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Invention

Since the Second World War, in the Anglophone world, technology has come to be closely identified with invention. This conflation has been unhelpful to the understanding of technology and has also had negative effects on our understanding of invention. We do not have a history of invention, but instead histories of the invention of only *some* of the technologies which were later *successful*. That in itself biases our understanding. But the history of inventions we have is itself innovation-centric. It focuses on (some) aspects of what is new in invention, and it highlights changes in invention, not what does not change.

The innovation-centric picture comes in a number of different versions. One focuses on inventions in academic scientific research; another on what are taken to be the crucial technologies; yet another looks at what are taken to be the most novel inventing organisations. Very often an overall argument is made that as time has passed novelty has itself become ever more novel. Each of these images, while it has some points in its favour, deserves to be challenged. One of the most important and interesting things about invention is that it exhibits important continuities which are insufficiently recognised, and indeed that it has changed in ways we do not sufficiently appreciate. Prolific invention has been with us for a long time – novelty is not new, but there are new things to say about it.

Academic science and invention

The academic research picture focuses on what it takes to be the most important and innovative aspects of science, and claims that crucial inventions which then shape our world derive from them. Implicit in this view is the argument that something called 'science' has become, since the late nineteenth century, the main source of technologies. What is meant by 'science' is something very particular. Just as technology and invention are conflated, so are science and research. The twentieth-century belief that 'Science implies the breaking of new ground' has made science research.[1] But just as most engineers are not inventors, and most scientists are not researchers, so most science is not research.

Even the research that is referred to when 'science' is used, is usually only a small part of all scientific research – that done in universities or similar bodies. There is a very particular innovation-centred view of academic research, which privileges organic chemistry and electricity in the nineteenth century, nuclear physics in the first half of the twentieth century, and molecular biology since the 1950s. From these particular academic researches, come, implicitly and explicitly, world-changing technologies – synthetic chemistry, electricity, the atomic bomb and biotechnology. The list will by now be familiar. Indeed, our standard picture of what is important in the history of invention in academic research has been profoundly affected by what are taken to be the most important technologies of the century.

Only a tiny proportion of twentieth-century academic research was in particle physics and molecular biology. These branches of physics and biology did not even dominate those fields, let alone academic research as a whole. One of the most striking omissions is chemistry, the largest academic science for most of the century; others are academic engineering and medicine. In these sectors too, the constant generation of novelty became the rule in rapidly expanding universities. Most novel university research has been in what are wrongly taken to be 'old' subjects.

In any case new subjects of research in universities derived from older practices. The university was keeping up with a changing technological world rather than creating it: there was flight before there was aeronautical engineering; there was photography long before any theory of the photographic process; there was any amount of highly specialised metal manufacture before metallurgy; and solid-state devices existed before solid-state physics. Industrial firms, not universities, pioneered the scientific study of photography, metallurgy and the semi-conductor; the academy followed.

The relations between the world of practice and invention in the academy have long been close. For all the talk of ivory towers, academic science, engineering and medicine have been closely connected to industry, as well as the state, since at least the late nineteenth century. The great German organic chemistry centres in the universities had close links with German industry before and after the Great War. Fritz Haber, of the Haber-Bosch process, was an academic. Academic experts on coal and in chemistry were involved in coal hydrogenation. The University of Goettingen was an important centre of aeronautical research before the Great War. Penicillin was spun-off from St Mary's Medical School and the University of Oxford in the 1940s. MIT set up a spin-off arm before the Second World War. Stanford was also spinning out in the 1930s – its Klystron microwave generator became the first great product of what much later would be Silicon Valley.

Yet historically-ignorant analysts insist that only in the last two decades have the barriers between the academy and industry been broken down with the creation of great entrepreneurial universities. These are only now, it is claimed, driving the creation of new industries. Not only is the novelty of this greatly exaggerated, so is its significance. At the beginning of the twenty-first century US universities and hospitals were receiving around $1 billion worth of licence (largely royalty) income from their intellectual property per annum. That is a huge sum, but needs to be kept in proportion. The largest recipients got no more than tens of millions of dollars, with most of the money coming from a very few patents in the medical field, a notable case being Florida State University's patent related to the cancer drug Taxol.

It was far from self-financing. Most university patents were the result of huge *public* investments in academic research. The Bayh–Dole Act of 1980 was critical in that it gave universities intellectual property rights on the results of federally funded research. The universities and hospitals were spending some $30–40bn on research per annum, some $20–25bn funded by the federal government, with the balance coming from industry, local and state governments and the institutions themselves. The big story in US academic research continues to be what it has been since the Second World War: federal research funds, military and civil. For all the emphasis on private health care in the USA, the federal government has played a massive role in funding academic medical research, one which has increased very significantly in the past decade.

Academics have wanted funding to be provided by government, and to be independent of funding directly concerned with invention and development, which was largely a matter for industry.[2] That there is a particularly widespread belief in the significance of academic science as a source of invention is testimony to the great influence of academic research scientists. There are indeed cases where academic research has led to new technologies. Many examples are given, but not all are convincing. Good ones would be X-rays and atomic weapons; poor ones, the cavity magnetron and the laser. The cavity magnetron, which generates high-power high-frequency radio waves, was used before academics studied it. The laser was the product of academic research guided and stimulated by the US military.

The great bulk of invention – let alone the development of inventions – takes place, and always has done – a long way from university research laboratories, and no serious analyst of invention ever believed otherwise. Most invention has taken place in the world of use (including many radical inventions) and furthermore has been under the direct control of users. It has been the realm of the individual inventor, the laboratories, workshops and design centres of industrial firms, and the laboratories, workshops and design centres of governments, and especially their armed forces.[3]

Stage models of invention

One important myth is that invention is highly concentrated in particular areas where the most radical inventions happen. These are taken to be the technologies which are thought to shape particular historical eras. In the case of industrial technology, invention is thought to be concentrated in electricals and chemicals in the first half of the century, giving way to electronics and rockets, and then to computers and biotechnology. In recent years one could be forgiven for believing that there was no invention going on outside information and biotechnology. There is evidence of shifts in inventive effort between areas over time, and to a lesser extent of changes in inventive output over time, but it does not correspond to the stages suggested. Inventive effort in electricity and chemicals not only persisted, but radically expanded in the twentieth century. So did invention in mechanical engineering. However, the proportion devoted to rockets and electronics undoubtedly grew in the 1950s and 1960s and it undoubtedly shrunk in the last decades of the century. It is the case that within industry life-sciences research, whether in pharmaceuticals or agriculture, has increased while heavy-chemicals research has fallen. That has happened even within particular firms.

Perhaps the most powerful proof of the importance of the old is that the largest private spenders on research and development at the end of the twentieth century were not computer giants, or even pharmaceutical firms, but motor-car producers – General Motors and the Ford Motor Company top the list, not Microsoft or Novartis (see Table 8.1, p. 204). The cost of design of a new car at the end of the twentieth century was around £100–500 million and about the same for a new car engine. It is in the same range as a new drug. Of course, it may be that research and development in these areas is expensive because to produce anything worthwhile, one needs to put a lot in. In other areas the returns may be much larger, and technical change much swifter. Micro-electronics may be the key case.

There is no doubt that there has been a belief that technical progress has concentrated in particular areas, but it is hard to untangle whether

24. John Garand, employee of the US Federal Armoury at Springfield, and inventor of the US Army's semi-automatic rifle, the M-1, at work in his model shop. The M-1 was the standard US infantry rifle of the Second World War. Mechanical inventions by employees of corporations and governments, and by private individuals, remain a significant proportion of all patents in the twenty-first century.

this is because a lot of effort is devoted to it, or because it is productive. There is an old Soviet joke which goes to the heart of the issue: an inventor goes to the ministry and says: 'I have invented a new buttonholing machine for our clothing industry.' 'Comrade,' says the minister, 'we have no use for your machine: don't you realise this is the age of the Sputnik?'[4] Such sentiments shaped policy, not only in rockets, and not only in the Soviet Union. Planners hope to focus invention and development on what they take to be the 'cutting edge', or some other similar cliché, of technological advance. Much more has been invested by governments in invention in aviation than in shipping, or in nuclear power than other energy technologies. Of course military imperatives to build rockets and nuclear stations, subsequently justified by claims about their general technological fecundity, for example in the notion of spin-off, were important. Yet behind the spin-off argument was a key hidden assumption that spin-off happens only in what are considered advanced technologies. We believe that spin-off from rockets is more likely and more significant than from button-holing machines.

So powerful is the idea that important invention is confined to new technologies, that a special concept was used to explain innovation in old industries. It was the 'sailing ship effect'. This is the argument that firms in old industries innovated only in response to new technology that threatened their survival. The examples given, all nineteenth-century ones, are: the sailing ship improving after the introduction of steam; the development of the Welsbach mantle for gas lights, which followed the introduction of electricity; and improvements in the Leblanc process for making alkali, following the introduction of the Solvay process. However, in all these cases there is no evidence that invention was not happening anyway in the 'old' industry.[5] In some instances there may indeed be a sailing ship effect. The speed up in invention in condoms and other forms of contraception after the introduction of the Pill is a case. But it may be explained by the special circumstances of the industry.

Invention and innovation have been happening everywhere. Agriculture has been an important site of invention and development activity, with the devising of new agricultural practices as well as many new plant varieties, such as IR8, the new dwarf rice introduced in 1966 by the International Rice Research Institute in the Philippines. Intensive development led to new animal hybrids (for example, in the case of chickens) and husbandry practices such as the use of growth-promoting antibiotics. The declining British cotton industry and government supported research and development in the growing of cotton, and the manufacture of cotton goods, on a large and increasing scale from the 1920s. In the early decades of the twentieth century the largest single corporate research project in the USA may well have been the American Tobacco Company's development of a cigar-making machine.[6] Armed forces paid for research and invention in small arms and artillery, as well as in aviation and radio. Inventive activity in shipping has not only led to much larger ships but to such now widespread twentieth-century things as the bulbous bow. Even though it was very unfashionable and badly under-resourced, work was done for decades after the Second World War on improving the performance of steam locomotives.

By the 1960s it was felt by some that whole areas of technology were not receiving the inventive attention they deserved. At its most basic there was an argument that too much was spent on aircraft, rockets and nuclear power, often labelled 'prestige' projects. More should be spent, it was argued, on bread and butter research and development, on improving electronics, and chemicals, even trains and buses. A particularly strong and interesting version of the argument came from the economist E. F. Schumacher. He argued for the development of 'intermediate technologies' which would stand between the traditional technologies of the poor world, and the capital-intensive large-scale ones of the rich world, an idea developed in a famous book called *Small is Beautiful* (1973). These ideas were very influential, leading to the development, on a small scale and funded by charities, of a very wide range of new and improved things. For example, an academic engineer at the University of Oxford, Stuart Wilson (1923–2003) developed an improved cycle-rickshaw, called the 'Oxtrike'. It was designed to be more efficient that the standard rickshaw, and also to be easily manufactured in small workshops in the poor world. Yet it, like many such technologies, did not diffuse around the poor world to any great extent. There was suspicion of such technologies as second-rate technologies. Why should not poor countries have the best, they asked?

There is a great difference between invention *for* the poor world and invention *in* the poor world. The invention and development taking place outside the world of western NGOs was surely much more significant than these efforts. Although not recorded in patents or copyrights, it too is important, changing the material structure of the world, for example in the case of the poor mega-city, the work of millions of untutored architects, engineers and builders.

New inventive institutions

The third kind of account focuses on telling the story of successive kinds of inventive organisation. In essence, it goes like this. In the heroic period of the industrial revolution invention was the work of individual inventors. From the late nineteenth century science and technology came together, and invention became the province of the corporate research laboratory, particularly in electricity and chemicals. By the 1970s and 1980s the key inventive institution had become the biotechnology and information technology start-up, the science park and the entrepreneurial university. Again, there is something in the story, but the timings and the substance are very misleading.

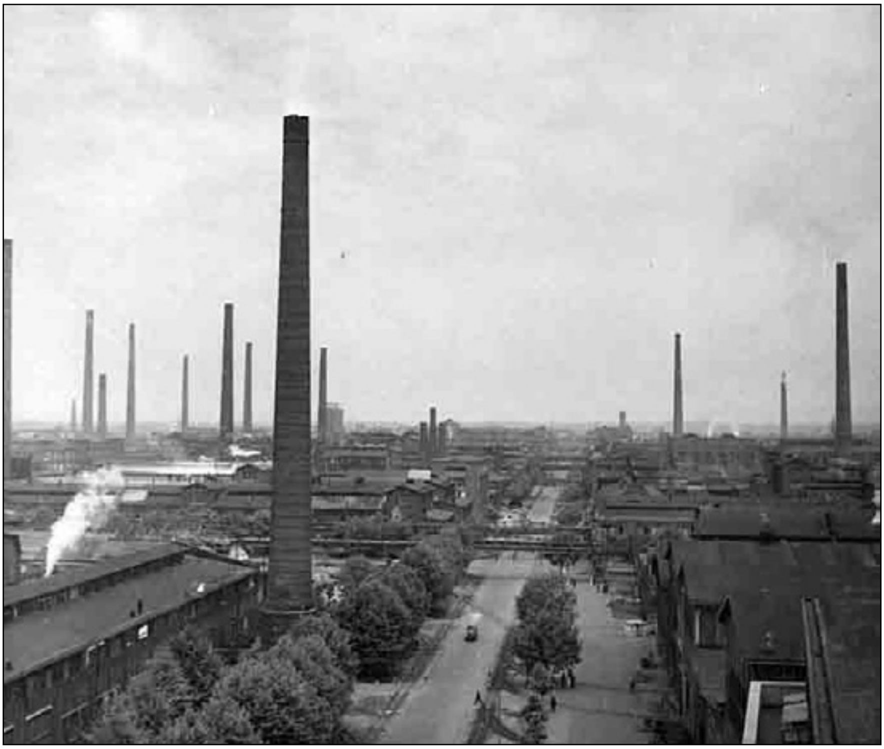
Take the timing. Around 1900 there is little doubt that a majority of patents were still granted to individual inventors. Only as the century progressed did significant proportions of patents go to large firms. Corporate research laboratories and state organisations, while active around 1900, really came into their own only after 1945. Since then, the individual inventor has not disappeared – he (for invention has been a very masculine activity) has operated in a new context. Nor indeed has the large corporate inventor. One of the most striking features of the history of invention is the long lives of inventing organisations.

Around 1900 one could see an important change within some industries and some firms as to how they organised some of their inventive activity. 'Research' was established in firms for the first time, to supplement the existing scientific and engineering work.[7] A majority of scientists and engineers continued to be employed in routine jobs, in production, in analytical labs and in development labs.

The first research revolution in industry was not, as used to be thought, derivative of a research-centred academy, a kind of application in industry of an academic model. It was the result of a revolution that was taking place slowly but simultaneously in industry, government and the academy. In each a new research-focused science and engineering emerged. Universities went from being teaching institutions to teaching and research institutions (as did medical schools); government scientists and engineers became concerned not just with building roads, or enforcing, say, food standards, but also creating new knowledge and new things.[8]

Research organisations were typically created in firms which were already large and technologically progressive, indeed often dominant in their field. The German synthetic dye firms, such as BASF, Hoechst, Bayer and AGFA, were well-established world leaders in synthetic dyes when they introduced research laboratories. Bayer did so in 1891, and only 20 per cent of its chemists were in research by the early part of the new century. In the United States the research revolution was led by even larger firms. The first case usually cited is the 1900 establishment of the General Electric Laboratory. Other significant research laboratories were established by the explosives firm Du Pont (1902 and 1903), telephone company AT&T (around 1911, when a research branch was added to the engineering department of its manufacturing arm, Western Electric), and the photographic giant Eastman Kodak (*c.* 1912). All these firms were already very large, innovative in 'science-based' technologies, and employed an abundance of scientists and engineers. Kodak and General Electric were already powerful multinational enterprises, leading the world photographic and electrical industries. AT&T dominated American telephony and telegraphy.

One of the main factors leading to the establishment of research in these firms was potential threats to their dominance from *European* innovations. These innovations were not themselves the product of industrial research. Eastman Kodak felt threatened by the Lumière brothers' Autochrome process, which produced beautiful colour images. GE was concerned about a radically different kind of electric light invented by the German academic chemist Walter Nernst. His lamp was made of a material which conducted electricity and glowed when hot. It could be lit with a match. The rights had been acquired by the German electrical firm AEG, and they made Nernst a rich man. The lamp was to have only modest success, mostly in micro markets. One such was in the first successful photoelectric fax machines, which were designed by Arthur Korn and in use before the First World War.



25. One of the world's great centres of invention, the Bayer works at Leverkusen, c. *1947. It was a great centre for the production of dyestuffs, pharmaceuticals, and much else besides, from the late nineteenth century to the present. Like many great researching corporations it is older than most nation-states.*

AT&T feared radio would undercut its telephone business; radio was the work of individual European inventors, among them Guglielmo Marconi.

Industrial research would prove to be one of the factors that kept these firms dominant for decades, hence the familiarity of their names. The main research laboratories of General Electric and Du Pont are still where they were established more than one hundred years ago. At least fifteen out of the twenty-three firms listed as the top R&D spenders in 1997 (and 2003) were formed before 1914, and of these at least five were important in industrial research. Of course there were new entrants to the top ranks, and they include Japanese car and electrical firms in particular.

The great industrial research centres founded around 1900 had a history of expansion. Before the Great War Du Pont spent around 1 per cent of its turnover on R&D, going up to 3 per cent in the interwar years. Between the 1950s and 1970s it was at 7 per cent of a much larger company. At the end of the 1960s Du Pont declared its programme of development of new products from its own research an expensive failure. In the 1970s it cut back on its R&D expenditures, and short-range work on existing products was emphasised. By 1975 the research intensity fell to 4.7 per cent, and to 3.6 per cent by 1980. The 1980s saw a return of interest in R&D, but largely in the life sciences. Yet Du Pont remains among the great spenders. It is still in the list in 1997, but because of cuts in research it dropped way down by 2003.

Another example of long-term dominance is AT&T. Its research branch was incorporated into its subsidiary Bell Labs in the 1920s, a company which saw quite extraordinary growth and output through the twentieth century. It was a world leader in information technology from the 1920s and expanded enormously through the 1930s and up to the late 1970s. Among its products are the transistor, invented in 1947, the UNIX operating system of the 1960s, and the Digital Signal Processing chip in 1979, now ubiquitous in mobile phones and much else. Much reduced since by the breakup of AT&T's telephone monopolies and its transfer to Lucent Technologies, it was nevertheless still in the top twenty in 1997, well ahead of, for example, Intel. Since then it has shrunk enormously, but it is still bigger than it was in the mid-1920s.

The development of the transistor and the integrated circuit in the 1950s, 1960s and 1970s was in part the work of entrepreneurial small firms. Transistor development and production were quickly taken by Bell staff to smaller and newer enterprises. Texas Instruments, with a former Bell employee, made the first silicon transistor in 1955. William Schockley, one of the inventors of the transistor, set up a semiconductor firm in California. Experts left to form Fairchild Semiconductor in 1957, the company that introduced the key planar process for the making of integrated circuits. Fairchild and Texas Instruments were granted key patents in 1959. Fairchild employees set up most of the new semiconductor enterprises of the 1960s, largely in the area that became known as Silicon Valley, which had since the 1930s welcomed new industries and had strong connections to new universities and, critically, the expanding US military. Among the new semiconductor enterprises was Intel (1968), which introduced the microprocessor, the computer on the chip, in 1971.

The great firms in information-technology invention today are a mixture of ancient firms, and start-ups of decades ago: Siemens, IBM, Microsoft, Nokia, Hitachi and Intel (see Table 8.1, p. 204). They have R&D budgets only ever exceeded by those above them in the list today. In semiconductors and software the age when the small entrepreneurial university-linked start-up was crucial was the 1950s, 1960s and 1970s, not the later period when they were supposedly dominant. Hitachi and Siemens were both formed before the Great War, as of course was Bell Labs. But perhaps the most telling case is International Business Machines (IBM), for decades synonymous with the computer, from the mainframe to the PC. Even before the Great War it was a huge force in calculating machines around the world. In the 1940s and 1950s it still led, now in electro-mechanical machines. In the 1950s an MIT engineer designed a vast computerised system for US air defence, project SAGE. The contract to build these machines was given to IBM, despite it not having any experience of electronic computers. From this IBM would become, unexpectedly, the leading force in electronic computing, especially with the launch of System 360 in the early 1960s. It remains a major R&D spender.

In the case of biotechnology too, the boom in pharmaceutical research has been led by gigantic spenders, not by start-ups. And they have also been around for a very long time. All the largest spenders on R&D in pharmaceuticals/biotechnology are very old firms. Pfizer, Johnson and Johnson, Roche, each of the Swiss companies that merged to form Novartis (Ciba, Geigy, Sandoz) and those that merged to form Aventis (Hoechst and Rhone Poulenc) were all founded in the nineteenth century, as were all the parts of Glaxosmithkline (Glaxo, Wellcome, Smith, Kline French, Beechams, Allen & Hanbury). Not all, but many, were important in pharmaceutical research and production before 1914. The start-ups of the 1970s and 1980s are well behind.

Although the big car companies which head the list of R&D spenders today existed well before the First World War, they were not known for R&D until well after the Second, with the partial exception of General Motors. They did little research, yet they were very inventive. The Model T launched in 1908 was a new kind of car: it was a sturdy, light, cheap vehicle, well adapted for use in the countryside. It did not come out of a laboratory, but from a small firm. By January 1910 Ford moved into a vast new concrete and glass factory at Highland Park, with its own palatial power station. It employed 3,000 production employees in 1910, expanding to over 14,000 by 1913.[9] Few of today's fastest-growing companies could match that rate of growth.

In the car industry, and many others, there were few if any laboratories, but plenty of development workshops and testing facilities such as tracks, wind tunnels and hydrodynamic tanks. Such facilities were important in generating much-needed knowledge of things such as propellers, hull shapes, aeroplanes and materials. In these places the designers and the engineers long held sway. They were trying to achieve particular levels of performance, often through incremental change, and much design work involved a great deal of calculation and modelling.

The Second World War brought a radical change in scale in this inventive and development activity. Aircraft and aero-engines alone were huge elements of post-war R&D, largely funded by the state. Alongside this and organised in a similar way were the rocket programmes and the development of new computing machines. Decisions were made to devote huge resources to particular projects, resulting not only in an increase in spending, but in a progressive reduction in the number of projects. The DC3 airliner cost around $300,000 to develop in the late 1930s, while the larger DC4 in the mid-1930s took $3.3m; in the 1950s the DC8 cost $112m.[10] The cost of aircraft development continued to increase, as did that of car development.

Missiles, short-range and intercontinental, as well as space launchers also consumed vast sums in development expenditure.

The phrase 'research and development' became a term of art especially around the Second World War in both government and industry. It is an unfortunate term, as 'development and research' would more accurately reflect the fact that development expenditure was much larger than research expenditure.

How does the bomb project fit in?

In the history of twentieth-century science, technology and invention, no project has so central a place as the US atomic bomb project of the Second World War (though not the later work). It has profoundly affected what we take to be significant in the history of twentieth-century science, especially before 1939, and figures as one of the great technologies of the middle of the century. It also marks what is regarded as a hugely important organisational innovation in the history of science and technology – the rise of 'big science'. It is made unprecedented in world history through the discounting of the many precedents that existed. Once we put the old into the story, it will look very different.

Let us start with the name. The use of the term 'Manhattan Project' obscures an important word in its full name, which was 'Manhattan Engineer District'. It was so-called because it was run by the US Army's Corps of Engineers, a prestigious old institution that had long taken the best graduates from the West Point military academy. The Corps was organised into districts, and they created one for this new project, which was a production, development and research project. In the usual stories about big science, its phenomenal cost of $2bn is referred to as if this was the cost of the research and development effort, when, in fact, most of the $2bn went on the building of two nuclear factories at Oak Ridge and Hanford. General Leslie Groves, the head of the project and senior member of the US Army Corps of Engineers, had previously supervised the building of munitions plants, issuing contracts worth much more than the entire cost of the Manhattan Project.[11]

Through the war the research and development cost was $70m

26. The military engineer Brigadier General Leslie Groves was the director of the Manhattan Engineer District project, running everything from its research to the construction of the factories. Yet it is often implied that one of his subordinates, the director of the Los Alamos laboratory, Robert Oppenheimer, was in charge. The academic-research-centred view of invention systematically downplays the crucial non-academic and non-physics elements of such projects as the bomb.

($800m in 1996 dollars). This was a very large sum for the time, but within an order of magnitude of other projects. Assuming each type of bomb cost $35m to develop, that was around ten times the development cost of the pre-war DC4 aircraft, and about the same as that of a new car today. There were many other very large projects, even in the USA. Among them were radar development, a huge effort to make new synthetic rubbers following the fall of the world's main rubber plantations to the Japanese, and indeed large projects in medicine, among them penicillin and anti-malarial compounds. These all built on decades of experience in large-scale research and development, from nylon to coal hydrogenation, from motor cars to large airships.

Is the rate of invention ever increasing?

Given the paucity of and poor quality of the data, constructing a historical story of the changing patterns of invention is problematic. We should thus be sceptical of any claim for an increase or a decrease in the rate or significance of inventions in any particular historical period. The measures by which any such conclusion could be arrived at simply do not exist, and such measures as do exist suggest caution should be exercised. The main statistical information we have on invention is numbers of patents. Patents are legal documents granted to inventors giving them exclusive rights to the invention for fixed periods. Yet only some inventions are patented, and many developments cannot be patented. The existence of a patent gives no indication of its significance, nor that of the underlying technology. Furthermore different nations adopted different patent systems, and all changed over time. Inventors have differed in their desire to get patents too. Only a small proportion of patents are ever worked; indeed only 10 per cent or less have been kept in force for their permitted time. Unlike most property, most patents turn out to have no value at all. Patents are a particular kind of legal claim on a certain invention, not necessarily one anywhere near being exploited successfully.[12]

Yet we can get some useful hints from this statistical history. Firstly, and perhaps most surprisingly, the rate of patenting has not changed much over time. US patents granted to US residents varied between around 30,000 per annum to 50,000 per annum between 1910 and 1990, despite population growth, and even more significant economic growth. In some periods, notably the early 1930s, there were significant falls in patenting activity. This led to the belief among many that large US monopolies were retarding technological progress.[13] Since the early 1980s there was a steady increase, such that resident patent grants reached around 80,000 at the beginning of the twenty-first century. In the European Union growth has been slower. To reinforce the earlier caution about drawing too many conclusions we should note that by these measures Japan was, at the end of the twentieth century, three times more inventive than the United States, and Korea more than twice as inventive. Is this plausible?

Another way of getting at these issues is to look at research and development expenditure. This is an input into some, but certainly not all invention. Most has gone on the development not invention as such. R&D expenditure was tiny in comparison to the economy in 1900, then these expenditures, by both government and industry, grew rapidly through the decades to the 1960s, much faster than the growing economy, especially in the long boom, and reached around 3 per cent of GDP for the richest countries. From the late 1960s R&D grew about as fast as the economies of rich countries, meaning that the proportion of GDP accounted for by R&D has hardly changed. Given that the rate of growth of the main R&D performers was low by historic standards, it follows that the rate of increase of R&D expenditures slowed down in the last decades of the twentieth century. Although the rate of growth of R&D has slipped very considerably, the actual amounts spent were greater year on year, with falls in some years.

Increases in R&D expenditure suggest that inputs into invention and development have grown very significantly with time. Yet, these increases did not lead to any comparable rise in the number of patents. This suggests, again, that patents may be a very poor indicator of invention, and certainly of development. It could also suggest that over the century the costs of invention and development have been rising. Some have felt that innovations became increasingly trivial and expensive.[14] One area where there has been a clear decline in R&D productivity is in pharmaceuticals. The number of new chemical entities (NCEs) approved by the US Food and Drug Administration doubled between 1963 to the end of the century: they averaged about fourteen per annum in the 1960s and 1970s, rising from the 1980s to reach around twenty-seven in the 1990s. Yet over the same period the R&D expenditure of the pharmaceutical industry grew nearly twenty-fold.[15] The common explanation is that the development costs of drugs have increased, especially as clinical testing becomes more expensive and time-consuming. A recent estimate of the total cost of

R&D to achieve an approved new chemical entity in the pharmaceutical industry is about $400m, though it needs to be recognised that this includes costs of projects which were stopped before they reached the end; thus the costs of *successful* projects are lower.[16]

Another factor needs to be taken into account. The NCE measure tells us nothing about the efficacy of new approved drugs, nor how different they are from each other. It could be that new drugs are radically better than old ones, yet there have been few if any drugs of the significance of those produced decades earlier. Pharmaceutical companies make huge investments in the development, testing and marketing of 'me-too' drugs, minor variants of existing treatments. They are inventing and developing better mousetraps.

The pharmaceutical firms now account for around one-third of all development and research expenditure. Pharmaceuticals plus the motor-car industry perform around half the world R&D total. Yet it is hardly the case that the *new* products of either industry have been making anything like the radical difference made when these industries spent much less on R&D. There is nothing as novel or as significant as penicillin or the Model T.

What then of biotechnology, a central case for the argument that invention has shifted from corporate laboratories? The record here has been very disappointing if one looks behind the hype. Even in the face of low invention in traditional pharmaceuticals, only about a quarter of new pharmaceuticals are of biotechnological origin, though on a stricter definition it is considerably lower. Even on the widest possible definition, biotechnology-originated pharmaceuticals account for only 7 per cent of drug sales. In 2004 the leading biotech firm (Amgen, founded in 1980) had sales only one-fifth of each of the leading three or four firms in the pharmaceutical industry. The pioneering company, Genentech, founded in 1976, had sales of $4bn in 2004. There have been twelve significant new biotechnological drugs in terms of sales since the 1980s, three of them synthetic replacements for existing ones. Only sixteen new biotechnological drugs offering more than minimal improvement over previous treatments have been launched since 1986. More interestingly, biotech innovations are already declining in additional clinical efficiency, and there has been a lot of me-too innovation in this field too. The impact on overall health will be minimal, despite enormous private and public investment in invention, partly because the drugs are for rare conditions.[17]

It is little wonder that in the pharmaceutical industry and biotech the investment in public relations and marketing is so huge. Pharma companies spend more on marketing than R&D – which tells us that they are not selling products that are obviously superior to those of their competitors. Penicillin did not need marketing; particular variants did.

It is against that background that we should consider easily the most cited piece of evidence for a rapidly increasing rate of change in technology in recent years – the power of computing. It has proceeded at an astonishingly fast rate. In 1965 Gordon Moore, the research and development director of Fairchild Semiconductor, and soon to be one of the founders of Intel, suggested that the number of transistors on an integrated circuit that could be economically made would continue to grow at the same rate as in the early 1960s. In 1975 he thought growth would continue, but at *half* the rate he was measuring in 1965. Indeed the rate did fall, but there was a steady increase at roughly the rate predicted in 1975. But that rate of change was enormous. Between the 1970s and the early 1990s Intel's own processors increased the number of components at the constant rate of 100 times per decade. In the late 1990s that rate increased, though not to 1960s' levels.

That one-hundred-fold-a-decade rate of change sustained for forty-five years is unprecedented. We do not find it in motor cars at the beginning of the century or since. We do not find it anywhere else today either. It cannot stand for technical change in general.

By the standards of the past, the present does not seem radically innovative. Indeed judging from the present, the past looks extraordinarily inventive. We need only think of the twenty years 1890–1910 which gave us, among the more visible new products, X-rays, the motor car, flight, the cinema and radio, most of them expanding technologies to this day.

Table 8.1 **R&D expenditures of the largest R&D-funding firms in the world 1997 and 2003, £m at 1997 and 2003 exchange rates**

Company	***1997 R&D spend £m***		***2003 R&D spend £m***
General Motors	4983.591	*Ford Motor*	4189.71
Ford Motor	3845.266	*Pfizer*	3983.58
Siemens	2748.690	*DaimlerChrysler*	3925.45
IBM	2617.601	*Siemens*	3883.17
Hitachi	2353.534	Toyota Motor	3483.99
Toyota Motor	2106.695	*General Motors*	3184.18
Matsushita Electric	2032.720	Matsushita Electric	3019.18
Daimler-Benz	1914.146	Volkswagen	2917.14
Hewlett-Packard	1870.670	*IBM*	2826.1
Ericsson Telefon	1856.885	Nokia	2802.99
Lucent Technologies	1837.243	*Glaxosmithkline*	2791.00
Motorola	1670.111	*Johnson & Johnson*	2616.61
Fujitsu	1649.168	Microsoft	2602.65
NEC	1629.157	Intel	2435.62
Asea Brown Boveri	1614.805	Sony	2309.76
EI du Pont de Nemours	1576.516	*Ericsson*	2275.52
Toshiba	1554.453	*Roche*	2152.67
Novartis	1538.814	Motorola	2106.59
Intel	1426.401	*Novartis*	2098.21
Volkswagen	1487.240	*NTT*	2063.94
NTT	1535.634	*Aventis*	2060.32
Hoechst	1348.656	Hewlett-Packard	2040.11
Bayer	1339.868	*Hitachi*	1938.1
		AstraZeneca	1927.83

Italics – company founded before 1914. In the case of NTT, the crucial date is the foundation of the telephone and telegraph system in Japan, both nineteenth century.
Source: 2004 and 1998 R&D Scoreboards.
http://www.innovation.gov.uk/projects/rd_scoreboard/downloads.asp

Table 8.2 **Industrial Nobel prizes**

Physics

1909 Guglielmo Marconi – Marconi Co.
1912 Nils Gustaf Dalén – Swedish Gas Accumulator Co. (AGA)
1937 Clinton Davisson – Bell Labs
1956 William Schockley, John Bardeen and Walter Brattain – Bell Labs
1971 Dennis Gabor – British Thomson-Houston (AEI)
1977 Philip W. Anderson – Bell Labs
1978 Arno Penzias – Bell Labs
1986 Gerd Binnig and Heinrich Rohrer – IBM Switzerland
1987 Georg Bednorz and Alex Mueller – IBM Switzerland
1997 Steven Chu – Bell Labs
1998 Horst Stormer – Bell Labs
2000 Jack Kilby –Texas Instruments

Chemistry

1931 Friedrich Bergius – various and Carl Bosch – BASF/IG Farben
1932 Irving Langmuir – General Electric
1950 Kurt Alder – academia/IG Farben
1952 Archer Martin and Richard Synge – Wool Industries Research Association, Leeds

Medicine

1936 Henry Dale – academia/Burroughs Wellcome
1948 Hermann Mueller – Geigy
1979 Godfrey Hounsfield – EMI
1982 John Vane – academia/Wellcome
1988 James Black – ICI/SKF/Wellcome, Gertrude Elion – Wellcome USA and George Hitchins – Wellcome USA

In some of these years the prizes were awarded to more scientists and engineers than those named here. The firms given are those associated with the Nobel prize work – the Laureates were sometimes elsewhere when they were awarded the prize.
Source: my analysis of the extensive online information available for Nobel Laureates provided by the Nobel Foundation (*www.nobel.se* or *www.nobelprize.org*).

Conclusion

We have long been told that we live with an 'ever-increasing rate of change', yet there is good evidence that it is not always increasing. Measuring change is extremely difficult, but let us start with economic growth in the rich countries as a crude measure. While there was rapid growth before the Great War, there was slower growth overall between 1913 and 1950. There was spectacular growth in the long boom, followed by less strong growth since. In other words, growth rates were lower in the interwar years than before 1914, and average rates of economic growth after 1973 were considerably lower than in the period 1950–1973. In the 1970s there was a 'productivity slowdown' and since then the rich world has continued to grow, but not at historically unprecedented rates.

Since the 1980s one could be forgiven for believing that high growth rates had returned, not least because of the constant evocations of the notion of 'ever increasing change', and all the talk of fundamental transitions to new economies and new times. But in the USA, Japan, the EU and Britain, growth rates were lower in the 1990s than in the 1980s, lower in the 1980s than in the 1970s and lower in the 1970s than in the 1960s.[1] In the USA it appears that productivity growth increased in the late 1990s, but there is still a dispute as to whether this was general, or concentrated in the computer-manufacturing sector.[2] Growth is not the same as change but there is no evidence that structural change in the rich countries was any faster in recent decades than in the long boom. Once again, our future-oriented rhetoric has underestimated the past, and overestimated the power of the present.

Not all parts of the world grew at these same rhythms. For example the USSR grew very fast in the 1930s, while the rest of the world did not.

Especially since the 1970s many economies in the Far East have grown very fast, but from a low base. The increasing scale of the Chinese economy in particular has meant that its growth has been enough to alter the global statistics materially. For example, global steel production is growing at long boom rates again thanks to China.

Another important feature of change in the last three decades is that there has been a decline of economies, as well as growth. In some places the last years of the twentieth century saw retrogression. The income per head of the 700 million sub-Saharan Africans fell from $700 per head in 1980 to the even more miserable $500 at the end of the century; to make matters worse for the majority, 45 per cent of this output was produced in South Africa so the real fall elsewhere was even worse.[3] Malaria has become more common, and new diseases such a HIV/AIDS have swept through the continent as no other. Yet this is not a reversion to an old world, for this is a continent with cars and new kinds of shanty towns, a rapidly urbanising world without what is taken to be modern industry.

From 1989 there was a remarkably rapid collapse in the economies of the Soviet Union and its former satellites, of 20, 30 and 40 per cent, far outstripping the capitalist recession of the early 1980s. Although this dramatic fall in output cannot be characterised generally as a technological retrogression such a phenomenon was evident in some places. Now independent Moldova, formerly part of the USSR, lost 60 per cent of its output. Machines virtually phased out as the economy had developed since the Second World War, things such as 'spinning wheels, weaving looms, butter churns, wooden grape presses and stone bread ovens – are now back in use', it was reported in 2001. The 'only way to survive is to be totally self-sufficient,' claimed the curator of the ethnological museum in Belsama, 'and that means turning the clock back.'[4] Cuba, as we have seen, expanded the number of its oxen as it lost its supply of tractors.

In some industries, such as shipbreaking, there has been a move towards a new kind of low-tech future. By the 1980s Taiwan had become by far the largest shipbreaker, demolishing more than a third

27. The Brazilian aircraft carrier Minas Gerais*, broken up with a novel lack of modern technology on Alang Beach, Gujarat, India, in 2004. Alang Beach became the single largest centre of the shipbreaking industry, and a startling example of the new technological retrogression. The ship, originally HMS* Vengeance*, launched in 1945, was built at a time when shipbreaking was more capital intensive.*

of the world's ships. By the early 1990s it was out of this industry, now dominated by India, Pakistan and Bangladesh, which between them had more than 80 per cent of the world market by 1995.[5] Taiwan used specialised dock facilities, but the new shipbreaking was on beaches, with the most minimal equipment, carried out by thousands of barefoot workers. The reason shipbreaking was done in these places was that scrap steel was in demand locally. But it is used in a markedly different way from other places and times: it is re-rolled, re-worked, rather than used to make fresh steel.

What seems at first technological retrogression was perhaps not unknown in earlier years of the century. No one had ever attempted to build such a large canal with what were then such primitive means as were used on the White Sea canal or in the erection of the great steel works of Magnitogorsk. Collective farming itself involved technological retrogression, for all the emphasis on the tractor. However, not for many centuries has a global industry retrogressed like shipbreaking has.

This book has argued for the importance of the seemingly old. It is also a plea for a novel way of looking at the history of the technological world, one which will change our minds about what that world has been like. And implicit in it is a plea for novel ways of thinking about the technological present.

We should be aware, for example, that most change is taking place by the transfer of techniques from place to place. The scope for such change is enormous given the level of inequalities that exist with respect to technology. Even among rich countries there are very important differences in, for example, carbon intensity. If the USA were to reduce its energy-use levels to those of Japan, the impact on total energy use would be very significant. But for poor countries, as well as for rich ones, such a message is often unwelcome. For *imitating* is seen as a much less worthy activity than innovating. To imitate, to replicate, is to deny one's creativity, to impose upon oneself what was designed for others by others. '*Que inventen ellos*' ('Let *them* invent') is seen not as sensible policy advice, but a recipe for national humiliation. To have technology or science is, it is often deeply felt, to create something new. The answer to such concerns that is implicit in this book is that all countries, firms, individuals, with rare and unusual exceptions, have relied on others to invent, and have imitated more than they have invented.

Arguments about imitating policies and practices for innovation might seem to fall in the same category. That is to say, that it might seem like a good thing that they should be the same or similar around the world. Indeed there is a remarkable lack of originality in innovation policies globally, and many explicit calls for copying those perceived as the most successful models. Yet while copying existing technology is very sensible, imitating innovation policies may be a mistake. For if all nations, areas and firms are agreed about what the research should be, by definition it will no longer be innovative; and it might not be a good thing that all nations pursue the same policies for research, because they are likely to come up with similar inventions only a few of which will be used even if technologically successful. 'If I knew the future of jazz I'd be there already,' said one wise musician.

Calling for innovation is, paradoxically, a common way of avoiding change when change is not wanted. The argument that future science and technology will deal with global warming is an instance. It is implicitly arguing that in today's world only what we have is possible. Yet we have the technological capacity to do things very differently: we are not technologically determined.

Getting away, as this book has, from the conflation of use and invention/innovation will in itself have a major impact on our thinking about novelty generation. The twentieth century was awash with inventions and innovations, so that most had to fail. Recognising this will have a liberating effect. We need no longer worry about being resistant to innovation, or being behind the times, when we chose not to take up an invention. Living in an inventive age requires us to reject the majority that are on offer. We are free to oppose technologies we do not like, however much interested pundits and governments tell us it is essential to accept, say, GM crops. There are alternative technologies, alternative paths of invention. The history of invention is not the history of a necessary future to which we must adapt or die, but rather of failed futures, and of futures firmly fixed in the past.

We should feel free to research, develop, innovate, even in areas which are considered out of date by those stuck in passé futuristic ways of thinking. Most inventions will continue to fail, the future will remain uncertain. Indeed the key problem in research policy should be ensuring that there are many more good ideas, and thus many more failed ideas. Stopping projects at the right time is the key to a successful invention and innovation policy, but doing this means being critical of the hype that surrounds, and often justifies and promotes funding for invention.

Although we can stop projects, it is often said that we cannot un-invent technologies, usually meaning that we cannot get rid of them. The idea is itself an example of the conflation of invention and technology. For most inventions are effectively un-invented, in being forgotten and often lost. A few things are going out of use as the world economy grows – among them are asbestos, declining since the 1970s, and refrigerants like CFC gases. And one of the new tasks faced by scientists and engineers is actively making old technologies disappear, some of which, like nuclear power stations, are extremely difficult to dispose of.

Thinking about the technological past can give us insights into 'the question of technology' – what is it, where does it come from, what does it do? But this book has attempted to do much more than take historical examples to address this perennially interesting question. It has been concerned primarily with asking questions not about technology, but about technology in history – asking questions about the place of technology within wider historical processes. This important distinction is not obvious, but it is central to a proper historical understanding of technology. It will help wean us off the idea that invention, 'technological change' and the 'shaping of technology' need to be the central questions for the history of technology. Instead the history of technology can be much more; and it can help us rethink history.

If we are interested in the historical relations between technology and society we need a new account not only of the technology we have used but also of the society we have lived in. For existing histories of twentieth-century technology were embedded in particular assumptions about world history, while world histories had embedded in them particular assumptions about the nature of technological change and its impact – each was already defined, usually implicitly, in relation to the other. Hence the history of the society into which this new account is placed is very different from the one usually found: for example, it takes as central the expansion of a new kind of poor world, a world which has been almost continuously at war, and in which millions have been killed and tortured. This necessitates an account of the global technological landscape that is very different from those found implicitly and explicitly in existing global histories and histories of technology – and an account that might help revise our views about world history.

It is a measure of the importance of technology to the twentieth century, and to our understanding of it, that to rethink the history of technology is necessarily to rethink the history of the world. For example, we should no longer assume that there was ineluctable globalisation thanks to new technology; on the contrary the world went through a process of de-globalisation in which technologies of self-sufficiency and empire had a powerful role. Culture has not lagged behind technology, rather the reverse; the idea that culture has lagged behind technology is itself very old and has existed under many different technological regimes. Technology has not generally been a revolutionary force; it has been responsible for keeping things the same as much as changing them. The place of technology in the undoubted increase in productivity in the twentieth century remains mysterious; but we are not entering a weightless, dematerialised information world. War changed in the twentieth century, but not according to the rhythms of conventional technological timelines.

History is changed when we put into it the technology that counts: not only the famous spectacular technologies but the low and ubiquitous ones. The historical study of things in use, and the uses of things, matters.