IMPACT OF BASIC SCIENCE ON ECONOMIC DEVELOPMENT

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ABSTRACT

In this presentation, the role of basic science in the economic prosperity is emphasized. A number of examples from the past are given where a discovery in fundamental science has led to a major economic development. Even the industrial revolution of the 17th century had its origin in basic science. Funding basic science should be the responsibility of the Government and not that of an individual or a group. The benefits of basic science are long-term and unpredictable, so in general one cannot expect a return in the short-term.

INTRODUCTION

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 the 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 defense of the US?"* replied *"Nothing, but it will make it worth defending".* A number of scientists would like to defend the basic sciences without any practical applications, at least in the beginning; 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 are 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."*

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. The reason we have practical computers today, 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 certain kinds of science, which yield benefits that are general, rather than specific to individual products, and hence generate economic returns which cannot be captured by any single company or entrepreneur. People or organizations that have no commercial interest in the results consequently fund most pure research, and the continuation of this kind of funding is essential for further advance.

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 indeed happened, it is also easy to find examples of advances in

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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 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 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; thus:

Basic science - motivated by curiosity
Applied science - designed to answer specific questions.

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:

"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 improved methods 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.

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:

"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 electronics 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 even ask whether induction coils in motorcars 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 emphasized 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.

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

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. The well-known economist John Kay has 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 20 billion pounds, or some 40 billion pounds today.

3. A much cited study by Mansfield 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 taken with a large pinch of salt. Indeed, given the very non-linear relation between research and final products, quantitative measurement is clearly impossible.

CONCLUDING REMARKS

In this paper it was argued that:

- Basic science is very important, culturally and economically.
- Governments should support basic science 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 favorable in most industrial nations. 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 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, then those scientists would be available to help in the case of war, as had happened during the Second World War.