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FUNDAMENTAL RESEARCH, APPLIED RESEARCH AND ECONOMIC GROWTH

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Abstract

The author deals with the complex relationships which can, or should, exist between fundamental and applied research, and discusses the ways in which healthy relationships can contribute significantly to a Nation's economic growth. The author concludes that the traditional classification of research as either fundamental <u>or</u> applied may interfere with the development of the healthy relationships required. He suggests an alternative model, based upon distinctions between <u>intrinsic</u> and <u>extrinsic</u> motivation of the research activity.

It gives me great pleasure to be in Cambridge again and to have this opportunity to talk to you at the beginning of the decade of the 80's, a decade that I am confident will be recognised by future historians as a time of great changes that will be as fundamental and decisive as the changes that occurred during the first Industrial Revolution.

As I look around this room, I am also conscious that I, and the Procter & Gamble Company I represent, are very much "Johnnie come latelies" to the paper industry. In fact, to many of the British people in the audience it may come as something of a surprise that the producer of products like `Ariel', `Fairy Liquid' and `Head & Shoulders', is in the paper business at all. We did, however, acquire our first paper machine in 1957 and have since expanded our activities until today we operate five paper-making plants and earlier this year commissioned our fifth major pulp facility on the Flint River in Georgia, giving us an annual captive capacity of over one million tons of pulp.

Obviously, for an industry that started in China at the beginning of the second century A.D., our involvement of a mere 25 years makes me very conscious of being the new boy on the block.

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Let me begin with a few words about economic growth.

Under the respective leaderships of Prime Minister Thatcher and President Reagan, both Britain and the United States have adopted new government strategies with the avowed intention of providing climates more conducive to individual and corporate initiative.

But governments do not, in fact cannot, create healthy economies. Governments can only create a <u>climate</u> in which the aggregate of the decisions and actions of individuals and companies can produce a vigorous and healthy economy. Whether we as individuals, and especially those of us concerned with the development and utilisation of technology, respond to the opportunity which has been established, will determine the quality of life in both countries for the next century.

Whatever strategies governments follow, it is crystal clear that we can achieve our basic societal goals, such as adequate job opportunities, a healthy environment, and a reduction in poverty, crime and disease, only when we achieve a significantly high rate of real economic growth. It is now recognised by most economists that in the two remaining decades of the Twentieth Century the major factors necessary for such growth are the development of market-relevant technology, and the formation and use of capital to commercialise that new technology.

In net, if we are going to succeed in dealing with unemployment and improving the standard of living of people in Great Britain, in America, or elsewhere in the world, we must accelerate the development of the new and improved products and services that people want, and the resources and systems must be brought into existence to commercialise this accelerating rate of technical innovation.

The rate of development of new and improved products and services is essentially determined by the effectiveness and efficiency of the total national research and development effort. How effective are we? Are we adequately trained and organised to carry out this fundamental responsibility for the future of our societies?

As a guest here, I am not going to presume to answer such a question for the U.K. It is over twelve years since I lived in Europe and was in a position to study the British situation in any detail. However, as I discuss the U.S. situation, you may identify useful parallels.

I plan to begin this discussion with an examination of several factors upon which the capability for technical innovation depends.

Firstly, technical resources and skills which match the needs of the present situation must be available.

Secondly, we must achieve appropriate balances amongst the following:

a) the creation and utilisation of knowledge.

b) scientific, economic and political project motivation

c) short and long-term objectives

Let us first examine the availability of technical resources and skills.

In any society, the pool of qualified scientists, engineers and technologists is a limited resource. The number of students able and willing to enter a scientific career is strictly limited and, in the U.S., has for some time been in decline as a percentage of the total student population.

In addition, although theoretically it might be possible to increase somewhat the number of technically orientated students, it is doubtful if such an increase would result in a <u>significant</u> increase in the rate of technical innovation. We are almost certainly already attracting most of those who are going to be truly outstanding in either science or technology.

It is not only student enrolment in the aggregate but also its distribution among the various scientific and engineering disciplines which determines a nation's inherent ability to conceptualise, develop, and commercialise technology. Given a limited personnel resource, what percentage should be engineers or scientists, civil engineers or electronic engineers, physicists or geneticists? And at least as importantly, how do we achieve the necessary balance between one discipline and another?

I'm going to come back to this question of academic balance later in my talk, but let's move on for a moment to the second question, that of the various necessary balances.

For the U.S., these have to be considered in light of the shifts which have occurred since World War II.

These shifts are of fundamental importance because they have had a serious impact on the ability of our limited resources to help meet the nation's economic and other social goals.

a. The balance between the creation of knowledge and its utilisation has favoured new knowledge creation at the expense of utilisation. As an indication of this trend, the ratio of U.S. engineering first degrees to science doctorates has declined from over 10:1 in 1926-1950 to under 5:1 for the 1951-1975 period. Similarly, the ratio of Applied Research and the ratio of Development to Basic Research have been in decline. In 1953, each dollar of Basic Research was accompanied by $2.90 in Applied Research and $7.72 in Development; by 1979, these levels had fallen to $1.75 and $4.95, respectively(1).
   Incidentally, in recent years Japan has been graduating more engineers than the U.S., despite having a population barely half the size. Of course, they don't graduate as many scientists(2).

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b. The balance among scientific, economic and political values and objectives as motivators of scientific and technological development has shifted away from the scientific and economic, and toward the political.

To a large degree, this reflects the influence of Federal funding of R&D in universities. Prior to World War II, the Federal government was not a major source of university R&D funding except in agriculture. During the war this changed and, by 1953, 54% of university research was funded by the Federal government. The Federal share grew steadily to a peak of 74% in 1966, and today stands at about 68%[1].

Describing the consequences of this condition, Professor Gilpin, in his 1975 report to the Joint Economic Committee of Congress, said:

> "As in the case of government financing in general, there were problems; the emphasis on particular areas and the neglect of others caused serious distortions and imbalances in the overall national basic and applied research effort. Government overfinanced 'big technology' and 'big science' such as aeronautics, particle accelerators and electronics to the detriment of technologies and sciences of equal or greater relevance to social welfare and civilian industry".[3]

c. The balance between speculative, longer term but potentially more valuable research and shorter term but frequently less rewarding research, has shifted in favour of the latter. This shift is difficult to quantify and is challenged by some in the light of notable exceptions in computer science and biochemistry. Nevertheless, there is considerable evidence that the shift has occurred. For example, a National Science Board 1975 survey of research administrators from industry, universities, federal laboratories and non-profit organisations concluded:

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> "All of the research sectors reported that they felt a pressure to do short-term targeted, applied research rather than long-term, basic research."[4]

Returning for a moment to the question of academic balance in the education of students. To a great extent, the fields in which students are trained, especially at the postgraduate levels, are determined by the availability of research fellowships. Hence, imbalances in university research produce imbalances in university education.

F. Karl Willenbrock, Dean of SMU's School of Engineering and Applied Science, describes the situation.

> "The amount of funding available in the various technical fields has been determined primarily by the programs and missions of the Federal funding agencies and have not been uniform over the traditional fields of engineering. As engineering faculty members and administrators have responded to the availability of external funds, some technical areas have undergone rapid growth and change within the schools while others have remained relatively static."[5]

Many of these conditions have occurred not as the result of deliberate policy decisions in government, the universities or industry, but to a large extent as the indirect and unintended result of decisions, many apparently unrelated to science and technology, in all three sectors.

To be blunt: the processes of technology development and technical innovation weren't well understood, and as a result, serious mistakes were made.

Whether we succeed or not in the next decade will be determined by how well we <u>now</u> understand these processes and by our willingness and ability to respond to that understanding.

What is this research and development process? Let me offer you three propositions:

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1. The output of successful R&D is knowledge, not knowledge for its own sake, but knowledge that now or later will be used to benefit society, from the provision of services and products to improving the cultural pursuits still necessary for a full and satisfying life. In the case of new and improved products and services, the knowledge is often incorporated in formulae, specifications, patent applications, safety clearances, etc.,

2. Since the output is knowledge, the R&D process is one of learning, that is, of creating new knowledge.

3. To be of value to society and, in the industrial case, of value to our companies, new knowledge must be relevant to society's needs and be used to create change.

In net, successful R&D involves learning and the application of the resulting knowledge to create societal change.

To some, this process is seen as a linear series of events, an orderly progression, from basic research, through applied research, to development. However, as those of us in industry, who have the job of producing new products year in and year out, are acutely and sometimes painfully aware, the orderly process described by the linear model bears little resemblance to the actual cycling and recycling, false starts and redefinitions of hypotheses and objectives that characterise our world. The reason is that we are essentially involved in the process of learning, and learning is not a linear process.

Learning is in fact a cyclic process, including a divergent phase, when data are gathered and alternatives generated: an assimilation phase, when theories are developed and hypotheses formulated: a convergent phase, when hypotheses and theories are selected for evaluation: and finally an executive phase, during which the hypotheses and theories are put to the test, producing new data to provide the starting point for a repeat of the whole cycle until, finally, the objective is achieved.

Both successful Basic Research and Applied Research follow this cyclical process.

But let me just step back a minute now that I´ve introduced the words "basic" and "applied". I wish I knew who was responsible for introducing these two words into the lexicon of science, together with the untold mischief they have caused.

In the U.S., the National Science Foundation has blessed these words with official definitions:

> Basic Research: Original investigations for the advancement of scientific knowledge not having specific commercial objectives, although such investigations may be in fields of present or potential interest to the reporting company.
>
> (The term "fundamental research," sometimes used synonymously with "basic research," is slightly less subject to producing the mischief I will shortly describe.)
>
> Applied Research: Investigations directed to the discovery of new scientific knowledge having specific commercial objectives with respect to products or processes. This definition differs from that of basic research chiefly in terms of the objectives of the reporting company.[6]

Unfortunately, in the last half century we also have adopted additional hidden meanings that we apply to these words. Just what are these hidden meanings?

> Basic research (the hidden meaning tells us) is honourable, altruistic, long-term, technically sophisticated, universal and of great value to society; in sum, basic research is entirely meretricious.

> Applied research, on the other hand, is mercenary, self-serving, short-range, technically second-rate, narrow and of limited value to society; in sum, applied research is decidedly inferior.

Let us examine these impressions for a moment from the perspective of history.

The investigations conducted by Benjamin Franklin during which he risked his neck by a combination of a kite, a string, a key, and a natural phenomenon called lightning, I would certainly classify as basic; he was clearly motivated by a desire to advance scientific knowledge. However, the work was short term, hardly scientifically sophisticated, but certainly of enormous value to those of us who came later.

By contrast, Franklin's research work in developing the lightning rod was clearly applied, as was the work leading to the invention of the incandescent light bulb by Sir Joseph Wilson Swan in 1860, and subsequently by Thomas Edison in 1879.

Incidentally, neither Swan's work nor that of Edison could reasonably be described as short term, technically second-rate, nor of limited value to society, despite its classification as applied.

I would like to suggest that we reject the terms "basic" and consider alternative dimensions for conceptualising the R&D process.

As scientists, engineers or managers of research, I submit that the most important decisions we make are the decisions to undertake specific research projects, that is, to engage in particular learning processes. It is therefore appropriate to consider the <u>criteria</u> we use in making these crucial decisions. For simplicity, I suggest that these criteria can be classified as either <u>intrinsic</u> or <u>extrinsic</u> to the science and technology involved. I further submit that this classification is more useful than the traditional dichotomy between "basic" and "applied" research.

The intrinsic value of the learning to the science or area of technology involved is the principle criterion for selection, and monitoring progress, for "traditional" basic research projects.

Extrinsic factors are those not directly related to progress in the science and technology involved. While eventual commercial opportunity is one extrinsic factor, it is by no means the only one. Other extrinsic values might include the impact of the project on other branches of knowledge; for example, research into radioactivity has provided extrinsic value in many other scientific areas from medicine to archaeology.

In fact, there are very few research programmes in either universities or industry that are motivated purely by the intrinsic merits of the science. To one degree or another, most research programmes are consciously or unconsciously responding to extrinsic factors, from the education of students to the development of new technology.

The history of the paper and cellulose industry is replete with examples of research and learning in response to extrinsic factors.

The development of paper machines led to a demand for fibres that could no longer be satisfied from the traditional sources, rags and used rope, which was the extrinsic factor that eventually influenced the research into and the development of mechanical wood pulping.

Military need was clearly an extrinsic factor in the identification, stabilisation, and safer processing of the first cellulose derivative, nitrocellulose.

Let us now move back to the U.S. I would suggest it is now time to knock down the institutional and other barriers created by the troublesome concepts of basic and applied research. We still need research which is primarily motivated by its intrinsic value to the discipline involved, but for much of our research and education we need to restore the historical interplay of extrinsic as well as intrinsic values.

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In this context I should note that the newly-appointed Director of NSF, John B. Slaughter, finds the "basic/applied" dichotomy no longer useful as a governing criterion in organising the foundation. He has stated:

> "I want to stress at the outset that the bulk of the work in this `applied research´ category borders on being as fundamental as anything we have so far discussed. It involves only the additional nuance that it is directly responsive to a felt or expressed need of society; it answers a question that has already been asked. Its place in NSF requires no apology. Indeed, so interconnected do we see the so-called basic and applied sciences that from now on their support at NSF is to be handled in tandem by the research directorates. No longer will particular research programs be separated from their disciplinary mainstreams just because they can be construed as addressing so-called real-world questions."(7)

Donald Kennedy, President of Stanford University, reflects the same sentiments in his description of how capable adminstrators merge quality and utility:

> "Frequently, in their efforts to match the best work to their agency´s mission, they engage in personal bargaining `Look,´ their argument might run, `we are interested in a solution to this practical problem in navigation and you do sensory perception. There are some interesting problems in the orientation of birds and bats that you may not have thought about.´ Or: `Is there any reason why you couldn´t do this work on an economically significant pest instead of the insect you may have chosen?´ That kind of matching, based on intimate knowledge of the utilitarian need, a clear understanding of the researcher´s interest, and respect for both, produces remarkable results."(8)

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It is certain that in order to achieve the degree of change needed we will need new institutional and organisational mechanisms: for example, the problems with the scientific peer review process are being increasingly recognised.

> Scientific peer review is an excellent method for establishing the intrinsic merit of research proposals, especially those proposals which follow well-established lines of investigation within a single discipline. Peer review is more difficult to apply to proposals which depart sharply from established lines of investigation, or which involve several quite different disciplines. Moreover, peer review was not intended to be used to establish the extrinsic merit of research proposals; whether it can be adapted to this purpose remains to be discovered.

A number of exciting initiatives is already underway, including new mechanisms for university/industry interaction in the identification of long-range fundamental projects, with both extrinsic and intrinsic facets.

It's always interesting to see how other countries organise these matters. In Japan, the visionary documents prepared by MITI provide an extrinsic framework for both their industrial and academic research.

A German initiative that I find particularly intriguing is their use of the Fraunhöfer societies to create an interplay between the market extrinsic and scientific intrinsic factors in research.

In both instances, a mechanism is provided not only for considering extrinsic factors at the project level but also in the much more difficult area of trying to achieve extrinsic as well as intrinsic balance of the resources committed to different areas of science and technology.

To summarise, the challenge facing the scientific and technical community at both the national and corporate level are immense. Because our people resources are limited, we can only succeed if we can significantly increase our effectiveness and efficiency. One way to do this is to recognise that R&D is a learning process to be managed in such a way as to maximise extrinsic as well as intrinsic factor involvement.

I would like to close with a few words about the paper industry.

We are clearly entering a period of revolutionary change. The end uses of paper are being challenged as never before. As the principal means of communication via the written word, we are being challenged by the electronic revolution: as the principal means of packaging, we are being challenged by the world of polymers and increasing concern about distribution costs and environmentally acceptable disposal.

The key elements of our processing are being challenged. We're all conscious of the increasing energy costs we face, particularly for electricity. What is going to happen to timber costs and availability as it is increasingly recognised to be the cheap source of energy that in fact it is? With the increased cost of the capital, can we still afford to be as capital intensive as our present pulp and paper mills?

As R&D professionals, we <u>can</u> and <u>must</u> meet these challenges. It is easy to make excuses for inaction. We can argue that huge capital investment requirements limit our ability to implement technical innovations. We can assert that our companies don't give us enough money for R&D (in the U.S., R&D spending as a ratio to sales is only about one-third that for manufacturing as a whole).

We can complain that our industry is too prosaic, that the glamour fields, like microprocessors and genetic engineering, are getting all the attention and funds.

But to me as an R&D manager there is only one answer.

We must accelerate the learning process, accepting these challenges as extrinsic factors to be addressed as we select and manage our projects. Thus, we will turn excuses for inaction into rallying points for progress. If capital is a problem, invent new, less capital-intensive, methods of production. If R&D funds are limited, use the limitation as a basis for sharpening our skills in selecting and managing priorities and projects. and

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let's make sure that we help to <u>create</u>, as well as to utilise, opportunities from the field of genetic engineering, just as today we are utilising the benefits of microprocessor technology.

Look for other opportunites. For example, in the U.S., declining Federal funding of university science offers an extraordinary opportunity for academic and industrial scientists to re-establish meaningful co-operative projects that address the intrinsic and extrinsic objectives of both sectors.

Now for the commercial: some of you are probably aware that Procter & Gamble manufactures and markets in many parts of the world a disposable diaper or nappy called Pampers. One of the more obvious extrinsic factors to be considered as we develop improved versions of this product is its need to absorb and retain liquids, sometimes lots of liquids.

At first sight, it appears as though this liquid is retained by an absorbant core of wood fibres, but you know that this isn't really true. The bulk of the liquid is retained in the void space between the fibres. The fibres merely serve to hold these voids spaces together. The useful raw material, the voids, is free, but the fibre that connects them together costs several hundred dollars a ton.

Unfortunately, we don't yet know how to get the voids to hold the liquid without the expensive fibres.

Similarly. we don't know how to provide societal value from research in the voids in isolation. The connecting communication links along which the extrinsic factors flow is the key to its ultimate value to society.

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