2 Invention, Science, R&D and Concepts of Use and Market

The examples in this chapter have primarily been chosen for the light they shed on the relationship between technology development and use. Through the examples of the laser and penicillin this chapter examines one type of inventive step, the type that generates great technology development potential. It continues with a discussion of how the creative inventive step necessarily involved in innovation relates to the established uses that make up existing markets. The second half revisits two classic accounts of radical technology development through the research and development department, again with emphasis on the role that concepts of prospective use play in technology development. In short, the chapter introduces some of the many ways technologies are made to vary in relation to their prospective uses.

Invention as the Creative Contribution to Innovation

The Cognitive Contribution in Invention

The inventive step is interesting in its own right and receives a great deal of scholarly attention when it generates spectacular new technologies. This special scholarly attention and the popularisation of spectacular examples of invention probably contribute to a widespread confusion that radical inventive steps are the most important part of the innovation process. The numerous acts of insight that are minor or that prove to lay a false innovation trail are quite reasonably less likely to attract academic or popular interest. Their relative underreporting is probably the reason for a further tendency to find the contribution of mental creativity, the 'eureka moment', or what Usher called the 'act of insight' (Usher 1972), the most glamorous aspect of the radical inventive step. However, invention, like innovation, will prove to be as much a social as a cognitive phenomenon.

The study of the radical inventive steps involved in the development of the laser and penicillin give us the chance to study not only the originating moment of highly novel technologies, but also the social context for the creative process of invention. It should be added at this point that it is within the discipline of psychology that we find the study of creativity is treated as a topic in its own right. However, a recent review of the contribution of psychology to the understanding of innovation, written for a management audience, argues that the difficulty of defining the degree of creative contribution in this tradition has led to widespread acceptance on the part of psychologists that the most productive definitions focus on the attributes of creative products (Ford 1996: 1114). In our terms, psychologists have begun to study creativity as cognitive change related to the artefact, in other words the inventive act as we defined it. Of course good scientific histories of invention provide an accessible means of studying both the social and cognitive aspects of invention.

The study of invention especially highlights the difference between the prospective or *intended use* that inspires and then guides the initial design of an artefact and the *actual use* people make of it. The two may be quite different, and this distinction in terms will prove particularly valuable for understanding the genesis of a science-based technology such as the laser.

Intended Use and the Invention of the Laser[1]

The laser immediately presents the problem of which of two inventive steps was the most significant. It might appear obvious that the greatest 'inventive significance' belongs to the first working device to exploit 'amplification through stimulated emission of radiation' (what we can call the 'laser effect', represented by the acronym ASER). However, the laser effect was first demonstrated using microwaves in 1954 in the device termed 'the maser' (Bromberg 1991: 66). In 1958 the effect was shown to be extendable in principle to optical wavelengths (Bromberg 1991: 73) and the first operating laser was demonstrated in 1960 (Bromberg 1991: 10). Although the physicist Richard Feynman commented on the laser in his *Lectures on Physics*, that it is 'just a maser working at optical frequencies' (Feynman et al. 1965, section 9-3), by Bromberg's and by Townes' accounts the extension of the effect to optical wavelengths was not entirely straightforward and the inventive effort behind the laser must be considered to have been distributed in time over several steps. Notwithstanding this caveat we shall focus on the first working device to demonstrate amplification through stimulated emission, a device that exploited the effect at microwave frequencies, hence the name 'maser'.

The inventor of the maser, Charles Townes, had trained as a physicist and was a pioneering developer of the scientific field of microwave spectroscopy (Bromberg 1991: 16). The Second World War diverted him from a pure physics career into the design of radar-guided bombing systems for Bell Laboratories. He therefore combined a professional physicist's interest in advancing spectroscopic understanding (science) with an understanding of the engineerin problems and approaching limits of the current technology for generating and receiving radar radiation. This unusual combination of physics and electrical engineering expertise would prove crucial to his invention of the maser.

Townes' 'intended use' for the maser is evident in the priority he gives to his basic science goals:

> I had myself been stubbornly pursuing shorter and shorter wavelengths. Because they interacted more strongly with atoms and molecules, I was confident they would lead us to even more rewarding spectroscopy. (Townes 1999: 54)

Some members of the US military were also interested in shorter wavelengths, but not for reasons of scientific advance. They understood that equipment which generated shorter wavelengths should provide, for example, lighter, more compact military equipment and shorter-range radar of greater information content (Bromberg 1991: 14). Townes' known interest in working to shorter wavelengths (albeit for reasons of scientific advance) led the Office of Naval Research in 1950 to ask Townes to form an advisory committee on millimetre wave generation with Townes as its chair 'to evaluate and stimulate work in the field of millimetre waves' (Townes 1999: 53).

This committee could not solve the problem of how to generate millimetre waves (Townes 1999: 55). The methods in existence used resonating cavities with dimensions similar to the wavelengths of the radiation they generated and at dimensions of a millimetre it was difficult to manufacture such cavities with useful power output and effective heat dissipation; below a millimetre it became effectively impossible.

> It was out of a sense of frustration over our lack of any substantial progress that the conceptual breakthrough came. (Townes 1999: 55)

The moment of insight came as Townes pondered the generation problem just before an all-day meeting of this committee in 1951. But to obtain some understanding of the 'focusing role' of this objective of millimetre-wavelength generation, some analysis of the scientific–technical content of Townes' inventive step is necessary.

Townes knew, as did other physicists, that molecules naturally resonate at the desired millimetre wavelengths. The focusing role of the intended use on this physical 'effect' is evident when Townes systematically set about thinking how molecular resonance might be exploited to generate millimetre-wavelength radiation (Townes 1999: 56). This systematic review enabled him to see significance in 'stimulated emission', a quantum physical effect with which physicists had also been long familiar, but that in *normal* physical circumstances could not be expected to be useful.[2] Townes' conceptual breakthrough (Townes 1999: 54) came when he realised that in *abnormal* physical circumstances,[3] stimulated emission *could* generate an amplified beam of radiation at the human scale.[4] Townes' engineering-derived knowledge of the design of cavities allowed him quickly to work out that a feasible device could be built based on a resonating cavity, into which would be pumped a source of excited molecules.[5]

In Townes' own words, his device 'employed only standard, known physics' (Townes 1999: 59) and this raises the question of why no other physicist invented the maser before him. Townes includes in his account a review of the relevant work of physicists that preceded his own and comments that 'ideas about stimulated emission were thus floating around, but they were not being pursued … no one had any idea that it could be useful' (Townes 1999: 62). Townes himself had apparently thought of demonstrating stimulated emission through amplification before (in 1948), but 'decided it was rather difficult to do and, because there was no reason to doubt its existence, I felt that nothing new would be proven by such a test' (Townes 1999: 57). In other words, at this time, the maser served no apparent *theoretical physics* purposes. When conceived by Townes, it would be as a useful tool: an instrument with a purpose, a potential technology for the investigation of microwave spectroscopy.

The Role of the Working Prototype

Only two years later in 1954 when the prototype maser had been built and could be seen to work was Townes' physics vindicated in the eyes of his colleagues, and only then were a range of organisations stimulated to develop the maser for a variety of applications (Bromberg 1991: 21) that included a precise clock (Bromberg 1991: 25), missile guidance systems (Bromberg 1991: 26) and in Bell Laboratories as a low-noise amplifier and communication device[6] (Bromberg 1991: 27). The long process of development of laser technology for a proliferating list of uses had begun.

The role of the working prototype in validating the idea of the maser is demonstrated by the status of Townes' idea before the construction of the device. Townes worked in the Columbia University Radiation Laboratory, funded by and dependent upon the military, and his colleagues had clear ideas about what was specifically useful to the military. According to Townes, they took the view that his work was *not* useful, because the physics was unsound, and that therefore it might endanger the laboratory's military funding. There were 'gentle suggestions that I should do something in tune with the Pentagon's interest in magnetrons' (Townes 1999: 52) and at one point the head and former head of the department attempted to persuade him to abandon his maser development work (Townes 1999: 65). Townes had tenure and could afford to ignore their intervention. In other words, by Townes' account, it was with a background of disapproval that he pursued the creation of a working device.

The Role of the Military

There is no doubt that military funding was very important in *facilitating* the invention of the maser; through the wartime work that had given physicists like Townes radar engineering expertise; through the radar equipment that had resulted and that could be put to spectroscopic uses; through their grants to the Columbia Radiation Laboratory where Townes was employed; and through their funding of the organisation of the advisory committee that acted as a prompt for his development of the maser idea.

With this variety of forms of funding intervention it is a temptation of hindsight to imagine that the military made *specific* demands for the new technologies that would 'meet their needs'. Townes is adamant that the military never specifically funded the development of the maser or laser and takes pains to correct what he clearly thinks is a deviant interpretation:

> Some science historians, looking back on those days, have concluded that we were being in some way orchestrated, managed, manipulated or manoeuvred by the military, as though the Navy already expected explicit uses for millimetre waves and even anticipated something like the maser and laser.... From our vantage point, the Navy didn't have any specific expectations at all about something like the maser or laser.... The military seemed quite uninterested in my maser work until some time after it was proven. (Townes 1999: 68)

The military were not funding research into radiation in a fit of altruism. They understood that such fundamental scientific research had proven useful during the war and they expected and hoped that it would prove useful again. However, they had no way of knowing when and how such research would prove useful: their method was to fund scientists and trust and hope that they would eventually generate useful knowledge. They had to refrain from attempting to select between different paths to the general end of developing improved means of generating and receiving radiation. In this way they acted as pioneers of the public funding of science.

The physical effects that the maser exploited may be obscure to the non-physicist; nevertheless it is clear that the cognitive act of insight was prompted by social context and prior experience and expertise. The laser also illustrates that an intended use need not relate to economic criteria, nor need its subsequent uses relate to the original intended use.

The invention of penicillin provides an interesting contrast to the laser. It can also be described using the terms of physical effect, intended and actual use, but intended use has a very different role in the penicillin story to its role in the invention of the laser. Thus the story of the invention of penicillin promises to extend our understanding of the process of invention.

The Penicillin Discovery Myth and the Unclear Significance of the 'Mould on the Plate'

The penicillin story has been popularised in the Anglo-Saxon world as an example of the good that science can do for society.[7] This popular and mythical story of the discovery of penicillin is usually represented as a classic of 'discovery through observation' and would typically run as follows: scientist Alexander Fleming observes a culture plate that contains both a mould and colonies of bacteria and observes that the bacteria have died in the area immediately around the mould. Fleming realises that the mould has secreted a substance that has killed the bacteria – and so he has discovered penicillin. The story is sometimes completed with the aphorism 'chance favours the prepared mind'. And it is implicit in this story that with this critical observation Fleming understood the significance of penicillin – in our terms, he at once connected the 'effect' demonstrated by the mould on the plate to the 'intended use' of penicillin-as-antibiotic.

Yet if he ever made this connection, he almost certainly dismissed it within a few months. Through its contrast with the popularised myth the 'true' story of penicillin both enriches our understanding of the inventive step and demonstrates how and why the innovation myth prospered.[8]

The Reinterpretation of Fleming's Discovery of Penicillin

The first great problem for the 'myth' of invention-upon-observation is that 11 years passed between Fleming's observation of the mould-on-the-plate in 1928 and the serious *development* of penicillin-as-antibiotic by Florey's research team at Oxford University. If Fleming made the correct link, what did he do with it for these 11 years?

Fleming wrote up his work on penicillin, but in the 27 research papers he published between 1930 and 1940, there are only two lone references to the potential therapeutic value of penicillin and these are really references to penicillin having a possibly significant *antiseptic* effect, not an antibiotic effect (MacFarlane 1984: 249). It is significant that in his 1931 paper entitled 'On the Indications for and the Value of the Intravenous use of Germicides' he does not mention penicillin at all (Hare 1970: 107). The absence of written or oral advocacy of penicillin as antibiotic is strong *prima facie* evidence that Fleming had not understood the potential of penicillin. If he had understood its significance, then his behaviour was extraordinary to the extreme.

MacFarlane used Fleming's publications, interviews with his colleagues and, most importantly, Fleming's original laboratory notes to reconstruct Fleming's thought at the time of his experimental investigation of penicillin (MacFarlane 1984). In their accounts of the invention, both MacFarlane and Fleming's former colleague Hare take pains to establish the state of bacterial expertise and Fleming's previous professional experience as influences on his behaviour at the time of the penicillin discovery.

Significant Available Professional Experience and Knowledge

In the 1920s the single existing effective systemic treatment for a bacterium was Salvarsan, an arsenical compound developed by Ehrlich that could cure syphilis. It worked as a poison that happened to be more poisonous to the delicate syphilis bacteria than to the cells of the body – but it had to be administered with care and with exactly the right dose to avoid human organ damage.

Fleming had more experience with the injection of Salvarsan than anyone else in Britain and MacFarlane comments that it was strange that Salvarsan appeared not to prepare Fleming or anyone else for the potential of penicillin (MacFarlane 1984: 251). Yet the probable reason why it did not is suggested by Hare, who comments that at the time 'even the most optimistic of bacteriologists' thought that any antibacterial infection compound would function essentially as antiseptics functioned (and as Salvarsan functioned) – as general poisons that destroyed bacterial cell proteins more effectively than the cells of the patient (Hare 1970: 142).

In addition to Salvarsan, Fleming (with the head of his laboratory, Wright) had spent years testing the new antiseptics that were constantly being proposed as effective treatments of surface infections. He had consistently shown that antiseptics applied to wounds worsened recovery rates because they killed the immune system's white cells faster than they killed infecting bacteria (MacFarlane 1984: 86). MacFarlane is even able to cite Fleming apparently drawing the general conclusion from this work that there was little chance of finding a chemical agent capable of destroying bacteria in the circulating blood (MacFarlane 1984: 109).

What no one anticipated was the radically different mode of action of penicillin and the other antibiotics. Not until 1957 was it understood that penicillin works by preventing synthesis of a polysaccharide that bacteria use to build their cell walls (MacFarlane 1984: 146). It therefore does not kill mature bacteria, but prevents the growth of new bacteria. In the body this allows the immune system to overwhelm the still living, mature bacteria, but in a test tube mature bacteria would persist in the presence of penicillin.

Fleming's Interpretation of His Penicillin Experiments

Fleming abandoned experimental investigation of penicillin only three months after the discovery of the mould-on-the-plate. According to MacFarlane the pattern of his penicillin investigation initially followed that of an antiseptic. Yet he extended his toxicity tests in a way that MacFarlane suggests shows that he did suspect penicillin might have had some systemic antibacterial activity.

Fleming had first demonstrated that, unlike most antiseptics, penicillin was non-toxic in the body, but his crucial 'extension' experiment was the injection of penicillin into a live rabbit to test its persistence in the body. He showed that within the short time of 30 minutes, penicillin was eliminated from the animal's body. He had also observed that penicillin took many hours to kill bacteria in culture and that this activity apparently diminished in blood serum experiments in glass containers *outside* the body. MacFarlane comments that these two results

> must have dashed any hopes that Fleming might have had for it as a systemic antibacterial agent. He did no further animal experiments and in consequence did not progress to the sort of protection tests that might well have encouraged him (and others) to greater efforts. … Fleming had (or probably thought that he had) good reason to suppose that penicillin would be useless in the body. What would be the use of injecting something that takes over four hours to kill bacteria when it is itself destroyed in a few minutes? (MacFarlane 1984: 128)

The crucial 'animal protection' experiment that Fleming did *not* perform involved, as the Oxford team would later design it, the injection of penicillin into eight mice, four of which had been previously injected with a standard lethal dose of bacteria. The four mice given penicillin survived, the four without all died. This experiment would later do for penicillin what the first maser did for *its* technology: it showed that it *worked*, that penicillin was active and effective within the body against bacteria. The success of this experiment provided the motivation for a further scale-up of production to enable human trials.

For MacFarlane and Hare the best explanation for Fleming's failure to perform the animal protection experiment is that by early 1929 he had allowed himself to become convinced that penicillin was without therapeutic value as an antibiotic[9] (Hare 1970: 99). In our 'reconstruction' of Fleming's probable state of mind when he abandoned penicillin research, he had the experience of repeated negative results in his antiseptic work and he had Salvarsan as a 'model' of a working whole-body, antibacterial agent. This current state of disciplinary expertise and the *absence* of an understanding of, or hypothesis about, penicillin's true mode of action suggest that Fleming would have had a strong predisposition not to find the non-toxic penicillin a credible candidate as an antibiotic. His motivation for the experimental investigation of 'penicillin-as-antibiotic' was weak from the start and this coupled with his probable interpretation of his experimental results was enough to destroy his interest entirely (MacFarlane 1984: 128). Amid a welter of weakly antibacterial substances competing for attention, he simply did not believe it was credible that penicillin was something special – a true systemic antibiotic with a novel mode of action.

Fleming's Real Contribution to Penicillin 'as Innovation'

Fleming did find a use for penicillin. Fleming's laboratory was self-financing and drew much of its income from the production of vaccines. Fleming realised early on that penicillin's selective *inhibition* of common bacteria in culture could be used to grow pure bacterial cultures necessary for the preparation of some vaccines. This was Fleming's actual and enacted 'concept of use' for penicillin – he maintained penicillin in culture for *use as a laboratory reagent*.

If we ask what was his contribution to the 'innovation' of penicillin as antibiotic, then he was neither innovator (he never brought penicillin to market) nor 'inventor' (since he dismissed the major future use of penicillin). Nor was he the 'discoverer of an effect', since so many others had made similar observations before him – there was even a book published in 1928 that reviewed existing work on moulds' inhibition of bacterial growth, that he apparently never consulted or knew about (MacFarlane 1984: 136).

His contribution to the idea of 'penicillin-as-antibiotic' was therefore this maintenance and free distribution of a living culture of an extraordinarily powerful strain of mould. He never understood how rare or powerful it was (MacFarlane 1984: 264) and had he not found his bizarre use for penicillin and so kept it in culture, it would have been very unlikely to have been found again (MacFarlane 1984: 137). Without a sample of his mould, his 1928 paper would have been of little use to Florey and Chain at Oxford 11 years later.[10] So after all, the penicillin story remains an extraordinary illustration of the role of chance in invention.

Lessons from the Invention of the Laser and Penicillin

In both cases the creative mental step can be understood as the conception of a link between the effect and a use, but in neither story did the relevant physical effect immediately and unequivocally suggest its uses. In both cases professional expertise and experience mediated the inventor's estimation of the credibility of the link and so influenced the inventor's motivation to proceed with the laborious work of investigation. In both cases the conversion of scepticism to belief in the credibility of the 'idea' required an appropriate demonstration of feasibility; this was the essential feature of the experimental work on penicillin and the development of a working device for the laser. The categories of 'scientific experiment' and technological feasibility collapse into one another here.

In both cases the first real uses differed wildly from later uses, but served the vital functions of motivating the inventor to create something that worked (the maser), or to maintain something in use (penicillin). These 'initial' technologies became material and cognitive resources that the inventor and others could draw upon to develop the technology for other uses.

Because of subsequent development and evolution of uses, in both cases it requires some effort to disengage ourselves from our familiarity with some of the uses of the technology today, so that we do not prejudge the process of invention in the past. The impact of penicillin as an antibiotic has truly revolutionised medicine and it is our familiarity with this current important use and the trivialising popularisation of the penicillin myth that make it difficult to believe that another conception was possible and preferred when it was discovered. The laser differs in that it has been developed into a range of technologies with quite distinct uses; nevertheless, Townes' intention to use the laser as a spectroscopic instrument first probably appears to most of us as a relatively arcane use and striking for its professional scientific, rather than directly economic, origin.

The Role of Existing Patterns of Use in Invention – Reference Markets for Innovation

It is because of their subsequent importance as technologies that the invention of the laser and penicillin have become interesting and rightly attract much attention. It would nevertheless be a mistake to imagine that the creativity intrinsic to the inventive step was confined to such radical steps. The more normal context for the inventive step is a developing technology with established uses. When a new technology substitutes for an older technology, it again becomes interesting to analyse the inventive process, for the established uses of the old technology become a cognitive resource for the newer technology's development, even while the established capabilities of the old technology may remain largely irrelevant.

Some years ago I went so far as to invent the term *reference market* to describe the cognitive role of an existing pattern of use of an established technology, and distinguished this from the *innovation market concept*, the projected future market for the developing technology (Howells 1997). The innovation market concept is a mental construction of those qualities by which prospective users would value the new product and that managers use to guide the construction of new production technology. The reference market, on the other hand, is an existing market based on real, traded products; an existing pattern of production and use which is understood in most detail by those producing and consuming these products. The reference market is so called because it is the market conception to which the innovating firm managers refer most often in the process of constructing the innovation market concept; it is the major cognitive resource in building the innovatory market concept. The construction of the innovation market concept is a cognitive process of understanding the qualities that make up the reference market and *selecting* those that are valued, for inclusion in the innovation market concept.

The cost of generating new jargon then seemed worthwhile, because it allowed a discussion of how the old patterns of use set the context for the construction of the new, intended patterns of use. And perhaps it was worthwhile, for others have also found it necessary to invent a name to distinguish between how existing objects are used and how innovators conceive of future patterns of use; Hounshell and Smith appear to use the term 'target market' in essentially the same way as I use 'reference market' (Hounshell and Smith 1988).

The distinction between the market concepts accommodates the frequent observation that in innovation existing users' articulation of their needs is strongly influenced by the properties of the technologies that already exist and so cannot be entirely trusted as a basis for development. Some intelligent interpretation and amendment of understanding of the reference market may be necessary to obtain a viable innovation market concept. The original study furnishes an interesting example of how the reference market can be deficient as a source of ideas for the innovation market concept.

Reference Market for Bioprotein Innovation

Until the oil price rises of the 1970s there was a general fear of an impending protein shortage and this acted as a general stimulus for the development of bioprotein fermentation technologies. These involved the controlled fermentation by microbes of fossil-fuel-derived inputs such as methanol or North Sea gas to produce a high-protein biomass product.[11] The 'reference' or 'target' market for the fermentation technology was the existing animal feed market. This market is serviced by specialist 'feed compounders' that match varied proportions of inputs, largely soya and fish meal, to the various nutritional requirements of animals, whether goldfish, gerbil or veal calf. So the innovating firms guided their development process by reference to the compounders' understanding of how to combine protein sources to make a 'good' animal feed. So a good feed for veal calves is one that gives rapid weight and lean tissue gain without tainting the flavour of the flesh and this is understood to relate to the amino acid, mineral and vitamin content of the feed inputs. The compounders therefore valued feed inputs by these properties and their valuation procedures were the basis for the bioprotein innovators' anticipated valuation of their novel feed products. For example, economic value could be attributed to the 'property' that, in contrast to fish meal, high-protein novel feeds did not give animal flesh a fishy taste.

The role of the trace nutrient selenium in the novel feeds provides an example of a limit to the compounders' ability to articulate 'user needs'. In some of the feeding trials conducted by the chemical company ICI on its protein product, named Pruteen,[12] animals sickened when fed very high percentages of the novel feeds. ICI's R&D department then conducted experiments that showed that this was because Pruteen was selenium deficient. Once selenium was added to Pruteen-based feed, animals thrived on a high-Pruteen diet. The problem had arisen because Pruteen's production engineers had adjusted selenium inputs to the fermentation process to meet the nutritional requirements of the bacterium. Because this bacterium differed from multicellular life forms in having little need for selenium, the result was that compounded feeds containing very high percentages of Pruteen could then be selenium deficient compared with conventional feeds.

An understanding of the role of selenium in animal feed was an essential element of the innovation market concept for novel feeds, but one unavailable from the reference market articulated by existing 'users', because fish and soya meal contained more than sufficient selenium for all animal feeds; feed compounders had never had to specify minimum selenium content and so could not articulate such a requirement for the novel feeds.

The danger for the novel feed innovators had been that if government agencies produced adverse results from similar feed trials, they would not be motivated as the innovators were to understand their results. They would be more inclined to conclude that the novel feed products were toxic, or were potentially toxic. Even the suspicion of toxicity could be fatal to this kind of innovative effort.

The example shows that the reference market alone may be a deficient source of understanding of the market for innovation and that 'scientific' research rather than 'market' research can be the source of the future-market understanding on which successful innovation must be based. It is also a demonstration of how development involves inventive steps in its own right and cannot be thought of as merely the exploitation of the original inventive idea.

The User as Originator of Market Concept

If in the reference market case we have an example of the need for continued intelligent investigation of a strong guiding concept of existing use, then the point should be made that in general 'users' vary greatly in their ability to recognise and articulate their 'needs'. The 'work' that a technology developer must do on the market concept varies greatly depending on technology and circumstance. Within the innovation literature this is recognised in, for example, Souder's 'Customer Developer Condition' model and Souder gives examples of users presenting their requirements even in as detailed a form as a product specification (Souder 1987). In this case of 'sophisticated user' Souder argues that the developer firm's marketing department has little or no role, but that the less sophisticated the user, the more the marketing department of the developer must compensate by (in our terms) active development of reference and innovatory market concepts.

Von Hippel develops the idea that the relative contribution of users to innovation varies systematically by technology through his studies of innovation in scientific instruments. He studied the origin of not only the first, but also all subsequent major and minor improvements that occurred in a period of 20 years to four important types of scientific instrument (gas chromatograph, nuclear magnetic resonance spectrometer, ultraviolet absorption spectrophotometer and the transmission electron microscope). For 77% of his 111 innovations the user – typically a scientist working in a university – not only made the invention, but then researched and built the prototype to demonstrate its feasibility (von Hippel 1988: 13). In other words, what we found to be Townes' motivation in the invention of the laser is of common occurrence.

Von Hippel's examples of user-dominated innovation processes (others are the semiconductor and printed circuit board assembly processes) are another version of Souder's sophisticated user scenario, except that for scientific instruments another typical function of the developer, the creation of prototypes, the user now performs.

The picture that we now have is that stages of the innovation process may be variably located outside the firm that finally produces and markets the product. Like Souder, von Hippel points out that the work of R&D and marketing departments must be adapted to the typical source of innovation in these technologies (von Hippel 1988: 9) and that in general we must take care not to assume innovation takes place in a 'stereotypical' innovating firm possessing all the necessary functions to generate successful innovation.

This is fair enough, but there is an obvious response – that scientific instruments make a special case because *by definition* scientific 'users' have developer abilities at the laboratory-scale level – all scientists are trained in the manipulation of instruments for experimental purposes. This is fundamental to the practice of science whether in an R&D department or university laboratory. It makes sense that the evolution of science may involve the evolution of those instruments and techniques – the technology of scientific instruments. This is exactly what happened in the invention of the laser. Scientists and engineers are the one 'user group' that is in a position to engage effectively in its own product R&D and the scientific instrument 'industry' essentially complements their R&D capability with a mass manufacturing capability.

On the basis of his research von Hippel advises managers to assess their own industry for its characteristic location of innovation activities and to organise their innovative activities around this. One might argue that industries anyway evolve structures that account for the various characteristic strengths and weaknesses in user innovative abilities. An illustration in extreme contrast with scientific instruments is the development of nylon by the DuPont chemical company. DuPont not only produced novel textile fibres, but it worked closely with the fabricators of conventional textiles to create novel textile fabrication technology that would ensure that its novel materials could be turned into valuable textiles: for nylon, nearly every stage of the conventional, silk stocking fabrication process had to be changed to avoid the dangers of the finished product being wrinkled, discoloured or snagging easily. These intermediate users could not have done this without DuPont. It had to extend its organisational 'reach' into its users' fabrication technology so that it could secure the profits on its chemical innovations (Hounshell and Smith 1988: 264).

These examples certainly demonstrate that the ideas and various steps of development may be distributed between organisations in different ways, but more than this, the variation is a result of active accommodation to the innovative strengths and weaknesses of existing organisations.

Invention and Innovation as Socio-cognitive Processes

A striking feature of the stories of invention was the role of social context and prior expertise for the cognitive act of insight. A major point of contrast was that while these worked to prompt the act of insight that generated the laser, they also worked to weaken motivation to investigate penicillin-as-antibiotic. In neither case would it make sense to judge the cognitive part of the process as separate from social context and prior expertise. Of course, that is exactly what has happened in the generation of the myth of penicillin discovery by Fleming. Through simplification the context was dropped and the act of insight alone, falsely credited to Fleming, imagined to be the work of invention.

Because of the different outcomes in the laser and penicillin, it seems important to capture the sense that social context and expertise influence the act of insight that is more commonly understood as the inventive process. Several authors, including myself, have coined the term 'socio-cognitive' to capture the intertwined nature of social and cognitive context in invention and the development of technology (Howells 1995; Garud and Rappa 1994; Garud and Ahlstrom 1997).[13] Invention, then, can be understood as a socio-cognitive process. Examples from philosophies of science and organisation studies show that there is a degree of convergence among disciplines on this basic understanding: that the social and the cognitive interact, are not independent and the interaction can and should be studied.

Socio-cognitive Processes in the Philosophy of Science and Organisation Studies

There are many theories about science and the development of scientific knowledge, and as with the approaches to technology discussed in the last chapter, approaches to science have tended to be characterised by the academic discipline of the theoriser. It may be significant then that Thagard's recent work (1999) in the philosophy of science has also moved in the direction of a socio-cognitive understanding of science, so breaking with a tradition in that subject for a purely logical analysis of scientific method.

After a short review of the logical, psychological and sociological theories of science, as a philosopher of science, Thagard argues that cognitive and social explanations can be 'complementary rather than competitive, and can be combined to fit an Integrated Cognitive-Social Explanation Schema that incorporates both mental processes and social relations' (Thagard 1999: 19). For our purposes, the essential feature of this work is that it insists that science should be understood through the analysis of *interacting* cognitive and social practices and that it develops the argument most forcefully through the analysis of a principal, but complex case: the success of the theory that peptic ulcers are caused by a bacterium.[14]

The significant characteristics of the case can be outlined in social and cognitive terms. Gastroenterologists were the community of medical specialists that studied and treated ulcers. Marshall and Warren, two scientists from outside the gastroenterological scientific community and possessing expertise in bacterial treatment, thought of using relatively novel bacterial staining techniques to demonstrate that a bacterium, *Helicobacter pylori*, was present in the majority of gastric ulcer tissue samples. Acceptance by the gastroenterologist community of the bacterial theory of ulcer causation was delayed because of the novelty of the technique, the outsider status of the discovering scientists and the long-standing assumption on the part of this group that no bacteria could survive the extreme acidity of the stomach. Such was the strength of this belief that one of Marshall and Warren's early conference papers was rejected by the Australian Gastroenterological Society, and the paper they submitted to the *Lancet* was delayed by negative reviews of its plausibility (Thagard 1999: 88). However, these social and cognitive blocks served only to delay acceptance. Gastroenterologists first accepted the existence of the bacteria that the new staining techniques could reveal. It took a little longer for them to accept that *H. pylori* was the causative agent of ulcers, but controlled experiments with antibiotics showed a significant rate of cure and the experiment could be replicated effectively. The delaying effect of social and cognitive obstacles in this example was in the order of years, not decades.

In mainstream organisation research, March and Simon could write in the late 1950s that there was little concern with cognition; that is, with the adaptive, reasoning behaviour of human beings in organisations (March and Simon 1973: 233). Their book expressed the information and attention limits to human mental capacity through the term 'bounded rationality'. Cognition has received more attention since March and Simon, for example, the interaction between the cognitive and the social is recognised in Weick's work. Here individuals are understood to be theory-makers, or sense-makers on their own account, but what they understand to be useful knowledge and their ability to elaborate cognitive understanding are strongly influenced by how they are organised within the firm (Weick 1979). What this work generally ignores is the role of the artefact in the sense-making and negotiation over meaning and purpose that occur within the firm: artefacts, like organisations, are not 'facts' with entirely self-evident meaning; that has been the starting point of this book. Weick's work, drawing on the work of the psychologist Donald Campbell, can be applied to technology if the artefact is included as one of the elements of organising. So not only invention but also innovation should be understood as a socio-cognitive process, for the development of technology is a form of organising. The value of this approach in innovation is that it accommodates the micro political processes that occur, for example, when project conditions change and a new understanding of the viability and future of the project must be negotiated. Such questions may open and close throughout project lifetimes, as will be shown in the next chapter.

The way I have introduced 'socio-cognitive' is as a category, a new piece of jargon, and not a theory. For the study of innovation its value lies as much in the understanding it avoids as in the orientation towards understanding that it represents. So on the one hand it clearly breaks with the idea that invention could be no more than an isolated act of insight, from 'out of the blue'. On the other hand this term 'social' is loose and not deterministic, and this is deliberate. In our search for the creative element of innovation, we have found that the source of ideas and early idea-proving activities are not necessarily confined to one category of organisations. We found that there could be selective sourcing of ideas for a prospective new market in existing patterns of use – the reference market. Of course, just as the source of ideas can be variously sourced in different organisational forms, new ideas of use can become the basis for new organisations in development. For all these reasons it seems fair to increase the burden of jargon and to describe invention and innovation as socio-cognitive processes.

The term also has the virtue that it avoids the undue determinism that infects the most widespread alternative model of the relationship between technology and its uses, the characterisation of innovation as a product of either 'technology push' or 'market pull'. The push vs. pull metaphor survives in management studies despite its decline in the field of innovation policy dating from Mowery and Rosenberg's critical review of its abuse (Mowery and Rosenberg 1979). It will prove worthwhile to revisit this idea both because of its persistence and for the explanation of why it is not a good theory of the relationship between technology and its uses.

Popular 'Explanations' of Innovation – Market Pull, Technology Push Revisited

Some version of the push vs. pull 'explanation' of innovation can be found in texts as different as Simon's study of the success of small, innovative German companies, Fruin's field study of a Toshiba electronics plant and Kotler's textbook on marketing (Simon 1996; Fruin 1997; Kotler 2000). So in Simon's discussion of the 'driving forces' for innovation he uses survey evidence that sorts managers' choices of whether their companies are primarily market driven or technology driven (Simon 1996: 131). It is not clear whether the opposition between technology and market is assumed by his research method or represents the managers' own thinking about the sources of innovation, but the most significant feature of this research is that the technology and market are understood to be alternative sources of innovation.

Marketing texts are the other major site of survival of the idea that technology and market are in opposition, but first it makes sense to return to the problems with the original push vs. pull studies of the 1960s and 1970s.

Myers and Marquis' study of the technology-push or market-pull 'causes' of a collection of innovations provides an example of the kind of conclusion that these studies tended to generate.

> The primary factor in only 21% of the successful innovations was the recognition of a technical opportunity. Market factors were reported as the primary factor in 45% of the innovations and manufacturing factors in 30% indicating that three quarters of the innovations could be classed as responses to demand recognition. … Recognition of demand is a more frequent factor in innovation than recognition of technical potential. (Myers and Marquis 1969: 60)

Even in this quote it is evident that it is the way the authors define 'market factors' that allows them to draw the simple and suggestive conclusion that recognition of demand is more important than technical potential – why should manufacturing factors be included as a subcategory of market factors? Myers and Marquis' other categories of 'demand' include: 'anticipated potential demand'; 'response to a competitive product'; 'attention drawn to a problem or inefficiency'. Reflection on these categories makes one uneasy with their classification as 'demand' factors: is 'response to a competitive product' not as much a response to the competitor's technological choice for that product as to that competitor's market choice? There is an evident degree of arbitrariness here.

Mowery and Rosenberg's critical review found that most of the innovation case surveys, like Myers and Marquis, drew the conclusion that demand/pull factors were more important than technical factors (Mowery and Rosenberg 1979). This tendency to privilege demand over technology as a cause of innovation mattered because it was used to support government policies of 'laissez-innover' – a non-interventionist policy of leaving innovation to the market. Mowery and Rosenberg drew the contrary conclusion that the evidence and methods of the studies did not allow one to privilege 'market pull' over 'technology push'.

They showed that the meaning of technology push and market pull had no inherent precision and varied between research projects, so that comparison of results was difficult. There was a tendency across studies to define the two categories such that most innovations were classified as 'market pull', but these studies did not recognise that their defined category of technology push tended to include more of the radical and important innovations than the 'market-pull' category.

According to Mowery and Rosenberg, much of the problem derived from the imprecise use of the economics term 'market demand', which suggests the 'market-pull' explanation for innovation, and in their view:

> Market demand must be clearly distinguished from the potentially limitless set of human needs. Demand expressed in and mediated through the marketplace, is a precise concept, denoting a systematic relationship between prices and quantities, one devolving from the constellation of consumer preferences and incomes. To be taken seriously demand-pull hypotheses must base themselves upon this precise concept and not the rather shapeless and elusive notion of "needs". (Mowery and Rosenberg 1979: 229)

This is a valuable critique of the 'push vs. pull' body of work, but it raises the question of whether it is really possible to use the concept of 'market demand' from economics to explain innovation.

There are problems with the precise concept of market demand; many of the assumptions required to make this concept precise have little to do with the real world and everything to do with creating a mathematically tractable concept (Ormerod 1994). These include assumptions about consumer preference functions, that supply and demand curves are (mathematically) continuous functions rather than discontinuous, that they can make incremental shifts and that they are knowable. Further, market demand is a static concept, that is it applies to a known good or service, and does not inform us of the mechanism by which shifts in supply or demand curves generate new products. Finally, it is this very concept of market demand that did inspire many of the push vs. pull studies, and their problems may be taken as evidence that it is difficult to operationalise the idea.

If the precise idea of market demand must be abandoned as a basis for explaining innovation, we appear, in Mowery and Rosenberg's terms, to be left with the 'rather shapeless and elusive notion of "needs"' and the continuing opposition between technology and market suggested by the popular push and pull metaphor. The trouble with the push vs. pull metaphor is that it is a *bad* metaphor. The worst feature is its statement of a false opposition between technology and market in the development of innovation. Technologies are designed for a purpose and so some idea of use is always implied, and when technologies are changed a change in use is also implied.

The push–pull formulation also confuses the role of agency in development; *all* technological innovation requires 'push' – human effort – for its development. It must be made to happen by motivated people, typically organised within the firm and this is obviously true even if this agency is distributed between actors and includes users as contributing developers. However, it was earlier argued that markets serve as patterns of use that are a source of ideas in the innovation process – they are not agents that can push, pull or demand specific innovation. To oppose developer push with market pull is therefore additionally misleading. Nevertheless, because certain innovations, such as barbed wire as discussed below, appear to strongly invite classification as 'market pull', a little more analysis will prove useful.[15]

Desperately Seeking 'Market Pull' – the Invention of Barbed Wire[16]

As cattle ranches spread west and into the great plains of the USA in the nineteenth century, a fencing crisis developed because there were few trees and traditional wooden fencing was prohibitively expensive in its role as an effective property demarcator. Farmers could not protect their crops from freely roaming herds of cattle, so

> fencing quickly became the farmer's primary concern. Between 1870 and 1880 newspapers in the region devoted more space to fencing matters than to political, military, or economic issues.... In 1871 the US Department of Agriculture calculated that the combined total cost of fences in the country equalled the national debt, and the annual repair bill for their maintenance exceeded the sum of all federal, state and local taxes. (Basalla 1988: 51)

Such was the scale of the problem that Hayter writes that westward expansion in the early 1870s was 'slowed down considerably' (Hayter 1939: 195).

There was then a pressing 'need' for a 'wooden fence substitute', but the duration of the problem shows that it could not immediately generate an adequate solution. For a while it was thought that the solution was a thorny hedge grown from osage orange. Osage orange is a bush with long tough thorns at right angles to their stems. It grew naturally in some southern US states and a small cultivation industry developed to export its seeds to the plains region. However, it took three to four years to grow an effective hedge and of course it could not be moved if property boundaries changed.

Bare wire fences were cheap and in use but were a poor deterrent to moving livestock. According to Basalla, it was the pattern of thorns on osage orange that inspired the 'thorny fence', as one inventor called his patented wire invention (Basalla 1988: 53). 'Barbed wire was not created by men who happened to twist and cut wire in a peculiar fashion. It originated in a deliberate attempt to copy an organic form that functioned effectively as a deterrent to livestock' (Basalla 1988: 55). In other words, the 'biofact' of osage orange inspired the artefact of barbed wire, or in our jargon, osage-orange-as-fence/hedge acted as the reference market for the invention of barbed wire.

If the case invites a 'market-pull' classification it is because there is so much evidence of the damage done to frontier cattle farming by the absence of an economic fence substitute. The trouble is that on this basis it can just as well be classified as 'technology pull'. The need for a fence-substitute was acute because the fully developed *technology* of cattle farming had been transferred into a new environment where just one, albeit critical, artefact component of the working technology was missing. The idea of what is being 'pulled' is also unclear. There is no 'pull' acting on the idea of barbed wire: if there is 'pull' it is conventional market demand for the artefact component that cannot be supplied. In contrast to the laser, the case is better understood as a defined problem. But although a defined problem invites puzzle solvers, like the famous case of Fermat's last theorem, that does not mean the problem is easier to solve. And it certainly does not imply that solutions to defined problems are better as a 'class' than proposed improvements to what already exists and only appears to be functioning well enough.

The case is a good demonstration of the problem of 'needs' in innovation. Genuine and general human needs such as shelter and food are relatively few, obvious, and tell us nothing about why and when specific innovation events occur. Acutely expressed 'needs' as in the barbed wire case often have a technological context and do not necessarily exist for people outside of that context.

We shall adopt the view that 'needs' have been proven to exist by innovation *success*: that is, if users are prepared to switch spending from current products to the innovation. When the firm develops its innovation it no doubt hopes that its intended use will prove to be a 'need' when the product reaches the market – but given the context-dependency of needs, it should not be fully confident.

In sum, the push vs. pull metaphor is objectionable because it is misused to falsely oppose technology to market as a cause of innovation, or to falsely oppose the development of technology as an alternative to development for a market. It is a misleading and widespread oversimplification that obscures understanding and the terms 'technology push' and 'market pull' are probably best avoided in all their many manifestations. Important in its own right is the manifestation of the opposition of technology and market in the marketing literature, for this is where a naïve student might look to find these issues discussed clearly.

In Practice the Split Lives On – the 'Marketing Concept' of the Marketing Literature and Technology

Many marketing texts begin with an effort to define the conceptual basis of the subject and this can drift into a statement that resurrects a version of the 'push vs. pull' metaphor. Early in Kotler's *Marketing Management* comes a statement of the 'marketing concept':

> the 'marketing concept' holds that the key to achieving its organisational goals consists of the company being more effective than competitors in creating, delivering and communicating customer value to its chosen markets. (Kotler 2000: 19)

Few would object to this, but then it is claimed that this is a 'business philosophy that challenges' both the 'production concept' that 'holds that consumers will prefer products that are widely available and inexpensive' (Kotler 2000: 17) and the 'product concept' that 'holds that consumers will favor those products that offer the most quality, performance, or innovative features' (Kotler 2000: 17).

Kotler has defined the product and production 'concepts' as if they are distinct from the marketing concept, and by that word 'challenge' it is implied that they are perhaps inferior. They are better understood as specific examples of the marketing concept itself, in some unspecified, but surely relevant, technological context. In other words, the distinctiveness of the defined concepts and the idea that they are 'competing' (Kotler 2000: 17) in some general sense as alternative concepts are misleading here. This is even implied by the examples used, which allow, for example, that the production 'concept' is a valid contemporary market orientation of Texas Instruments.[17]

Other marketing texts are less subtle than Kotler and claim there is an historic succession of business concepts that run in the USA from a production orientation until the 1940s, through a product orientation in the 1950s and 1960s, to the eventual marketing orientation of today (Lancaster and Massingham 2001: 4–7). Lancaster and Massingham use the Japanese-import-driven decline of the British motor cycle industry to 'quite simply' illustrate what they mean by a 'product orientation' (Lancaster and Massingham 2001: 5). They warn us that 'this orientation leads to a myopic view of the business with a concentration on product engineering rather than on the customer's real needs' (Lancaster and Massingham 2001: 5). Yet good product engineering does not necessarily exclude an understanding of the market, as is shown by this same example in the same textbook. The success of the Japanese derived in part from the 'product-engineering' replacement of motorcycle kick starting with electric starters. Even with the limited detail available (no source is given) the strong claims that success was derived through the choice of a market orientation rather than a product orientation can be seen to be false. In short, neither the claims of an historic succession of general orientation concepts that culminate in the marketing orientation, nor the distinctiveness of these concepts themselves, should be accepted. The real marketing issue in innovation is how to manage technology development for an articulated market concept, when the market concept is not fully articulated by users.

A parallel confusion dogs the presentation of the basic issue of 'needs' in the marketing literature. Marketing texts usefully acknowledge that users are not necessarily aware of their real 'needs', but then they tend to emphasise the value of marketing as the means of investigating and servicing the 'real needs' that are supposed to exist – indeed, this is the very definition of marketing as a business activity (Kotler 2000). Once again, the definition, but not the examples, is in isolation from any technological context. So in Kotler's text, Sony 'exemplifies a creative marketer because it has introduced many successful new products that customers never asked for or even thought possible' (Kotler 2000: 21). One can agree that Sony is a creative marketer, but hold an alternative interpretation of marketing to Kotler's, that the most creative and exciting marketing opportunities occur in those industries whose technologies are in a process of development; which is to say that the full set of stable technology–market matches have yet to be imagined and worked out in practice. In the view of this book, real marketing creativity occurs within the constraints and potential of a developing technology.

What these examples do show is that like any professional specialisation, marketing feels a need to justify itself by projecting an image of progression, development and universal applicability. Unfortunately the way the profession has chosen to do this is by the resurrection of the technology-push versus market-pull dichotomy and the associated claim of superiority for the market 'side' of the concept. It might therefore be foolish to expect this use of the push vs. pull metaphor to die out in the near future, but if it persists it will be at the cost of a marketing text that properly resolves conceptions of technology, market, need and use.

Return to Innovation as a Socio-cognitive Process

Innovation is fundamentally about how and why technologies are made to relate to various uses. In the market economy the common means of organisation of technology is that firms specialise in the creation and development of production technologies in order to trade and users are distanced from the organisation of production via the institution of the market. It is likely this organisation of technology that suggests the push vs. pull metaphor as a means of classifying innovation, but the story of the persistent use of push vs. pull instead serves to illustrate the danger of oversimplified and confused terms. When the false promise of such simplification is abandoned, one is forced back towards a craft model of knowledge in answer to the fundamental question of how and why technologies are made to relate to various uses. In other words, we need to accumulate experience in diverse examples.

The Management of Industrial R&D

Statistics of national and industrial sector R&D are available, but they cannot tell us about the experience of 'doing' R&D. If we want to understand the issues in the process of development – the sequence of events and decisions made by management that gradually shape a novel technological step – we can turn to the small body of business histories that cover research and development work.

Two of these will be considered here. First, Graham's account of RCA's VideoDisc project is one of those detailed historical studies that conveys the experience of radical technology project management in uncertain conditions.[18] The account here is organised around the perennial R&D management themes of the identity of competitors, the interpretation of market research and the role of project and company 'leadership'.

Second, Hounshell and Smith's unique history of the R&D function in the chemical company DuPont provides insights into the coevolution of corporate strategy and the R&D function. The account is organised chronologically over 80 years to highlight the interaction between competitive and regulatory environment and the understanding of internal capability and company strategy.

Leadership Lessons from Failed Project Development – RCA and the VideoDisc[19]

RCA successfully pioneered in succession radio, black and white television, and then colour television development to become the leading US consumer electronics innovator. Its dominance in television had been achieved by a strategy of simultaneously establishing a TV broadcasting standard and programme production for this standard – RCA owned the broadcaster NBC. The early availability of programme content ensured the dominance of RCA's choice of broadcasting standard and thereby the importance for rivals of gaining access to RCA's TV receiver standard. RCA then reaped profits from the lucrative market for TV sales through the licensing of its TV receiver standard to the manufacturers who might otherwise have become rival technology standard manufacturers in their own right. When this 'closed' or proprietary standard strategy worked, as it did for colour TV, it was immensely profitable, but it implied the coordination and control of major interacting technologies. RCA would bring this understanding of its past success to the VideoDisc project. It would tend to assume that it must continue with the closed standard strategy while it also mistook companies with similar 'systems-innovating' capabilities, such as the broadcaster CBS, as its most dangerous potential competitors.

Once a professional model of a videoplayer had been made available come the 1950s by the US company Ampex, the idea of a videoplayer for home use became an obvious one. The more difficult questions were *which* technology would make the better commercial product and *when* should this technology be advanced towards full-scale commercialisation? As befitted an industry leader, RCA Laboratories had three technologies as candidates for development: holographic, capacitance and magnetic tape technology. Each had its internal advocates, but external events played a pivotal role in forcing internal 'selection' of one technology over another – with the exotic outcome that each of the three enjoyed some period when it had been selected for commercialisation. How this could happen has significance for our idea of R&D leadership.

Competitor Perception and Project Selection

When CBS announced in 1967 that it was developing a photographic technology into a videoplayer product (Graham 1986: 102), RCA Laboratories management used the event to interest its senior management in their own candidate technologies for a videoplayer product. A process of product definition and selection was begun, but it was short-circuited when in May 1969 Robert Sarnoff, the chairman of RCA, learnt from watching a TV show that a Westinghouse colour camera (based on CBS technology) had been selected for the Apollo 11 space mission instead of RCA technology (Graham 1986: 114). The next morning he 'expressed his deep personal displeasure' and demanded to know what CBS would do next in a memorandum to his key appointment in new product planning, Chase Morsey (Graham 1986: 106). This pressure led Morsey to fear that CBS's planned public demonstration of its videoplayer technology for the autumn would be seen by Sarnoff as yet another successful challenge by CBS. To pre-empt this danger he brought forward a planned public presentation of the RCA videoplayer project from December, which had been the internally agreed earliest possible date for demonstration of the laboratories' preferred capacitance technology, to September, before the CBS presentation.

The laboratories had seen the holographic technology as a 'distant second-generation approach' (Graham 1986: 110) until this time, but it was closer to a demonstrable working prototype system than the capacitance technology and it was an ideal candidate to demonstrate RCA's 'continued technological leadership' (Graham 1986: 115) – this had now become the purpose of the public demonstration, and the audience was as much RCA's own chairman as the industry and public who would attend.

The public demonstration of this technology at the press conference naturally led observers to believe that this was RCA's *preferred* technology for a commercial videoplayer. Worse, the marketing staff had taken the laboratories' estimates for earliest product introduction and 'manipulated the data into a plausible, though highly optimistic, plan' (Graham 1986: 231). The marketing group went on to forget the dubious origin of the technology selection decision and began to implement the business development timetable that they had prepared as part of the demonstration (Graham 1986: 119).

> In the end, when the compressed goals proved to be unrealistic, the R&D organisation was charged with misrepresentation and Robert Sarnoff, never personally involved in the preliminary discussions, lost faith in the process. (Graham 1986: 231)

The credibility of the holographic approach was further undermined as the CBS project was abandoned and as it became clear that most other potential competitors had chosen magnetic tape technology (Graham 1986: 134).

In 1970 there came another announcement of an apparently credible VideoDisc threat, this time by the joint venture 'Teldec' using electromechanical technology – a version of the established 'needle-in-groove' record technology. The effect this time was to strengthen RCA management support for the laboratories' capacitance technology and to provide the development team with a set of benchmarks against which they could judge their own work. More researchers were added and a project manager appointed, reflecting the more serious 'weighting' the capacitance technology had gained (Graham 1986: 135).

Then in 1973 Philips demonstrated a radical, optical, laser-based VideoDisc system at an industry show that convinced RCA managers that RCA had an inferior product in features and performance (Graham 1986: 161). RCA's top management commitment to the VideoDisc project 'evaporated' (Graham 1986: 162). The VideoDisc development team responded with a crash prototype development programme to restore senior management belief in their technology. They duly demonstrated a capacitance prototype by the end of 1973 that beat the Philips technology, but at the cost of six months' development time. The example further confirms the vacillating nature of senior management's attitude towards the R&D department (Graham 1986: 162).

The problem with competitor announcements was accurate evaluation of their credibility and degree of threat. The tendency was to attach greater credibility to technologies supported by established powerful companies like CBS, mediated by internal research to check the claims of rivals – and, as we have seen, perceived internal political imperatives. More than 10 different videoplayer technologies were developed for consumer use in the 1960s and 1970s (Graham 1986: 22) and this long history of aborted efforts clouded judgement of what would turn out to be the real threat – from Sony and Matsushita.

Market Research and the Missed Japanese Competitive Threat

So Graham writes that when Sony's Betamax system was launched for $1300 in 1975, it was first seen by RCA as another opportunity to learn about the market by observing consumer response. Then when Betamax established itself in a high-priced niche, opinion was 'evenly divided at RCA between those who saw Betamax as direct competition for VideoDisc and those who maintained that the two products were so different that they complemented each other' (Graham 1986: 181). The idea of two distinct markets was supported by both the initial very high Betamax price and the belief that it was not credible that magnetic tape technology could be developed so as to achieve low prices. Although RCA ran extensive market research from the 1960s onwards and this always confirmed that consumers would be interested in a videoplayer product, in retrospect it misled RCA managers, not for reasons of 'quality' or 'fault' in the conduct of this form of research, but because the very understanding of what the market would be depended in part on technological forecasts.

In 1976 RCA market research on the performance of Betamax and the proposed VideoDisc remained 'inconclusive as to the relative appeal of the two different product concepts – recording, versus only playback – but it left very little doubt as to the importance of price' (Graham 1986: 190). The market forecast depended in turn on an economic forecast for the developing rival technologies. And the economic forecast in turn incorporated an industry-wide technological understanding that magnetic tape technology could not be developed to support a mass market product, but would remain high priced, leaving the mass market to some kind of VideoDisc technology. This expectation continued into the 1980s, as demonstrated by GE, IBM and even JVC (the successful developer of the VHS standard of magnetic tape video recorder) all having VideoDisc products in development by the early 1980s (Graham 1986: 212).

In sum, market research failed to predict the huge scale of the eventual market for videoplayers and it failed to predict the 'revealed preference' of consumers for video rental. The scale of the market was partly explicable by the sale of pornographic videos, which accounted for nearly half of pre-recorded videotape sales (Graham 1986: 214), while Graham suggests that the very proliferation of competing technologies may have shifted consumers' preferences towards rental (Graham 1986: 214).

R&D Leadership Lessons from the VideoDisc Project

Technological transformation had been fundamental to RCA's success and its founder, David Sarnoff, had created the corporate R&D laboratory as the means of continuing this strategy for success.

However, his successors began a programme of diversification into unrelated businesses (for example, Hertz car rentals) that absorbed their time and distanced them from the R&D laboratory. Graham comments that 'the most destructive effects of diversification on R&D must be assigned to a failure of leadership, the unwillingness or inability of top management to define a new mission for R&D when major change takes place' (Graham 1986: 232).

The pursuit of diversification led to a neglect of the R&D department – yet it continued to exist, product and symbol of RCA's past strategy of technology-led growth. In these conditions the laboratories were never secure in their future except through what they could promise to deliver, or as Graham expresses the situation:

> When the survival of the Laboratories depended on its clear identification with a proprietary, revenue-producing "blockbuster" project, the Laboratories could not be depended upon for reliable judgements about competing technologies. (Graham 1986: 225)

One of the results of this situation for the VideoDisc project was that:

> When there was too little money to fund exploratory work on high-resolution recording methods for its own sake, for instance, the electron-beam recording technique became the tail that wagged the VideoDisc dog. (Graham 1986: 225)

The laboratories had become committed to the capacitance technology with electron beam mastering for its prospective corporate strategic properties, not because it was the best technology for a VideoDisc product. Despite the implied lack of investigation into alternative mastering techniques in Graham's account, electron beam mastering had been defended by the laboratories just *because* it was difficult – the prospective advantage being that precisely because it was so difficult to perfect, it promised to make RCA's VideoDisc technology extremely difficult to copy and hence highly profitable for RCA to license *if* successful. This thinking also reflected RCA's history as a pioneer and leader of technological systems.

When senior management for the third time turned to the laboratories for a winning technology to compete with the developing threat of the Japanese magnetic tape products, they were prepared, for the second time, to back the development of capacitance technology. But it would then be found, very late in the day, that it was impossible to perfect capacitance technology as a mass manufacturing technology. It would eventually be abandoned leading to another protracted senior management crisis of confidence in the R&D laboratory's development ability. This time several years were lost before the former marketing manager now running the company realised that RCA's very future depended on it having a viable VideoDisc product – the patents and profits on colour TV were rapidly expiring. The fourth time around RCA chose, for reasons of developmental speed, a conventional electromechanical mastering technology as the basis for its VideoDisc player that was introduced commercially in 1981. This would prove several years too late. The huge costs of commercial launch were nevertheless incurred, contributing directly to the company's later dismemberment and the relegation of the corporate R&D laboratory to a mere contract research operation.

Close contact of some senior manager with the developing project has been noted as a feature of successful innovation by many studies, for example the comparative survey research of Project SAPPHO (Rothwell 1977). Graham's principal conclusion is also about the necessity of committed leadership and the integration of R&D into its overall strategy and the 'only way to do it is for top management and R&D management to engage in a constant process of mutual education' (Graham 1986: 230). This means that top management must take the lead in forming the relationship and be interested in *making* the R&D organisation educate them about the issues it faces.

Managing the R&D Function in DuPont

The story of the expansion of the DuPont research function from a handful of research personnel employed in 1902, to over 6000 supported by a budget of over a billion dollars by 1980 (Hounshell and Smith 1988: 9) is also the story of the growth of this one-time 'high-technology' company. Hounshell and Smith's unique[20] history of 80 years of R&D management in the DuPont chemical company allows us to explore the degree to which R&D can be 'directed' as part of a conscious company strategy.

Before the Second World War

The practice of DuPont research before the Second World War and in a period of high growth for the company was anything but a focused search for original breakthroughs. By 1911 DuPont's success and near monopoly in its original explosives business had generated the threat of antitrust action, and at the same time the US military were expanding their own explosives manufacturing interests. DuPont's response was to adopt an aggressive policy of 'research-led diversification' by its 'Development Department'. Research was used to assess companies as potential acquisitions and if a company was acquired, research was organised to improve the operations of these businesses. If there was little that was spectacular about it, DuPont found that this kind of research could be costed and evaluated and therefore controlled – and that it paid handsome returns.

The company grew so quickly that it encountered problems of administration which led to four restructurings (Hounshell and Smith 1988: 13) but by the end of 1921 it had resolved into its essentially modern structure: a central coordinating office with autonomous operating divisions, with research also organised into decentralised research divisions located in the operating departments and supported by a small central department. DuPont, with General Motors, is famous as a pioneer of this 'modern' organisational form of the multidivisional structure. The historical account leaves us in no doubt that this organisational innovation was an evolved response to the problem of size, which itself resulted from success in the exploitation of research – but research deployed in the improvement of the operations of acquired businesses. One might guess that this organisational structure would come under renewed pressure for change and perhaps break up, if research gradually ceased to yield the high returns that supported that structure.

DuPont was intent on growth and there were two major departures from this research model before the Second World War – neither involved original R&D breakthroughs. In response to the creation of the giant German chemical and dyestuffs company IG Farben, DuPont in 1929 signed an extraordinary technology transfer agreement with Britain's ICI. This involved the free exchange of patents, staff, confidential research reports, secret process knowledge and a geographical division of markets to limit competition between the two companies. The US Justice Department eventually forced the cancellation of the agreement in 1948 (Hounshell and Smith 1988: 204) but it succeeded in greatly extending the research capability and effectiveness of the two companies when compared with the German colossus (Hounshell and Smith 1988: 196).

The second departure was also an attempt at technology transfer, but the much greater ambition was to become a full-range dyestuffs manufacturer by using DuPont research to copy German dyestuffs technology. The sheer chutzpah of this attempt to build an entire research-dependent business from nothing and in the face of the dominant German industry remains impressive. Like the technology transfer agreement with ICI, it was stimulated by unusual circumstances, in this case the British blockade of German dye exports during the First World War and the absence of any substantial US dye manufacturers that DuPont could acquire. There was also a more timeless reason. Such was the success of DuPont's research-led 'acquisition-improvement' strategy that

> prideful DuPont research personnel had convinced executives that the problems would not be insurmountable. For a long time after the dyestuffs venture, executives were more skeptical of the opinions of research men. (Hounshell and Smith 1988: 77)

The results showed the limitations of a strategy of 'research as a tool of technology transfer'. Massive injections of capital were necessary ($40 million before any profit returned; Hounshell and Smith 1988: 96), much of this to organise dyestuff-specific R&D departments. DuPont research could not recreate the production know-how that German companies had built over decades and success required a formal know-how transfer agreement with a British dye company and the illegal seduction (through salary offers) of German chemists to work for DuPont.

The scale of the effort was only possible because of DuPont's wartime super profits and when the company stopped losing money on dyestuffs in 1923, it was because by 1921 it had successfully lobbied Congress to impose a tariff on returning German dye imports (Hounshell and Smith 1988: 95).

It is very important for any judgement of the later research strategy involving 'academic' research that it is understood that DuPont's rise to a polar position in the chemical industry was attained through other means; primarily through a research-driven acquisition and technology transfer strategy. Yet even before the Second World War we see DuPont taking risks with major departures in research strategy and so learning the limitations to the corporate 'uses' of the research function.

'Innovation-led Growth' after the Second World War

Wartime conditions had promoted the diffusion of much of DuPont's proprietary technology and the company understood that competition would therefore increase in existing markets with a return to peace. It was also apparent that acquisition targets were becoming exhausted, but this former route to growth was anyway understood to be closed because of the renewal of antitrust action against the company.

If the 'pillars' of the pre-war growth strategy had fallen away, the company had acquired a model of what it wanted – the stunning commercial success of nylon, 'a paradigmatic invention for DuPont in the post war era' (Hounshell and Smith 1988: 317). The whole point of the post-war reorganisations of the research function was to produce more fundamental breakthroughs 'like nylon', and so recreate DuPont's proprietary technology advantage. How then had nylon been discovered within the older research structure?

The discovery of nylon was a result of a very limited experiment in the use of academic scientists. When DuPont adopted its divisionalised structure most central research department staff were allocated to the divisions and its staff fell from 300 to 21 (Hounshell and Smith 1988: 109). Central research was therefore marginal and might have been abolished (Hounshell and Smith 1988: 120) had not an exceptional research manager, Charles Stine, been appointed who sought and found a means of making central research contribute to the company's growth.[21]

It was Stine that gained reluctant Executive Committee approval for a small programme of fundamental research that *nevertheless related to the industrial divisions*. Stine selected the areas of chemistry he believed offered most prospect for returns and hired 25 of 'the best' academic chemists to run his programmes (Hounshell and Smith 1988: 229). The great breakthroughs of neoprene and nylon would come from the group run by W. H. Carothers working on the 'theory' of polymerisation that Stine had identified as potentially useful to the fibres department (Hounshell and Smith 1988: 135).

When the company needed a new strategy for post-war growth, nylon provided it with a model of the means, as well as the desired ends of the new strategy for achieving 'control' of scientific breakthroughs – an increase in the employment of academic scientists to do 'fundamental research'. When, in the 1950s, evidence began to mount that the company's competitive position was continuing to erode, it launched its 'New Ventures' programme and

> raised the ante one more time and assigned their research divisions to develop products of an unprecedented degree of technical complexity for special high-priced uses. DuPont continued to see its technological capability as its major advantage over the competition. (Hounshell and Smith 1988: 500)

This heroic phase of DuPont's research strategy represented an attempt to escalate the R&D arms race with its rivals to such a degree that they could neither copy the strategy, nor close the proprietary technology gap. What was the result?

The Experience of Maturity

While research costs stayed constant into the 1960s at about three years and $700 000 per project, development and initial commercialisation losses rose from 4% of earnings in 1955 to 25% in 1967 (Hounshell and Smith 1988: 534). An internal analysis listing the source of DuPont's earnings in 1972 found that nylon continued to be DuPont's biggest earner (Hounshell and Smith 1988: 577).

However, this was 'failure' only relative to DuPont's ambitious yardsticks. 'Venture worth' of commercialised products was calculated by charging pre-commercial expenses against profits in the first 10 years, discounted at some set interest rate. Although only three of the products introduced between 1952 and 1967 had positive net worth at an interest rate of 10% (Hounshell and Smith 1988: 533), that does not mean they were not 'successful' and 'useful' products over longer timescales and lower 'imposed' interest rates. So Kevlar, discovered in 1964, took a record-breaking 15 years of technology development before commercialisation and had cost $500 million by 1982 (Hounshell and Smith 1988: 431). Whereas nylon had been cost-justified on the high-value substitution market for silk stockings alone, Kevlar was developed 'without assurance that any single market would sustain the venture' (Hounshell and Smith 1988: 431), prompting *Fortune* magazine to comment that the fibre was a 'miracle in search of a market' (Hounshell and Smith 1988: 431). Rather, it was a miracle that found many markets; as a 'portfolio' of niche markets was developed over time the product became a long-run success in defiance of standard ROI valuations; gloves, bullet-proof vests, brake pads and fibre optic cables are some of the diverse applications today.

Another blow to the 'innovation-led growth' effort was that research-active competitors proved able to follow similar research routes to DuPont, either simultaneously or in imitation, and this destroyed the carefully constructed proprietary position on which returns to high development costs were predicated. High-density or 'linear' polyethylene and polypropylene were discovered as a result of the use of radical new catalysts – but these were discovered almost simultaneously by DuPont and Karl Ziegler, a Max-Planck Institute director in Germany (Hounshell and Smith 1988: 493). Ziegler's patents were registered slightly earlier and he licensed 'generously', so prompting many new manufacturing entrants – even a shipping line seeking to 'diversify' entered the market.

The result was that supply grew even faster than the rapidly growing demand and nobody could take a profit – DuPont had registered a $20 million cumulative loss by 1975 from 15 years of production (Hounshell and Smith 1988: 495). In the case of polypropylene five companies claimed patent priority on the 1954 catalysts and it took 30 years to settle the ensuing patent war, or 'interference' proceedings (Hounshell and Smith 1988: 496). While the patent rights were at issue, it was clearly not in anyone's interest to develop the production technology.

One could continue with examples, but the essential point is that even when DuPont had a good product – and these were very good products – it could not be sure of reaping what it considered an adequate return. These were the problems of approaching technological 'maturity', but there was no signpost to signal when 'maturity' had arrived. That was a matter of judging both that new product opportunities were 'relatively' exhausted and that an equality of technological developmental capability had been achieved between DuPont and its rivals. In other words, expectations of future proprietary control and financial returns to R&D would be lowered. Together with other development experiences at this time, Kevlar, high-density polyethylene and polypropylene provided cumulative blows that gradually exhausted the DuPont Executive Committee's commitment to the 'innovation-led growth' strategy.

Increased Management Control as a Response to Relative Decline

By 1969 the Executive Committee 'considered the entire effort a failure' (Hounshell and Smith 1988: 504). Only now and *as a response* to the decline in returns did the Executive Committee abandon its reactive role of vetting project suggestions filtering up from the R&D departments, and take responsibility for the direction of research into new fields.[22] Fundamental research was cut everywhere and absolute R&D levels held constant so that expenditure per dollar of sales fell from 6.6% to 3.3% by the end of the 1970s (Hounshell and Smith 1988: 573).

This was no comment on the *quality* of the fundamental research – DuPont scientists would eventually win Nobel Prizes for some of the work performed at the central laboratory and by the 1960s this laboratory had become

> one of the outstanding industrial basic science laboratories in the US … once the drift had begun it proved nearly impossible to contain or control. DuPont's top management tolerated and even encouraged this type of research because, as one research administrator put it, they had an almost 'mystical belief' that a 'new nylon' would be discovered. (Hounshell and Smith 1988: 376)

The historical sequence of events is significant – it would be an easy mistake to think that the existence of a formal control system implied an *ability* to control, in the important sense of being able to preselect the winning over the losing projects for development. Budgetary constraints reduced the finance available to play the research game, but they did not mean that project selection could be managed better. The significant phrase in the above quote on fundamental research is 'nearly impossible to contain or control'. Reduction of this kind of research was managed by requiring the central laboratory to move to be 50% funded by the industrial departments, as it had been in the early 1950s, rather than the 10% typical of the early 1970s (Hounshell and Smith 1988: 583). The problem of how to deselect fundamental research projects was therefore devolved to the laboratory itself.

Despite the restrictions on absolute R&D spending, DuPont remained the chemical industry leader because other chemical firms also reduced their R&D growth. In other words, the industry managed a collective 'de-escalation' of the competitive R&D arms race in response to the growing evidence of changing returns on R&D expenditure.

The story of DuPont provides a strong warning not to take measures of current R&D activity in any company or industry as evidence of some calculated, knowable and optimum level. The period of escalated expenditure represented a strategic choice based on an understanding of the post-war competitive context and a 'hope' that the example of nylon could be reproduced. Although this case is certainly exceptional because DuPont was an industry leader, a very rich company and had an unusual degree of discretion to raise R&D expenditure, the general warning is that *actual* levels of R&D expenditure are likely to contain a strategic, variable component related to future expectations of returns – and those future expectations will be based on an understanding of the past and current company and industry circumstances in a non-predictable manner.

If Hounshell and Smith's work finishes in the late 1980s with the advent of maturity in the established businesses and the beginning of an effort to move into the biological sciences, the newspapers provide some clues to later developments. Most relevant to an understanding of the evolution of maturity was DuPont's announcement in 2002 of the divestment of its fibre and textile division. It seems reasonable to take this date as symbolic of the end of the struggle to maintain innovative gains and innovation-derived profits for this former jewel within the corporation – a struggle that can be assumed to have continued for much of the preceding decade and a half (BASF FAZ Institut 2002).

Rise of the Organisational Innovation of the R&D Department

Comparisons between DuPont and Research-for-invention in the German Dye Firms

Elements of DuPont's experience of the returns to its R&D organisation and expenditure can be couched in terms of the relationship between science and technology. The discovery and development of nylon did derive from academic science, but this polymer science had been selected and brought inside DuPont for its promise in terms of useful new technology. Other science projects so selected did not deliver. DuPont's experiment in the support of academic science can be seen as the attempt to systematise the relationship between the development of scientific understanding and technological development opportunities in this industrial field. Its end can be seen as a measure of the erratic and unsystematic nature of the relationship. The management problem should not be seen as wholly one of the internal organisation of research-for-invention, but as part of the larger problem of how to relate industrial research activities to the publicly funded and separate institutions of the world of science. DuPont was only able to make the expensive incursion into academic science because of its leading position in the technology. Had DuPont not experimented with polymer science, nylon would surely have been invented one day by researchers elsewhere, perhaps in the universities.

Historical studies of the pioneering R&D departments of the German dye manufacturing firms in the second half of the nineteenth century confirm the essential pattern of development of industrial research shown by the example of DuPont. A brief comparison shows that first, like DuPont, firms such as Bayer only entered the uncertain field of research when they had already achieved a degree of market control and were financially secure (Meyer-Thurow 1982). Second, research-for-development, not research-for-invention, was an early priority in these firms as it was in DuPont. Chemists were first employed as either works chemists, to supervise production departments, or 'laboratory' chemists: the latter were most often lowly chemical analysts performing tests on materials entering and leaving stages of the production process (Homburg 1992). Once the research institutions had matured in form after 1890, 'a maximum of 20% of all employed chemists were working in the centralized research institution [IG Farben's main laboratories] with the other 80% in other laboratories, technical departments, or in the large sales agencies' (Marsch 1994: 30). This distribution of research activity persisted when these firms merged to form IG Farben, which by 1926–7 had 1000 research scientists working in 50 laboratories including 10 main laboratories inherited from the merging firms (Marsch 1994).

A difference between the German firms and DuPont lies in the nature of the first invention search tasks that these firms encountered. As described above, DuPont allowed a limited experiment with the import of selected academic research work into a central laboratory: the enormous commercial returns on the resultant discoveries of nylon and neoprene encouraged the company to expand academic research for invention. In contrast, the German dye firms needed to conduct a systematic search for the few commercially valuable azo-dye compounds amongst the hundred million possible products of the diazo coupling reaction (Meyer-Thurow 1982: 378). It was the nature of the work inherent in this search task that first brought forth the response of the centralised department for research-for-invention. Once the range of research organisations had been established and had proved their economic worth, the German dye companies extended inventive research into what appeared to be promising new areas such as pharmaceuticals, nitrogen, fertilisers and synthetic fibres. IG Farben never embarked on anything like the academic science experiment of DuPont in the 1950s: the search work of IG Farben's main laboratories remained orientated around their separate product areas. They were also physically located near the technical development laboratories and production sites to which they related (Marsch 1994: 56). However, according to an IG Farben director, between 5% and 10% of main laboratory research was what we might call 'basic' or 'fundamental' research unrelated to products and this element of their work is closer in content to DuPont's academic science experiment (Marsch 1994: 43).

So in both DuPont and IG Farben research-for-development was closely linked both physically and organisationally with production. In both, research-for-invention was organisationally separated and insulated from the immediate demands of production, but to different degrees: IG Farben used both product–area constraints and a greater degree of decentralisation as a means of controlling research-for-invention. It would be unwise to attempt to make a strong judgement of the relative success of the different strategies of control of research-for-invention given the great differences between the firms – IG Farben was the gigantic, established firm that engaged in all forms of chemical research many decades before DuPont. All that can be said here is that difficult as it is to manage this form of research to yield useful discoveries, there are nevertheless disciplines that can be imposed upon it.

If we have a tendency to privilege the importance of research-for-invention over what appear to be the more mundane production technology development activities, it is probably because we know of such spectacular examples of invention as the laser and penicillin and because we understand that in the dye business, the development activities could not have continued for long without success in invention. In response it should be said, first, that in contrast to the idea of creative heroism attached to the penicillin and laser inventions, the exploitation of the diazo coupling reaction was tedious 'scientific mass work' that demanded no creative ability from the scientist and that has its modern parallel in the tedious gene sequencing work that was necessary to complete a scientific description of the human genome. Second, that without organised development capability these firms would have been unable to exploit the commercial value of their inventions. Marsch makes the pertinent comment on IG Farben that 'No clear borderline was drawn between research and development, neither by the management nor by the scientists themselves. Development was seen as part of the scientific process' (Marsch 1994: 46).

A similar argument applies to the precedence relationship between such broad and overlapping terms as 'science' and 'technology': one should be wary of claims of any specific and general relationship between them. In particular, one should be wary about privileging the status, or granting *general* precedence, of one over the other in the innovation process. To return to another problem with our 'great invention' stories such as that of the laser, penicillin or nylon, it is perhaps because the inventors were scientists that the stories can be taken to suggest the general priority of science over technology, at least for invention.[23] 'Science' as a search for understanding is less likely to be privileged over technology in these stories if proper attention is paid to the 'technologies' of science itself, to the role of scientific instruments and experimental technique: the scientist is also a kind of technologist, most especially when engaged in invention. The priority relationship between science and technology may also be perceived to operate in reverse; working technologies have often inspired scientific research for the sake of understanding. For example, it was not understood why laser light was as coherent as it proved to be, and 'coherence theory' was developed years after working lasers had been established (Bromberg 1991).

A scientific field such as metallurgy, now 'materials science', grew out of the study of the properties of alloys and metals that were discovered through trial and error in practice (Mowery and Rosenberg 1989: 32). So industrial research conducted with the object of gaining understanding can generate significant practical opportunities and practical development can generate significant new scientific opportunities. However, as Rosenberg has pointed out, the USA's National Science Foundation (NSF) collects industrial research data under the categories 'basic' and 'applied', with 'basic' research defined as research conducted without commercial objectives and 'applied' research defined as research intended to meet a 'recognised and specific need' (Rosenberg, 1990: 170).

Given what we know about the variable and unplanned outcomes of research with these original management motives, Rosenberg finds their use as a means of defining distinct 'types' of research to 'less than useful' (Rosenberg, 1990: 171).

The Economy-wide Rise of In-house Industrial R&D

The history of DuPont's R&D facilities is an important example of a general trend that begins at the end of the nineteenth century for manufacturing companies in developed countries to found R&D departments. They spread rapidly through US industry in the early twentieth century and typified the high-growth industries of chemicals, electrical and aircraft manufacturing, later of electronics and pharmaceuticals (Edgerton 1996: 34). However, by absolute volume of expenditure, R&D was always more highly concentrated than production, being dominated by a few large leading firms in each sector (Edgerton 1996: 34). Most, perhaps two-thirds of the 'work' of these departments, was development (Mowery and Rosenberg 1989: 57).

From the beginning, industrial research was not only organised in-house, but also provided by free-standing laboratories on a contractual basis. However, in the USA the percentage of scientific professionals employed 'out-of-house' in this way fell from 15% in 1921 to 7% in 1946 (Mowery and Rosenberg 1989: 83). Mowery and Rosenberg reviewed the relationship of the two types of research organisation in the USA in the pre-war period and concluded that:

> Rather than functioning as substitute, the independent and in-house research laboratories were complements during this period and performed different research tasks. … The foundation of an in-house laboratory resulted in … a substantial expansion in the range of research possibilities and projects open to the firm. The growth of industrial research within US manufacturing reflected the shortcomings of market institutions as mechanisms for the conduct and distribution of research and development. (Mowery and Rosenberg 1989: 91)

As DuPont found from experience, contract R&D was even more difficult to control than in-house R&D. Independent laboratory owners could not always be trusted in their evaluations of research prospects – they sought to benefit themselves. In contrast, the in-house organisation of R&D activity encourages the generation and solution of problems likely to be useful to the firm. It aids the coordination of the project development that will necessarily involve most of the other functions of the firm. And the secure employment conditions that in-house organisation makes possible help secure the loyalty of the scientific professionals against temptations to defect and transfer their ideas to rivals, or to become independent developers themselves (Mowery and Rosenberg 1989: 108). For all these reasons in-house organisation of R&D remains an important way of organising innovative activity today.

A recent study of trends in the organisational location of R&D has been framed as a response to the occasional claims in the 1990s that R&D would show a tendency to become more decentralised in the 'post-bureaucratic firm'[24] (Hill et al. 2000). The authors were unable to find a clear trend towards decentralisation of R&D within their sample of mechanical engineering and food and drink firms. Instead there was a bewildering pattern of change: some firms had historically had centralised control over R&D, others decentralised control to operating divisions; some firms were busy decentralising R&D, others were centralising control over R&D. Two observations from the historical studies are relevant here. First, one is reminded of the many changes in emphasis between central and decentralised R&D organisation in DuPont as the company's research strategy evolved; second and related to the first, that in general a shift in research strategy would imply some shift in emphasis between centralised and decentralised R&D organisation because R&D consists of many types of research and some types, such as research for development, are suited to a decentralised organisational location. The degree of internal centralisation of R&D is probably not so significant in itself, but something that changes through time with varying emphasis on particular R&D projects. These authors preferred quite rightly to stress that where R&D was thought to be of strategic importance to the firm the management issue was not centralisation versus decentralisation in itself: it was how to achieve good integration between R&D and other functions – something that remains difficult to achieve (see Chapter 7).

The Interpretation of Aggregate R&D Statistics

Certain forms of aggregate data on R&D expenditure receive a high public profile through their compilation and diffusion by government. In Britain there is an annual 'R&D Scoreboard' compiled from the R&D expenditure reported in public companies' annual reports (DTI 'Future and Innovation Unit' and Company Reporting Ltd 2001).

These claim to support several generalisations. So 'sustained high R&D intensity' (R&D per unit of sales revenue) is positively correlated with company performance (measured for example by sales growth or productivity) (DTI 'Future and Innovation Unit' and Company Reporting Ltd 2001: 12). Much of this correlation can be associated with which business sector a firm occupies and the consequently varying innovation opportunities (the highest R&D intensity sectors are pharmaceuticals, health, IT hardware, software and IT services, and aerospace (DTI 'Future and Innovation Unit' and Company Reporting Ltd 2001: 4)).

Some stark international differences are also apparent, although their significance is less so. For example, the scoreboard analysis states that the 'UK pharmaceuticals sector is above best world levels in intensity but the UK average R&D intensity, excluding pharmaceuticals and oils, is significantly less than 50% of the US, Japanese or international averages' (DTI 'Future and Innovation Unit' and Company Reporting Ltd 2001: 3). This does raise the question of why the differences exist and the temptation for the scoreboard authors to offer policy advice proves irresistible:

> It is crucial for companies to benchmark their R&D against best international practice in their sector and to understand the ways in which their R&D investment will affect future business performance. (DTI 'Future and Innovation Unit' and Company Reporting Ltd 2001: 1)

At one level this appears unexceptionable – who could disagree with a maxim that urges greater awareness of best practice? Yet it is also an interpretation of the data that suggests managerial ignorance as the problem; if only low R&D intensity British firms realised what others were doing, and if only they understood the benefits of greater R&D investment, they would spend more. It is implicit in the above comment that more spending on R&D is a 'good thing'.

But perhaps low R&D intensity firms are already aware of their best practice competitors and are seeking to close an economic performance gap, or at least to maintain their trailing position, through technology transfer strategies that require less intensive R&D expenditure – rather as DuPont's rivals were forced to do for many decades. Or perhaps the national institutional environments are very different and help or hinder in different ways in different countries. For example, if one were to begin to investigate the British relative success in pharmaceuticals, it would surely be relevant to consider the effect on private activity of, first, Britain's possession of the Wellcome Trust, the 'world's largest medical research charity'[25] and second, the British higher education system's tendency to excel in the production of pure scientists.

The point here is that such R&D statistics reveal gross patterns, but in themselves cannot provide explanations. The imported explanation lurking behind the quote above is the assumption that 'more industrial R&D would produce better performance', but it is also possible that high R&D intensity is the result of past performance success.

The historian David Edgerton has produced an invaluable historical review of the various forms of the argument that insufficient science and technology explain the relative British industrial decline from the late nineteenth century to the present (Edgerton and Horrocks 1994; Edgerton 1996). His work forms a more secure basis for drawing conclusions about the meaning of relative international expenditures on R&D.

At the national level there appears to be no correlation between rates of civil R&D expenditure and rates of economic growth. For example, in 1963, Japan spent 1% of GNP on R&D for 8.3% economic growth, Britain 1.2% of GNP on R&D for 2.5% growth, Germany 0.9% GNP on R&D for 4.1% growth (Edgerton 1996: 57). There is, however, a strong correlation between GDP per capita and rates of civil R&D expenditure and rates of patenting.[26] So the pattern is that high rates of economic growth with low R&D intensity are associated with relatively poor countries (in terms of GDP per capita) that are rapidly catching up with the relatively rich. The classic recent example is Japan, whose post-war history is one of organised technology transfer into the country and associated high rates of economic growth, slowing as the output per capita gap with the leading rich countries closed. Japanese R&D expenditure accelerated as the output gap disappeared and as the country entered a decade of relative economic stagnation in the 1990s. High national civil R&D expenditure is therefore associated with being rich, the exhaustion of major technology transfer possibilities from other countries, and therefore the need for a more vigorous search for genuinely novel development opportunities through R&D.

Britain provides an apparent exception to this pattern. Post-war[27] private British industrial spending on absolute amounts of industrial R&D was second only to the USA, even if British government civil R&D is excluded to remove the effect of government-funded prestige civil R&D projects such as Concorde (Edgerton 1993: 41). British industrial R&D stagnated from the mid-1960s and research intensity actually fell in the 1970s, so that despite some recovery in the 1980s, research intensity at the end of that decade had only returned to the level of 30 years earlier:

> There has been a catastrophic relative decline in British industrial R&D over the last twenty or so years. (Edgerton 1993: 42)

This relative decline occurred as German, Japanese and later French and Italian industry surpassed British industry levels of output per head with lower levels of R&D spending. The post-war higher British national industrial R&D spending did not translate into higher economic growth and Edgerton suggests that

> it could be argued that in the 1970s British industry scaled down the R&D input to a level where it could be translated into new products and processes, given levels of investment and profitability. (Edgerton 1993: 49)

Industrial R&D is an expensive overhead and 'more' is not always 'better'. It is only one input into economic growth, and one that is relatively more important with increasing levels of relative economic wealth. The British example is consistent with this analysis.

Our reaction should neither be that Britain has now 'insufficient national R&D spending' nor alarm at the relative decline in British R&D expenditure. Such a decline is to be expected in a country that no longer leads the tables of output per capita. It would be more pertinent to examine how British industries have adapted better to manage technology and its transfer from best practice overseas companies.

Concluding Comments

The main topic of this chapter was the way technology may be conceived and changed in relation to prospective uses and markets. When innovation is the issue, it makes no sense to divorce the market concept from technology development. It is rather how the market concept is articulated and related to technology development that matters. With this in mind it was argued that the distinction between a reference market and an innovation market concept has some utility in modelling the creative relationship between existing technologies and markets and prospective technologies and their uses. The creative or inventive step obviously varies in its significance case by case, but even in the examples of the laser and penicillin, characterised by a breakthrough step, there were many other, subsequent creative contributions to development.

The popular technology-push versus market-pull metaphor is downright misleading as a means of understanding this process and if a shorthand term must be used, invention and innovation are better understood as socio-cognitive processes.

RCA as an exemplar of poor innovation leadership practice showed that as a significant organisational and economic venture, development needs senior managers committed to understand and interrogate the choices of the R&D function on behalf of the firm's long-term interests.

The experience of DuPont was that one could not scale up organised academic-oriented R&D and expect a proportional return in breakthrough innovations. In other words, companies cannot expect to 'manage' major opportunities into existence, although they can excel at exploiting the ones they find.

At a macro level, the interpretation of aggregate, national, civil R&D expenditure is that it rises with wealth measured relative to other countries and as a means of further increasing that wealth. Technology transfer from leading countries is a more important issue for poorer countries that wish to increase their wealth to match the richer countries.

In the next chapter other macro-level patterns in technology development come under scrutiny.

Notes

[1] I have taken Bromberg's history of the laser and the inventor Charles Townes' own account as my two major sources for the laser sections (Bromberg 1991; Townes 1999).

[2] Stimulated emission occurs when radiation of some wavelength is incident upon an atom, molecule or electron in an excited energy level of the same energy as the incident radiation. The atom, molecule or electron is 'stimulated' to drop into a lower energy state and at once emit a radiation wave packet (photon) of the same energy (and wavelength) as the incoming photon. So there would be amplification of the incoming radiation. This could not normally be expected to be useful because in a material in *thermal equilibrium*, there would be a characteristic distribution of energy states, ranging from low (absorptive) to high (capable of stimulated emission). Incoming radiation was as likely to be absorbed as to stimulate emission and what radiation was produced by stimulated emission was also likely to be reabsorbed by the many low-energy levels.

[3] Abnormal physical circumstances here mean that the population state of the energy levels of matter were not in 'thermal equilibrium' and instead an unstable 'inverted population' of these energy levels existed.

[4] A full description of stimulated emission, thermal equilibrium, population inversion and energy levels can be found in physics undergraduate texts. There is of course more to the 'laser effect' than in this simple description; Hecht and Zajac write of stimulated emission that 'A remarkable feature of this process is that the emitted photon is in phase with, has the polarization of, and

propagates in the same direction as, the stimulating wave' (Hecht and Zajac 1974: 481). In other words, stimulated emission radiation is 'coherent'. Bromberg comments that it was not understood *why* laser light was so many orders of magnitude more coherent than other forms of light at the time of the laser's invention, and that 'Coherence theory, even when it became elucidated in the course of the 1960s, held low priority for many laser workers. It was not needed to make lasers work, and it was hard to master' (Bromberg 1991: 109).

[5] This account is of course much reduced – see Townes on the steps and circumstance of the invention of the maser, especially pages 55–68.

[6] The maser was the basis for sensitive receivers that for the first time allowed ground to geosynchronous orbit communication – they therefore helped to make working satellites possible (Bromberg 1991: 56).

[7] Whereas it is difficult to find an American or British person who has not heard some version of the penicillin story, it is difficult to find a German or Dane who has heard the story – at least, if my students are any guide.

[8] Many accounts of the discovery of penicillin have been written, but two stand out for their painstaking method of reconstructing the inventive process. Ronald Hare, once a colleague of Fleming, reproduced the physical conditions that would generate the famous mouldy plate and Gwyn MacFarlane reconstructed Fleming's thought primarily through the available written evidence.

[9] MacFarlane finds that the penicillin concentrate that Fleming's two assistants prepared was certainly strong and stable enough to allow Fleming to perform the animal protection experiments with success had he been motivated to try them (MacFarlane 1984: 175). This matters because after the successful development of penicillin by Florey's team, Fleming 'took it upon himself to complicate the matter very badly . . . he used such expressions as "even skilled chemists had failed in their attempts to concentrate, stabilise and purify penicillin (Hare 1970: 102). This was not the case."' Fleming had two assistants, Ridley and Craddock, who worked on concentrating the 'mould juice' and MacFarlane comments that their methods were essentially the same as those independently developed by the Oxford group and two other independent researchers on penicillin. All these groups worked with Fleming's mould and so the difference between them was whether they could conceive of the animal protection experiment and then be motivated to perform it.

[10] Eleven years after Fleming's discovery, Florey's team should have had the advantage over Fleming that by then the sulphanomides, the first great group of antibiotics, had been discovered by Domagk working for Bayer in 1935 (Hare 1970: 148). The discovery had been made because Domagk had used live animal experiments and he reported the then-surprising finding that the dye Prontosil was lethal to bacteria in the body, *but harmless to them in culture*. Although the precise mode of action would be worked out later, it was now clear that one could *not* extrapolate from antiseptics and experiments outside the body to draw conclusions about the antibiotic properties of drugs. However, Clark's investigation of Florey and Chain's motivation for beginning their 1939 investigation into penicillin suggests Florey saw it as a way of attracting long-term private sector funding (Clark 1985: 38) and Chain was motivated by an interesting biochemical problem – not by its prospective use as a drug.

[11] See Howells (1994) for a full analysis.

[12] The source is Howells (1994) which is based on anonymised interviews with managers of bioprotein projects.

[13] Raghu Garud and myself seem to have independently coined the term for similar reasons in the field of management studies. Some years ago I searched for the term in the Proquest database and found six apparently independent uses of the term in different subjects.

[14] A full quarter of this book is dedicated to a thick description of the *Helicobacter* story, organised by the categories of discovery, acceptance, experiments and instruments and social interactions.

[15] A colleague uses barbed wire as a particularly clear demonstration of the concept of 'market pull'.

[16] I have relied upon Hayter and Basalla's derivative account for the detail of the barbed wire case.

[17] Although even here Texas Instruments' main product is not stated, again as if the market orientation floats free of the technological context.

[18] The book itself was a major project taking almost 10 years. There were more than 40 interviews, access was gained to all relevant company documents and Graham's interpretation is based on a factual narrative agreed with the RCA participants (Graham 1986: xiii).

[19] The single source for this account is Graham's work (Graham 1986).

[20] The editors write in their preface that 'heretofore no historian has written a full length, intensive study of the origins and evolution of the R&D establishment in one of the nation's leading, high-technology firms' (Hounshell and Smith 1988).

[21] Hounshell and Smith comment on the 'decision' of whether to site research in a central or devolved department that 'These issues had changed little between 1904 and 1921. They have not changed much since then either' (Hounshell and Smith 1988: 595). The tension that generated many changes was that between a need to make research relevant to current problems by siting it in the divisions and the difficulty of conducting any research that departed from divisional imperatives in the divisions. As overall strategy emphasised the one or the other so the role of the central department waxed and waned. It is difficult to beat the company's experience that the role of central research must be continually revised with the understanding of the state of technological potential and strategic position.

[22] The first major act was to begin a coordinated move into the life sciences in the 1980s.

[23] In saying this I am expressing a belief that 'other people' might believe in the infamous 'linear model' of innovation, a key component of which is that basic research precedes applied research and development and innovation. In a recent article Edgerton argues that the linear model's principal function is as an academic 'straw man' with which innovation analysts can disagree before setting forth their own ideas on innovation – his exhaustive search for those who purportedly believe in this model turns up no clear believer with a detailed catalogue of their belief (Edgerton 2004).

[24] The Hill et al. research takes particular issue with Whittington's (1990) thesis that R&D is moving from centralisation to fragmentation (Whittington 1990).

[25] The description is from the trust's website www.wellcome.ac.uk/.

[26] Result from Faberberg (1987) cited in Edgerton (1996: 57).

[27] British private sector R&D in the interwar period was also very healthy – however, in this period firms were so secretive about the amounts spent that to many contemporaries it appeared that the comparatively small amounts spent by government were more significant. In Sanderson's paper presenting figures for private R&D expenditure in this period he describes the consequent 'distorted picture among historians of the interwar years that exaggerates the role of bodies whose contribution to industrial research, though honourable, was quite marginal to the total activity and which virtually ignores, belittles or even slanders the vast bulk of industrial research which was carried out privately within the firm' (Sanderson 1972). The prize-winning *Economic History Review* article by Edgerton and Horrocks is able to revise upward the amount known to have been spent by interwar British firms on in-house R&D – the important kind (Edgerton and Horrocks 1994).

This situation has important consequences because those who have taken interwar historians' beliefs about British R&D at face value have tended to assume that interwar British R&D expenditure was in some way inadequate and therefore an explanation of British relative economic decline. Sanderson's article has not stopped authors such as Barnett, Mowery and Chandler from making this argument (Edgerton and Horrocks 1994: 215). Edgerton and Horrocks' review of the evidence and its deficiencies makes a convincing rebuttal of the argument of R&D insufficiency pre-1945.