simple new piece of equipment (a size box), even though it cost less than £100, and the payback period was clearly demonstrated by the Research Association and the manufacturers to be less than one year. Mansfield *et al.*'s study (1972) of the adoption process of numerically controlled machine tools in the American tool-and-die industry similarly showed that many firms did not intend to adopt, 'even when firm owners granted that the lack of numerical control would soon be a major competitive disadvantage'. Mansfield estimated the median payback period in this case as five years and suggests that in many firms in this category the owners were close to retirement.
2. The new product may, of course, be a process for other firms.

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