26/11/97

# "WHAT'S THE USE OF BASIC SCIENCE?"

C.H. Llewellyn Smith

## 1. Introduction

Over 200 years ago, at the beginning of 1782, the German physicist and philosopher Christof Lichtenberg wrote in his diary

*"To invent an infallible remedy against toothache, which would take it away in a moment, might be as valuable and more than to discover a new planet... but I do not know how to start the diary of this year with a more important topic than the news of the new planet".*

He was referring to the planet Uranus, discovered in 1781. The question Lichtenberg implicitly raised, of the relative importance of looking for technical solutions to specific problems, and of searching for new fundamental knowledge, is even more pertinent today than it was 200 years ago.

In this paper I shall argue that the search for fundamental knowledge, motivated by curiosity, is as useful as the search for solutions to specific problems[1]. The reasons we have practical computers now, and did not have them 100 years ago, is not that meanwhile we have discovered the need for computers. It is because of discoveries in fundamental physics which underwrite modern electronics, developments in mathematical logic, and the need of nuclear physicists in the 1930s to develop ways of counting particles.

I shall cite many examples which demonstrate the practical and economic importance of fundamental research. But if fundamental, curiosity-driven, research is economically important, why should it be supported from public, rather than private, funds? The reason is that there are kinds of science which yield benefits which are general, rather than specific to individual products, and hence generate economic returns which cannot be captured by any single company or entrepreneur. Most pure research is consequently funded by people or organizations who have no commercial interest in the results and the continuation of this kind of funding is essential for further advance.

---

[1] This paper, which makes no great claim to originality, is based on a colloquium given at CERN on 12 June 1997, which was developed from earlier talks and articles[1–3] given or written during the last twelve years. Over this period I have assimilated a number of arguments and quotations from a variety of sources, many of which I have now forgotten. I apologise to those whose contributions to the subject matter of this paper are not properly acknowledged. For references to the expert literature on science funding see ref. 4. As Director-General of CERN, I have been involved in discussions of science funding with representatives of governments on average about once a week. These discussions naturally focused on particle physics, which is therefore singled out for special comment at a number of places in this paper.

It would certainly be naive, even wrong, to equate the pure uniquely with the general, and the applied with the specific, but it is far more likely that a substantial proportion of the benefits of applied research will accrue to those who undertake it. Furthermore, once definite economic returns can clearly be anticipated, the private sector, motivated by profit, is generally better placed to undertake the necessary research and development. It follows that a policy of diverting public support from pure to applied scientific research would also divert funds from investment which only the public sector can make, to areas where the private sector is generally likely to do better.

Section 2 of this paper contains some general remarks on the difference between basic and applied science. Section 3 then describes the benefits of basic science. In Section 4, the above well-known argument that governments have a special responsibility to support basic science as a "public good" is elaborated. This argument, which is relatively easy to make, leads to two much harder questions, which are dealt with in Sections 5 and 6 respectively:

i) If companies can leave funding of basic science to governments, why can some governments not opt out – leaving it to others – as it is sometimes argued Japan has done very successfully?

ii) How should governments choose what to support, and at what level?

## 2. Basic versus Applied Science

In industry the term "research" is frequently used to describe innovation with existing technology, which academic scientists would normally describe as development. This different use of the word "research" can lead to many misunderstandings. In this paper I use the word in the sense understood by academic scientists.

Misunderstandings also arise from the frequent assumption that advocates of the utility of basic science subscribe to the so-called "linear model" according to which basic research is supposed to lead to applied research, which in turn leads to industrial development and then to products. While there are many cases in which this has happened, it is also easy to find examples of advances in technology which have led to advances in basic science, such as that given by George Porter (Nobel Laureate in Chemistry) who pointed out that "Thermodynamics owes more to the steam engine than the steam engine owes to science".

Unfortunately, such examples have led some people to advocate an anti-linear model. For example, Terence Kealey has recently written a book[5–6)] arguing that economic progress owes nothing to basic science, which should therefore not be supported by governments. He points out correctly that the development of steam power, metallurgic techniques and textile mills which drove the start of the industrial revolution in England were based on scientific understanding and mechanical engineering principles dating from before the 17th century, and owed nothing to the 17th century scientific revolution (Newtonian mechanics, calculus, etc.). This is true, but it is certainly not true of many later industrial developments, as I hope the examples that I shall give later will demonstrate.

So the connection of science and technology is neither linear nor anti- linear, but in fact highly non-linear, and it has been claimed[7)] that "historical study of successful modern research has repeatedly shown that the interplay between initially unrelated basic knowledge, technology and products is so intense that, far from being separate and distinct, they are all portions of a single, tightly woven fabric". Nevertheless a broad distinction can be made between science (~ knowledge) and technology (~ means by which knowledge is applied), and between different forms of science.

I do not like the terms basic and applied science: after all who can say in advance what is applicable? However, these terms can be useful provided they are defined in terms of motivation:

> **Basic science** – motivated by curiosity
> **Applied science** – designed to answer specific questions.

Given these definitions, I will later argue that governments have a special responsibility to fund basic science while applied science can generally be left to industry. The distinction is, of course, not always entirely clear cut, and the term "strategic research" is sometimes used to describe science in an intermediate category which appears to have a good chance of applications even if it is done to satisfy curiosity, and is leading to new fundamental understandings. An example is research on the properties of two-dimensional semiconductors.

The difference between basic, or pure, and applied science was beautifully illustrated by J.J. Thomson – the discoverer of the electron – in a speech delivered in 1916[8)]:

*"By research in pure science I mean research made without any idea of application to industrial matters but solely with the view of extending our knowledge of the Laws of Nature. I will give just one example of the "utility" of this kind of research, one that has been brought into great prominence by the War – I mean the use of X-rays in surgery...*

*Now how was this method discovered? It was not the result of a research in applied science starting to find an improved method of locating bullet wounds. This might have led to improved probes, but we cannot imagine it leading to the discovery of the X-rays. No, this method is due to an investigation in pure science, made with the object of discovering what is the nature of Electricity."*

Thomson went on to say that applied science leads to improvements in old methods, while pure science leads to new methods, and that "applied science leads to reforms, pure, science leads to revolutions and revolutions, political or scientific, are powerful things if you are on the winning side". The important and very difficult question for those responsible for funding science is how to be on the winning side.

## 3. Benefits of Basic Science

Four classes of benefits can be distinguished, which are dealt with below in turn:

i) Contributions to culture
ii) The possibility of discoveries of enormous economic and practical importance
iii) Spin-offs and stimulation of industry
iv) Education

- **Contributions to Culture**

Our lives are enriched, and our outlook changed, by (e.g.) knowledge of the heliocentric system, the genetic code, how the sun works, why the sky is blue, and the expansion of Universe. The point was elegantly, if arrogantly, made by Bob Wilson (first Director of Fermilab, a large particle physics/accelerator laboratory near Chicago) who, when asked by a Congressional Committee *"What will your lab contribute to the defence of the US?"*, replied *"Nothing, but it will make it worth defending".* Generally, however, scientists are surprisingly shy in advancing cultural arguments, and this is a very ancient phenomenon as shown by the following dialogue in Plato's Republic:

*Socrates:* *Shall we set down astronomy among the subjects of study?*

*Glaucon:* *I think so, to know something about the seasons, the months and the years is of use for military purposes, as well as for agriculture and for navigation.*

*Socrates:* *It amuses me to see how afraid you are, lest the people should accuse you of recommending useless studies.*

I consider that scientists should advance cultural arguments more boldly. In particular, public expenditure on particle physics can and should be justified largely on cultural grounds. The globalization of particle physics helps, and it is relatively easy to convince most people that mankind as a whole should continue to explore this frontier of knowledge, and can afford to do so. When justifying particle physics, it is tempting to invoke spin-offs, such as the World Wide Web which was invented at CERN (more examples are given below), but in my opinion they provide a secondary argument and the contribution to knowledge should be put first. In my experience the general public generally finds the cultural argument at least, if not more, convincing than spin-offs, and it is dangerous to base arguments on examples of spin-off which may not stand up to careful analysis.

- **The possibility of discoveries of enormous economic and practical importance**

  It is not hard to show that expenditure on basic science often leads to discoveries of enormous economic and practical importance, is highly profitable, and has easily paid for itself. Casimir, the renowned theoretical physicist, and one-time Research Director of Philips, has given a splendid list of examples[9]:

  *"I have heard statements that the role of academic research in innovation is slight. It is about the most blatant piece of nonsense it has been my fortune to stumble upon.*

  *Certainly, one might speculate idly whether transistors might have been discovered by people who had not been trained in and had not contributed to wave mechanics or the quantum theory of solids. It so happened that the inventors of transistors were versed in and contributed to the quantum theory of solids.*

  *One might ask whether basic circuits in computers might have been found by people who wanted to build computers. As it happens, they were discovered in the thirties by physicists dealing with the counting of nuclear particles because they were interested in nuclear physics.*

  *One might ask whether there would be nuclear power because people wanted new power sources or whether the urge to have new power would have led to the discovery of the nucleus. Perhaps – only it didn't happen that way.*

  *One might ask whether an electronic industry could exist without the previous discovery of electrons by people like Thomson and H.A. Lorentz. Again it didn't happen that way.*

  *One might ask even whether induction coils in motor cars might have been made by enterprises which wanted to make motor transport and whether then they would have stumbled on the laws of induction. But the laws of induction had been found by Faraday many decades before that.*

  *Or whether, in an urge to provide better communication, one might have found electromagnetic waves. They weren't found that way. They were found by Hertz who emphasised the beauty of physics and who based his work on the theoretical* *considerations of Maxwell. I think there is hardly any example of twentieth century innovation which is not indebted in this way to basic scientific thought."*

Casimir's examples have a number of features in common

– the applications of new knowledge were highly profitable;
– they were totally unforeseen when the underlying discoveries were made;
– there was a long time-lag between the fundamental discoveries and their exploitation;
– the discoverers in general did not get rich.

We will return to some of the consequences of these features later.

There have been some attempts to quantify the huge pay-offs from fundamental research. I will mention three[2]:

1. A recent US National Science Foundation study found that 73% of the papers cited in industrial patents were published "public science", overwhelmingly basic research papers produced by top research university and government laboratories.

2. In the first paper I wrote on this subject[1)], with the well-known economist John Kay, we estimated – on the basis of the conservative assumption that without electricity national income today would be at least 5% less than it is – that the benefit to the UK economy of accelerating the development of electricity by Faraday, Maxwell and others by one year would have been (in 1985) at least £20B, or some £40B today. This example was later turned into a sound bite by Mrs Thatcher who liked to say that the work of Faraday was worth more than the valuation of the British stock market.

3. A much cited study by Mansfield[10)] in 1991 claimed to show that public investment in basic science generates a return of 28%. Mansfield's figure was derived from a sample of 75 major American firms in seven manufacturing industries (information processing, electrical equipment, chemicals, instruments, pharmaceuticals, metals and oil). He obtained information from company R&D executives concerning the proportion of the firm's new products and processes commercialised in 1975–85 that, according to them, could not have been developed (at least not without substantial delay) in the absence of academic research carried out within fifteen years of the first introduction of the innovation. Mansfield's work clearly demonstrates that there are large returns, but his analysis involves many assumptions and the actual figure should be treated with a large

[2] See ref. 4 for further discussion.

grain of salt. Indeed, given the very non-linear relation between research and final products, quantitative measurement is clearly essentially impossible.

It is sometimes said that the examples given above are all very well, but major benefits are unimaginable from such esoteric sciences as particle physics. In fact researches such as those cited by Casimir were regarded as equally esoteric at the time, and the danger of such *a priori* arguments is illustrated by the recent use of number theory in cryptology, although only 20 years ago it would have been regarded as one of the most "useless" branches of mathematics.

It is true that so far there have not been any direct applications of the discoveries of particle physics, but there have been some near misses. For example, if the muon (an unstable particle discovered in the 1940s) lived somewhat longer before decaying, muons could be used to catalyse nuclear fusion and generate huge amounts of energy. The discovery of long-lived charged particles which would catalyse fusion is not unimaginable. To give another possible example, certain "grand unified" theories of the known forces predict the existence of monopoles, which could be used to catalyse proton decay, thereby providing an essentially limitless supply of energy.

It is therefore not true that application of knowledge discovered in particle physics is unimaginable, even if it is unlikely. What is certainly the case, is that it will not be possible to exploit laws and facts of nature that remain undiscovered.

- **Spin-offs and stimulation of industry**

By spin-offs, I mean devices and techniques developed to do basic research which turn out to have other uses. I give some examples from particle physics (many could equally well be credited to nuclear physics, from which particle physics developed):

**Accelerators**[3]

- semiconductor industry
- sterilisation – food, medical, sewage
- radiation processing
- non-destructive testing
- cancer therapy
- incineration of nuclear waste
- power generation (energy amplifier)?
- source of synchroton radiation
- source of neutrons

} biology, condensed matter physics...

**Particle detectors**

- Crystal detectors[4]
  - medical imaging
  - security
  - non-destructive testing
  - research
- Multiwire Proportional Chambers
  - container inspection
  - research
- Semi conductor detectors
  - many applications at the development stage

**Informatics**

- World Wide Web[5]
- Simulation programmes
- Fault diagnosis
- Control systems
- Stimulation of parallel computing

---

[3] There are some 10,000 accelerators in the world today, of which only some 100 are used for their original purpose of research in nuclear or particle physics.

[4] Crystals developed for experiments at the LEP collider at CERN are now in use for medical imaging in hundreds of hospitals; in due course they will doubtless be replaced by crystals with superior properties currently being developed for the future LHC at CERN.

[5] A UK group has recently estimated that the Web, which was invented at CERN, already generates 5% of the sales of large companies, and that this will rise to 20% by the end of the decade.

**Superconductivity**

Particle physics → multifilamentary wires/cables → nuclear magnetic resonance imaging

+ **many others** (**cryogenics, vacuum, electrical engineering, geodesy**...).

People sometimes seem to think that presenting this long list of spin-offs from particle physics is enough to justify expenditure on our subject. However, making such a justification is not easy. First it would be necessary to quantify the economic benefits. Second, one would need to analyse what would have been the result of spending the money that has been put into particle physics in other ways, i.e. work out the so-called opportunity cost. It is not surprising that the large expenditure at CERN produces spin-offs: on the contrary, it would be very surprising if it did not, and expenditure of similar sums on other high-tech activities would also produce spin-offs.

It is, however, certainly fair to argue that the value of the spin-offs should be taken into account when considering the cost of basic science, and it is probably the case that the special demands of particle physics, which requires very sophisticated purpose-built equipment, make it especially good at producing spin-offs. In fact, generally economists are increasingly recognising the importance of spin-offs, especially in the form of instruments developed to do fundamental research[4)]. Much of the equipment in a modern electronics factory began in university laboratories, and there are many examples of instrumentation passing through all or part of the chain from physics to chemistry, to biology, to clinical medicine, to health care.

Given that basic scientists are motivated by the desire to gain priority, and generally to publish and publicise their work, whereas applied scientists working in industry are motivated by the desire to protect, hide and patent, it may paradoxically be that there is more spin-off from basic than applied research. Even as abstract and esoteric a field as general relativity (Einstein’s theory of gravity) has produced a spin-off. It is the navigational miracle known as the global positioning system, which can instantly and automatically tell you your position and altitude to within about ten metres anywhere on Earth. Over 160 manufacturers are developing GPS based systems world-wide for a new multi-billion dollar market. These systems work by comparing time signals received from different satellites. The clocks in the satellites are special atomic clocks originally developed, without any other motivation, to do research in general relativity, and in particular to check Einstein’s prediction that clocks run differently in different gravitational fields.

"Big science" also plays an important role in stimulating industry by demanding products and/or performance that are at or beyond current capabilities. Two studies[11–13] have attempted to measure a quantity which the authors call the

"Economic utility" = increased turnover + cost savings

resulting from contracts awarded by CERN (additional sales to CERN are not included in the increased turnover). This was done by interviewing a very large sample of firms that had high-technology contracts with CERN in the period 1973–82 (in electronics, optics, computers, electrical equipment, vacuum, cryogenics, superconductivity, steel and welding, and precisions mechanics). The estimates were made by the industrial managers, and not by CERN, and in cases of doubt the lowest figure was taken.

The conclusion was that high-technology contracts placed by CERN have an economic utility (normalised to the value of the initial contracts) of 3.0, i.e. every ECU paid to an industrial firm generates 3 ECUs of utility (normalized to the total CERN budget, the economic utility was 1.2). It is notable that only 24% of the CERN-related increased sales were in the high energy and nuclear physics market, the rest involving unrelated fields such as solar energy, the electrical industry, railways, computers and telecommunications. Although no similar studies have been conducted in the last few years, interviews conducted with industrialists in the course of PhD work in applied economics confirm the strong utility resulting from CERN contracts perceived by industry.

It is interesting to note that a similar study[12, 14, 15] commissioned by the European Space Agency (ESA) found a similar multiplier factor (2.9 in the 1982 study; 3.2 in the 1988 study, or 1.6 normalized to the total budget), although nearly 80% of the ESA-related increased sales remain inside the space sector and the rest is mostly in aeronautics and defence.

- **Education**

Research in basic science provides an excellent training in problem-solving for those who go on to work in applied research or development in industry. Furthermore, this creates very valuable networks of links between researchers in different industries and in academia, which would not exist if all training took place in industry. The value of such networks is increasingly recognised by economists as a benefit of publicly funded basic science[4].

In the particular case of work in experimental particle physics, it is estimated that some 300 PhDs are granted world-wide each year based on work done at CERN (the total for the whole field is perhaps double this), and that at least half of these PhDs end up working in industry or commerce, where their experience in working on very high-tech projects in large multinational teams at CERN and other accelerator laboratories is greatly appreciated.

In addition, there is evidence that basic science (in the case of physics[16)], particularly astronomy and particle physics, with buzz words such as black holes and quarks) plays an important role in exciting the interest of young children in science and technology. This is extremely important, although the effect is hard to quantify.

## 4. <u>Why Governments must support Basic Science</u>

Funding of basic science is important for society as a whole, but is not in the interest of any individual investor. Those who make fundamental discoveries generally do not reap the benefits – the laws of nature cannot be protected and the applications are too long-term and unpredictable – and the cultural and educational benefits do not generate direct profits.

Newton's heirs (if he had had any) would be rich if it had been possible to patent the calculus and they received a royalty whenever it was used, but one cannot patent laws of mathematics.

Few scientists have the foresight of Faraday who, in reply to Gladstone's question "What use is electricity?" replied "One day Sir you may tax it". More typical is the remark of Rutherford, the discoverer of the nucleus, who as late as the mid-1930s stated that "Anyone who expects a source of power from the transformation of atoms is talking moonshine".

Quantum mechanics led to modern electronics and lasers, but even with the benefit of hind-sight, investment in the research which led to quantum mechanics would not have been a good commercial investment; the underlying knowledge could not have been protected, the time-lag was too long and the results too unpredictable.

So investment in basic science is not of interest for any individual enterprise, but it is nevertheless very important for society as a whole, i.e. basic science is what economists call a 'public good'. Public goods are items such as lighthouses and defence which are expensive to produce, but once produced are essentially automatically available to all even if they are unwilling to pay[6]. Such items are generally only likely to be supported collectively by governments.

---

[6] While the results of basic scientific research are generally freely available, highly trained people are needed to assimilate scientific publications and exploit scientific findings. In this sense the results of basic science are not a "free public good". Nevertheless, I consider that the overall benefit (research outputs, spin-offs, the basic training that is needed to exploit the results etc.) are a public good.

Governments should therefore support basic science, on the basis of the benefits of the directly acquired knowledge, the spin-offs and the training, as well as cultural grounds. Whenever profit is easily foreseeable, industry will invest and governments can generally stay away, although they can play some role e.g. by encouraging contacts and collaboration between industry and universities. Much of applied research is therefore the responsibility of industry. However, the situation is not entirely clear cut, since whether applied research will lead to direct profits is not always predictable, e.g. research on heart disease could lead to patentable drugs, or to the need for a better diet and more exercise. Furthermore, public funding of applied research on topics such as the environment or issues affecting transport policy is obviously necessary.

This analysis leads to the questions

i) If funding of basic science is not in the interest of any individual, is it in the interest of any individual country?

ii) How to choose what to fund, and at what level?

There are several answers to the first question. First, I consider that developed countries have a responsibility to fund basic science in the interest of society as a whole. Second, an active basic research base sustains and fosters technological development. The role of research in training scientists who go on to work in industry, and in creating networks, is extremely important. Geographical proximity to research centres gives some advantage in exploiting their output, and spin-offs and spin-off companies are most likely to occur locally. It is no accident that Silicon Valley is close to Stanford University or that there is a huge cluster of high-tech companies close to Boston (unfortunately it is not so easy to find such examples in Europe due to the weaker entrepreneurial culture in European universities and research centres).

Nevertheless, we can ask what about Japan?

## 5. <u>Can it be left to Others? Lessons from Japan?</u>

The question whether basic research can be left to others began to be asked in the 1980s, especially in the USA, when many science-based markets were lost to Japan, including very sophisticated areas such as dynamic access random memory, and the question was even raised whether the US semiconductor industry could survive at all. Japan (together with Singapore, Hong Kong, and South Korea) was often quoted as a country that had been very successful economically, and captured science-based markets, but had supported applied research and product development rather than basic science.

As it happens, the US semiconductor industry did not die, and while commentators were predicting its demise, US researchers were creating revolutionary new markets in biotechnology, multimedia, computer software and digital communications, etc. Meanwhile the Japanese economy has, of course, been in relative decline since 1989.

In any case the Japanese Government has no wish to leave basic research to others, and the Science and Technology Basic Plan, published in 1996, foresees a 50% increase in science funding in five years (although the initial rate of increase has not been maintained). Furthermore, earlier arguments based on comparative levels of investment in R and D as a percentage of GDP in the USA and Japan have been re-examined[17]. The data

| | |
|---|---|
| USA | 2.7% (53% private): much in defence, ½% in basic research. |
| Japan | 2.9% (81% private – more than in the USA in absolute terms): little in basic research |

had been used to argue that the larger Japanese investment in applied science and technology was the origin of Japan's economic success in the 1980s. However, the figures for overall non-residential capital investment as a percentage of GDP

| | 1980 | | 1990 |
|---|---|---|---|
| USA | 13% | → | 10% |
| Japan | 15% | → | 19% |

suggest a different conclusion. The factors that fuel economic growth are the supply of labour and capital. Labour markets having been stable, growth might be expected to be proportional to total investment, and therefore on the basis of these figures some one-and-a-half times higher in Japan than in the USA. In fact, however, sustainable growth is estimated to be 3% in Japan compared to 2.5% in the USA.

It therefore seems that the Japanese economy is considerably less efficient than the US economy (similarly in Singapore, for example, growth has been three times that in the USA, but investment has been four or five times as large). Reversing the traditional arguments, it has even been suggested[17] that the relative inefficiency of the Japanese economy is due to the facts that there is less emphasis on basic research, and that the universities in Japan are weaker than in the USA!

This argument is not particularly convincing (many other macro-economic factors are involved, not least the fact that Japan has a national bank that has even outdone the Bundesbank in refusing to reflate during a recession). However, the case of Japan provides no evidence to support the alternative hypothesis that reducing public support for universities or de-emphasising basic research would be a wise economic policy.

## 6. What Science to Fund

I have argued that economic, as well as cultural, considerations lead to the conclusion that public funding should be primarily directed to basic, rather than applied, science. If however we appeal to economic arguments in this way, we cannot object to their use in discussions of the partition of funding between different areas of basic science. The problem is that "both forecasting and innovation are highly stochastic processes, so that the probability of correctly forecasting an innovation, being the product of two low probabilities, is, in theory, close to zero."

If Rutherford, who discovered the nucleus, could not foresee nuclear power, could a government committee do better? Who could have foreseen warm superconductors, fullereenes, or the World Wide Web? Earlier I suggested that Faraday might have foreseen the applications of electricity but in 1867, nine years after Faraday's death, a meeting of British scientists pronounced that "Although we cannot say what remains to be invented, we can say that there seems to be no reason to believe that electricity will be used as a practical mode of power". In a similar vein, it is well known that Thomas Watson, the creator of IBM, said in 1947 that a single computer "could solve all the important scientific problems of the world involving scientific calculations" but that he did not foresee other uses for computers.

This unpredictability, which I have argued is one reason that it is up to governments to fund basic science in the first place, also means that in practice it is probably impossible, and very possibly dangerous, to try to distribute funding for basic science on the basis of perceived economic utility. The traditional criteria of scientific excellence, and the excellence of the people involved, are probably as good as any, and in my opinion these are the criteria that should continue to be used – after all money is more abundant than brains even in this cost-conscious era.

The fact that results of basic research are unpredictable does not mean that economic incentives to find solutions to specific applied problems are futile. 19th century scientists sought methods for artificial fixation of nitrogen, but failed until the First World War deprived Germany of fertilisers, where upon a solution was quickly found. US science, technology and money met the political imperative to put a man on the moon before 1970. But it is important to understand when such incentives are likely to be effective and when they are not. President Nixon launched a battle against cancer, modelled explicitly on the success of the space programme, but it failed. The reason is clear enough. The physical principles involved in putting men on the moon were well understood before the space programme began, while our knowledge of the biological principles underlying the growth and mutation of cells is still limited.

This brings me to the funding of applied research. I have argued that, generally, governments should keep 'away from the market', and fund areas that are 'public goods' because the returns are long-term, or not commercial, e.g. research on the environment or traffic control. Near market work can and should be left mainly to industry, which agrees according to J. Baruch on whose recent article[18)] the following paragraph is based.

Big companies such as 3M, IBM, Siemens, Ford, etc. want to innovate with current technologies that can be priced and predicted accurately, and do not want the help of academics which would only force them to share the profits. Nor are academics generally interested in such collaboration. The exceptions are academics wanting to innovate with available technologies into order to develop new instruments for their research (a category which includes particle physicists). Here there is a considerable mutual benefit and a considerable synergy between technological innovation for profit and technological innovation for research. Indeed, according to Baruch "The people who have most to offer [to industry] are the dedicated research scientists, not the academic technologists or engineers, who do not wish to be distracted from their research in order to help solve common place technological problems".

There was a time when governments were, as advocated here, generally prepared to direct funding primarily to basic science on the basis of scientific excellence. In the UK, for example, the 1978 OECD Science and Technology Outlook found that "objectives for science and technology are not centrally defined ... it is considered that priorities in fundamental research are best determined by the scientists themselves...". This has changed. In the UK Government's 1993 White Paper on Science and Technology, which was based on the premise that science and technology should be harnessed for wealth creation, it was proposed to set priorities by a "technology foresight" programme. The mission was "to ensure that Government expenditure on science and technology is targeted to make the maximum contribution to our national economic performance and 'quality of life'". This might seem no more dangerous, if no more useful, than deciding only to invest in shares that are about to increase in price. In fact, however, although the resulting foresight reviews have had some positive results, the results are being used in ways that threaten basic science.

Such foresight reviews have been undertaken in other countries. First Japan in 1970, then France, Sweden, the Netherlands and Australia, which were then followed by an initially sceptical UK. No doubt others will follow, so it is worth saying something about them (see ref. 19 for a review of various foresight exercises).

Typically, the Foresight Process is that:
1. A 'short list' of important enabling sciences/ technologies is developed by some means
2. 'Experts' investigate the technologies on the list
3. Multi-disciplinary, multi-sectoral 'groups' discuss the results of the investigation
4. Reports of the groups' discussions are presented to decision makers.

For example, the recent UK 'Technology Foresight Programme', which was designed to look ahead 10–20 years at markets and technology, set up Foresight Panels on the following topics:

- Agriculture, natural resources and environment
- Manufacturing, Production and Business Processes
- Defence & Aerospace
- Materials
- Chemicals
- Construction
- Financial Services
- Food and Drink
- Health and Life Sciences
- Energy
- Transport
- Communications
- Leisure, Education
- IT and Electronics
- Retail and Distribution.

The output was 360 recommendations, with the following six overarching themes:

- Communications and computing power
- New organisms, products and processes
- Advances in materials science, engineering and technology
- Getting production processes and services right
- Need for a cleaner, more sustainable world
- Social trends – demographics and greater public acceptance of new technology.

Under these themes, 27 generic priorities were identified for development by the scientific and industrial communities in partnership. The report also identified five broad infrastructural priorities:

- Knowledge and skills base
- Basic research excellence
- Communications infrastructure
- Long-term finance
- Continuous updating of policy and regulatory frameworks.

It seems to be generally agreed that the process served a very valuable role in bringing together people from industry, government and academia. Furthermore, the results are probably useful in identifying potential technological growth points on the time scale of interest to industry. However, for basic science there is a grave danger that the results will be used as a basis for 'planning to avoid failure', and will unduly influence choices in funding.

Indeed this seems to have already happened, and British Research Councils are now required to consider, as one criterion, whether a research application may serve the priorities of foresight, although this was not originally intended. Such a criterion would clearly have prevented Thomson from discovering the electron!

## 6. Concluding Remarks

I have argued that:

- Basic science is very important, culturally and economically.

- Basic science should be supported by governments, as their first priority relative to funding of applied research, and developed countries should not leave it to others.

- Attempts to "direct" research in basic science on the basis of economic objectives are generally futile, and could be counter productive.

From 1945 to the 1980s the attitude to funding basic science was generally favourable in most industrial nations[7]. In this period, there was wide acceptance of the arguments put forward in a celebrated report published in 1945 by a group led by Vannevar Bush, the US presidential Science Adviser, entitled "Science – The Endless Frontier". This report argued that money spent on basic research would, sooner or later, contribute to wealth, health and national security, and that one should not worry too much about exactly what form these benefits might take, and when they might occur. This view prevailed through the 1960s and public funding for basic research grew appreciably in real terms year by year. It must however be admitted, I believe, that in the US at least in the 1950s, there was a tacit understanding that if governments kept university scientists happy by funding their research, those scientists would be available to help in the case of war, as had happened during the Second World War (the Reagan administration tried unsuccessfully to cash this tacit cheque when seeking support for the star wars initiative).

However, the increase of science funding came to an end as public expenditure came under strain and there were greater demands for public accountability. The UK was one of the first to experience such pressures in the second half of the 1970s. The Netherlands was another early case, although there the reasons were that it was felt that there should be more emphasis on science producing social benefits. The model endured longest in Germany and the United States, only breaking down around 1990. In the German case, this was because of the unexpectedly high cost of unification. In the US, it was due to the growth of the deficit in the federal budget together with a belief that Japanese experience showed that the underlying philosophy was flawed.

Now, in virtually all OECD countries, a new social contract for science seems to be emerging. This is exemplified by the UK's white paper, referred to above, and the foresight exercises, which imply that governments will invest in basic research only if

---

[7] Parts of the following three paragraphs are almost direct quotations from ref. 4

it can be shown that it is likely to generate rather direct and specific benefits in the form of wealth creation and improvements of the quality of life.

I have argued that this is a bad policy. The demand that basic science should only be funded if the generation of specific benefits can be anticipated is misguided, and may actually be economically counterproductive. However, the tide shows no sign of turning, as indicated by the following quotation from an article published in Research Europe on 5th June of this year:

*"When the heads of Germany's biggest research organizations took the unprecedented step in January of writing an open letter to the Federal Research Minister virtually calling upon him to do a U-turn, it was not clear what the impact would be. Would Jürgen Rüttgers press ahead with plans to restrict funding for basic research and channel more money into research targeted on economic priorities, or would he heed the call of Germany's research community and back off? Now the outcome is clear. Rüttgers has not changed course one bit to please the Deutsche Forschungsgemeinschaft and its scientific allies".*

We must not give up the defence of basic science, however. In the wise words of 'Science – The Endless Frontier': "Under pressure for immediate results, and unless deliberate policies are set up to guard against it, applied science invariably drives out pure." If, as I do, you believe passionately in the value of pure science, be on guard.

**Acknowledgements**

I am grateful to Paul David, John Ellis and John Mulvey for comments, and to John Kay with whom I wrote reference 1) on which parts of this paper are based.

**References**


1) Science Policy and Public Spending, J.A. Kay & C.H. Llewellyn Smith, Fiscal Studies, Vol. 6, No. 3, p. 14, 1985.

2) The Economic Value of Basic Science, J.A. Kay & C.H. Llewellyn Smith, Oxford Magazine, February 1986.

3) What's the Use of Physics?, C.H. Llewellyn Smith, Current Science, Vol. 6, No. 3, p. 142, 1983.

4) The Relationship Between Publicly Funded Basic Research and Economic Performance: A SPRU Review (prepared for H.M. Treasury), B. Martin et al, Science Policy Research Unit, University of Susses, April 1996.

5) The Economic Laws of Scientific Research, T. Kealey, Macmillan Press, London, 1996.

6) For responses to Kealey's views see K. Parit, New Scientist, p. 32, 2 August 1996, and P. David, Research Policy 26 (2), 229, 1997.

7) G. Holton, H. Chang and E. Jarkowitz, American Scientists 84, 364, 1996.

8) Quoted on p. 198 of "The Life of Sir J.J. Thomson", Lord Rayleigh, Cambridge University Press, 1942.

9) H.G.B. Casimir, Contribution to Symposium on Technology and World Trade, US Department of Commerce, 16 November 1966.

10) Academic Research and Industrial Innovation, E. Mansfield, Research Policy 20, 1, 1991.

11) CERN Yellow Report CERN/75–6, H. Schmied et al. See also IEEE Trans. Eng. Mngt., EM–24, 125, 1977.

12) Results of Attempts to Quantify the Secondary Economic Effects Generated by Big Research Centres, H. Schmied IEEE Trans. Eng. Mngt., EP–29, 4, 1982.

13) CERN Yellow Report CERN/84–14, M. Bianchi-Streit et al (summarized in Czech J. Phys. B38, 23, 1988).

14) P. Brendle et al, Les Effets Economiques Induits de l'ESA, ESA Contract Report, 1980.

15) J. Shaeher et al, Study of the Economic Effects of European Space Expenditure, ESA Contract Report 1988.

16) What Attracts Students Towards Physics? P.P. Kalmus, Phys. Bull. 36, 168, 1985, and 1995 PPARC Survey of New Physics Undergraduates.

17) An Economic Case for Basic Research, E. Wong, Nature 381, 187, 1996.

18) Why Should Companies – Large or Small – Work with the Universities? J. Baruch, Physics in Business 14, 4, 1997.

19) Setting Research Priorities: Future Scenarios for the R&D Portfolio, Proceedings of a Conference held in Washington D.C., June 1995, sponsored by the US Department of Energy, Sandia National Labs and the Office of Science and Technology Policy, ed. J. Glicken, Energy Policy and Planning Department, Sandia Labs.