Science Policy: An Investment Perspective

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SCIENCE POLICY: AN INVESTMENT PERSPECTIVE

BY

J. STANLEY METCALFE AND LUKE GEORGHIOU

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In memory of John Stanley Metcalfe
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SCIENCE POLICY:
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J. Stanley Metcalfe
Professor of Economics and Director, PREST

Dr. L. Georghiou
Senior Research Fellow, PREST

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FOREWORD

Science Policy: An Investment Perspective is an unusual contribution to the MIoIR Working Paper series, not least because it was written over forty years ago but is published here for the first time. It is presented here as a tribute to the late Professor Stan Metcalfe to coincide with the Memorial Event being held on March 30th, 2026, to celebrate the immense contribution of our sorely missed colleague.

As well as demonstrating Stan's unique ability to cover the spectrum from economic theory to policy advice in the 1980s, readers may find messages that still carry pertinence for science and technology policy today. There is an early recognition of mission-oriented research. The framework speaks to current prioritisation debates about balancing curiosity-driven research and strategically targeted or industrial R&D, current issues in UK and European contexts.

The paper is an essay commissioned as part of a series by the Advisory Board for Research Councils (ABRC), a UK government body that operated from 1972 to 1993, advising the Secretary of State for Education and Science on the allocation of the science budget across the research councils. The remit was open, to contribute to thinking on science policy in a way that would assist the Council. It was an initiative driven by the Board's Chairman, Sir David Phillips, who pioneered a strategic approach to science policy in the United Kingdom.

Stan was very much the driving force in choosing and expounding the focus of the paper. I was privileged to be invited to act as his foil and co-author. Elements of the paper subsequently appeared in published works, for example the time-cost trade-off was applied to discuss the competitive advantage of nations making decisions about investment in strategic technologies and the impetus it provides for international collaboration.[1] Students of Stan's work will recognise the main theme of an investment perspective as a motif that recurred in several subsequent publications. An explicit discussion came in an exploration of the nature of evolutionary competition where he argued that consideration of asymmetric investment behaviour by firms provides a better explanation of their differentiation and propensity to growth than the prevailing view of an explanation confined to differences in unit costs of production.[2]

In the domain of science, technology and innovation policy his contributions included his address to the British Association for the Advancement of Science in 1995 which formed the basis for a paper in the International Journal of Social Economics.[3] In that piece he reviewed recent developments in technology policy through the lens of evolutionary economics, arguing that for national investments in science and technology to achieve an adequate return it is necessary to have effective technology support systems which bridge between industry and the science base. Pushing back against linear model perspectives, he stressed the need for division of labour in the

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[1] L.Georghiou and J.S.Metcalfe (1990) Public science, intellectual property rights and research administration, in J.de la Mothe and L.M.Ducharme, *Science, Technology and Free Trade* London and New York, Pinter, pp. 41-54.

[2] J.S.Metcalfe (2012) Investment in the theory of evolutionary competition. In C.Gehrke, N.Salvadori, I.Steedman and R.Sturn (Eds.) *Classical Political Economy and Modern Theory: Essays in honour of Heinz Kurz* London and New York: Routledge pp.159-181.

[3] J.S.Metcalfe (1997) Science policy and technology policy in a competitive economy, *International Journal of Social Economics, Vol. 24 No. 7/8/9*, 1997, pp. 723-740.

system whereby firms, universities and public research bodies are distinctively different kinds of institution, each adapted to a specific purpose.

Possibly Stan's most influential work, and certainly his most cited, was his 1995 monograph-length chapter The Economic Foundations of Technology Policy.[4] The wide span of this chapter outlines the economics of technology policy from the established equilibrium view and from the evolutionary view which Stan did so much to expound and advance. The themes mentioned here and others come together to form characteristically lucid and comprehensive guidance for the policymaker.

Professor Luke Georghiou

[4] J.S.Metcalfe (1995) The Economic Foundations of Technology Policy: Equilibrium and Evolutionary Perspectives in P.Stoneman (Ed.) Handbook of the Economics of Innovation and Technological Change, Oxford:Blackwell pp.409-512.

In this brief essay we shall consider some policy implications which arise from treating scientific expenditure as an investment producing national economic wealth. We shall seek to clarify this investment view, to link it with the question of selectivity in basic science and with the support of strategic areas of scientific activity. The following paragraphs are little more than a thumbnail sketch of an incredibly complex area, and so the first task is to limit the scope of enquiry. Before doing so it is appropriate to summarize the broad conclusions:

a) An investment framework for science should be judged by its capacity to ask logically consistent questions of science expenditure. It should not be expected to provide precise quantitative answers, rather it should be the basis for non-arbitrary judgements.

b) The investment view identifies certain data requirements for science policy, in particular relating to the productivity of research effort and the likely economic and social impacts of applying research output. It is essential to support an appropriate data gathering process if selectivity is to become a practical policy.

c) Investment comes to nought if it is not exploited effectively in the UK interest. The investigation of organizational structures to secure effective transfer of science to industrial application should be a major concern of science policy. The evaluation and monitoring of established structures should also be a permanent feature of science policy support.

Numerous detailed recommendations which flow from these broad conclusions are dealt with at appropriate places in the text.

1. SCOPE OF THIS ESSAY

We shall assume throughout that the structure of support for UK science will remain unchanged over the foreseeable future. The UGC will continue to fund general support for science in the Universities (albeit, perhaps, on an institutionally selective basis) while the Research Councils will support specific projects and programmes. Applied research will remain, in relative terms, the prerogative of industry and government departments. We shall be concerned in this essay only with Research Council supported science.

We assume that the Research Councils continue to be resource constrained for two reasons: the limits on public expenditure in the UK; and the increasing relative cost of scientific research as the sophistication factor continues to push up the costs of research faster than general inflation. The setting of priorities for science funding will grow more significant over the next decade. It has been suggested that the UK accounts for roughly 5% of world scientific activity. Without a remarkable change of trend in UK economic performance, its world scientific contribution (in volume terms) is certain to decline further.

We make no reference to the manpower[5] benefits of scientific support or the links between teaching and research which are at the basis of the current organization of scientific activity in the Universities.

[5] Editor's note – the term 'manpower' in the paper is gendered language that would not be used today but was still common usage in the 1980s, for example in the name of the executive body of the Department of Employment which was called the Manpower Services Commission (later to be replaced by the Training Agency).

Finally, we continue to assume that government will remain the sole funding agency for the Research Councils. The idea of a market mechanism to support fundamental science is simply fanciful. Fundamental knowledge is not a conventional commodity. Its production is subject to great indivisibilities, the outcome of research is very uncertain, and, most importantly, economic property rights in fundamental knowledge cannot be established or enforced.

2. ACCEPTABILITY OF THE INVESTMENT VIEW

It is clear that the attitudes of scientists towards an economic justification of their activities are not uniform and that this reflects different perspectives on the nature of science.

The traditional interpretation of science presents the researcher engaged in free, disinterested and sceptical enquiry, pursuing knowledge for its own sake within an open community of international scholars. The direction of research is determined by the judgements of individual scientists with the consequence that the accumulation of knowledge is governed by its own internal logic. Science is a culture within which success depends on personal creativity as demonstrated by priority in publication.

Whether this traditional view ever conformed at all closely to reality is a matter for philosophers of science to debate. What is not in doubt is that the exponential growth of science since the 18th century has culminated in a change of form of science (1), recently summarized as the collectivization of science (2). The salient features of this new view we may summarize as follows

a) The dominance of the experimental method has resulted in a growing emphasis on collaborative research by specialist teams sharing equipment and facilities. Science is increasingly a capital-intensive activity.

b) The fragmentation of science, as its progress becomes contingent upon the division of intellectual labour. This has increased barriers to communication, created a greater specificity of human capital within the scientific community, and makes it difficult to compare meaningfully the intrinsic worth of different scientific disciplines.

c) The general dependence on state support of fundamental science. Even if the direction of science remains governed by its internal logic, the rate at which progress can be made in each direction is contingent upon the overall level of funding. Given the different resource needs of different branches of science, it is no longer possible to argue that scientific progress is entirely internally directed.

The product of these changes is reflected in a certain tension in the academic scientific community. On the one hand, the traditional Mertonian norms are held up as exemplars of the research ideal. On the other hand, the resource needs of science result in funding demands which only appear justifiable in terms of the long-term utility of science.

"Fundamental research is crucial to underpinning the nation's long term ability to produce new technology. Out of such research can come great returns."

(ABRC, 1984, p.7)

The applicability of the utilitarian argument is beyond doubt. What is doubtful is whether the full ramifications of this investment view are fully reflected in the conduct of UK science policy.

Prima facie science is productive. That major new industries have their origins in scientific discovery is not in question. However, the foundations of the utilitarian view lie deeper than this. Every productive process entails the transformation of materials and/or energy into more useful forms, and it is such transformations which correspond to adding value, to creating national income. The potential contribution of scientific understanding is thus the entire field of industrial activity, mature technologies as much as sunrise industries.

Despite the scope for a utilitarian science policy, considerable disquiet is often expressed about the ability of the UK to benefit from the scientific knowledge it creates. To a considerable degree scientific knowledge is an international public good, in that countries which make discoveries are not necessarily in the best position to exploit them. The openness of science means that UK expenditure can be of great benefit to the world in general but of negligible benefit to the UK.

The dilemma of UK science policy is how to achieve an economic return in the UK on its investments in science. We shall suggest two prerequisites for achieving that return with greater success:

a) The application of an investment framework of analysis to science decision making. Investment is an economic concept, and an investment basis for selectivity requires an economic comparison of the competing claims on the science budget.

b) An organizational structure for science in which the creation of knowledge is not rigidly separated from the application of knowledge "science creates and industry applies" is not the basis for obtaining a good return on investments in science.

Investments are made in terms of their prospective returns. But if, over a run of years and projects, the anticipated return fails to materialize it will not be surprising to find that the funds for investment are curtailed. Achieving the return to science is a matter of self-interest for the scientific community. Indeed a greater success in the exploitation of science in the UK could well lead to a major shift in the resources publicly and privately devoted to research.

3. THE INVESTMENT FRAMEWORK

It will not be suggested that all scientific activity should fall within the investment view. Clearly applied science falls within its scope automatically. The difficult questions relate to the inclusion of fundamental science, for which numerous motives can be mobilized in its support. Provisionally we will take University Grants Committee supported science outside the net and suggest that a proportion, to be established, of Research Council support should be judged in investment terms. What then is to be appraisal in the investment framework?

The broad objective of science support becomes the creation of national wealth. The mechanism to achieve this objective involves the support of particular programmes of knowledge generation to underpin the development of specified technologies. The ensuing research is strategic research in the sense that clear technological objectives are employed to guide the investment process.

A strategic programme can be expected to have a number of distinguishing characteristics:

a) It will typically involve more than one specific research project;

b) It will require support of scientific work in more than one traditional discipline;

c) It will require the generation of knowledge over an entire spectrum from the fundamental to the applied;

d) It will involve the explicit choice of an organizational structure to generate knowledge efficiently, and to ensure close coupling with industry to achieve the ultimate objectives of economic return in the UK.

Given that science policy makers have before them a range of possible strategic programmes and a limited budget, the questions which now arise concern relevant criteria for choice. To support science as an investment requires the balancing of criteria internal to science against external criteria relating to the economic attributes of competing research programmes.

4. CRITERIA FOR CHOICE

The application of internal criteria for scientific choice is well developed in the UK in the form of the peer review process. However, in times of declining budgets it faces particular problems stemming from the fact that the output of a research programme is a change in knowledge for which there is no cardinal measure. Knowledge is valued in the mind and conventional, consensus valuations as to what is significant may be and often are misleading. To demand clearcut measures of the quantity and quality of knowledge is an illusion, a difficulty which citation methods cannot overcome. Hence the well-known problems of peer review: the difficulty of making meaningful comparisons across sub-disciplines; the alleged conservative bias and the problems of supporting 'new scientists' lacking an established record in a given area. Declining budgets place this system under particular strain, as policy makers are effectively required to redraw the boundary around top quality science.

Should a cardinal measure of the value of different programmes and projects be available it would greatly simplify the logic of scientific choice. The investment perspective has precisely this attribute. It provides a clearcut objective, the increase of national wealth, and a standard of comparison in terms of costs and benefits which permits an unambiguous ranking of projects.

5. CONSUMPTION VS. INVESTMENT

The nature of this approach can be clarified in terms of the economic distinction between consumption and investment. In principle the distinction is straightforward. Consumption activities involve the present outlay of resources for immediate benefits, no less worthy for their immediacy. Viewed in these terms, science would be akin to the performing arts. A leisure and entertainment activity with one peculiarity that the main consumers of its benefits are the practising scientists themselves (3). To repeat, this is perfectly worthy but it is no doubt a particularly weak basis for encouraging the public support of science. It is hardly surprising that the widespread economic benefits attached to investments in science provide a more secure foundation for the utility of scientific research. Now the distinguishing feature of an investment is that current expenditure contributes to future economic benefit and consumption. New knowledge creates future productive opportunities enjoyed by the community in general. To judge a scientific programme in terms of its investment potential simply requires a balancing of the costs of research against the future streams of economic benefits. Two distinct categories of data are thus required:

a) Data on the scientific resources required to generate the requisite knowledge base. Data that is on the productivity of scientific research.

b) Data on the costs of translating new knowledge into products and processes, and on the economic benefits which will be generated by their application.

We treat each of these categories in turn.

5.1. Resources and Scientific Productivity

For this purpose we can view science as a productive process in which scientific resources perform work on the existing knowledge base to produce changes in understanding. The knowledge base is, in economic terms, a free gift from the past and the scientific resources are manpower, equipment and laboratory facilities. For a given knowledge base one can look at the productivity relation between resources and output in two dimensions:

a) For a given time period of research, the output (changes in understanding) will increase with the application of resources but subject to diminishing returns.

b) For a given increment in understanding, the time taken to achieve this will be reduced with the application of resources but again subject to diminishing returns.

Gain in understanding need not be cardinally measurable, and the source of diminishing returns is the given knowledge base. Over time, changes in understanding alter the knowledge base, change the research agenda, and so shift these two relations. To the extent that exploring a particular area of science is akin to exploiting a mineral deposit then, ultimately, the agenda must approach a limit and the costs of advancing knowledge rise sharply. In such a state an area of science could be labelled 'mature'.

The determinants of productivity relations for science should be a central concern of science policy makers. As a first approximation, the two key factors will be the organization of research, and the technology of gathering and processing data. A number of questions on the productivity of science naturally follow.

a) How do the productivity relations vary across different scientific disciplines? How are they related to the time scale of experiments?

b) How do the productivity relations vary over the life of a scientific discipline? Is the notion of a limit to the agenda significant and in what way is it affected by developments in related disciplines?

c) How are the productivity relations affected by the balance between manpower and equipment? Is equipment:manpower substitution the source of the sophistication factor, or is the latter a reflection of approaching maturity and an exhausted agenda?

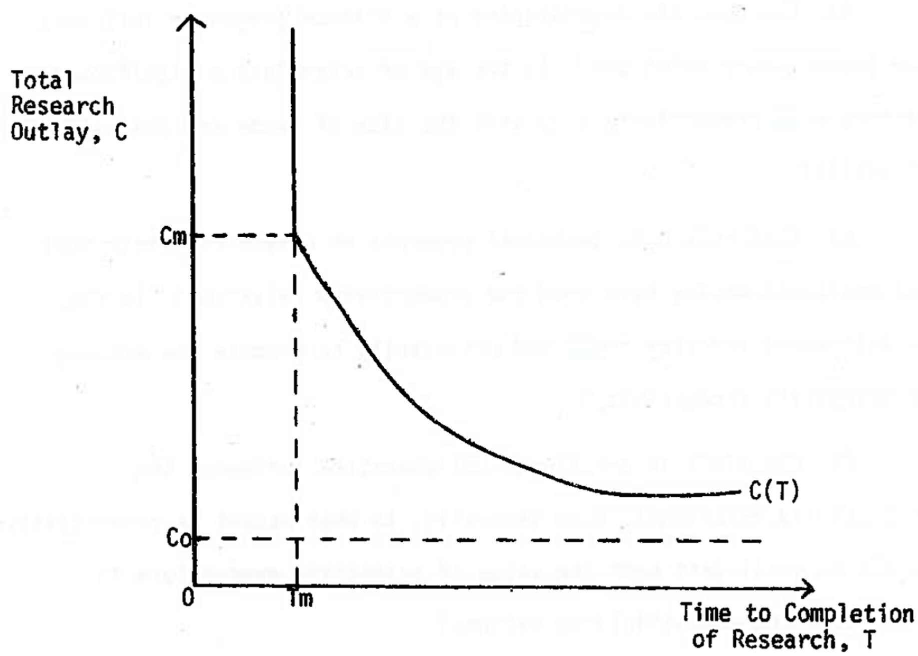
d) How does the organization of a science programme influence the productivity relations? Is the age of scientists a significant factor, does productivity vary with the size of teams and the balance of skills?

e) What role does technical progress in scientific instrument and equipment making have upon the productivity relations? Is the UK instrument industry organized efficiently to promote the advance of scientific productivity?

f) How might international collaboration influence the productivity relations? More generally, to what extent is productivity in the UK contingent upon the value of scientific expenditure in other advanced industrialized nations?

Of the two productivity relations it is the second, the time-cost trade-off which appears most useful in the investment context. For if the programme objective is to create a particular technological capacity and thus a particular underpinning of scientific understanding, the relevant question to ask is when will this be achieved and at what cost? More precisely the question should be what is the best time to acquire this knowledge and technological capacity? While the interests of scientists are in rapid discovery and the establishment of priority, the investment viewpoint indicates quite clearly that today is not necessarily the best time to acquire a given technological capacity.

The following diagram sketches a hypothetical time-cost trade-off (4).



Tm is the minimum time at which the research can be completed and Cm is the associated minimum cost of achieving the minimum time. Reductions in total programme outlay below Cm result in the research time extending into the future at an increasing rate. Co is the threshold level of expenditure, below which the results will never be achieved. The shape and position of the trade-off will depend upon the established knowledge base in the area, the technology of research and the organization of research. Of course, since the research process is uncertain, this trade-off should be formulated in probabilistic terms and relate expected cost to expected research time but such considerations take us beyond the scope of these notes. In summary, it is sufficient to recognize that the time-cost trade-off brings together all the relevant factors determining research productivity. As we shall show below it plays a central role in the selection of strategic research projects.

5.2. THE BENEFITS FROM EXPLOITING RESEARCH

On the benefits of strategic scientific knowledge we can be brief, since the issues entail conventional economic appraisal. The new processes and products which flow from the newly created technological base create economic benefits by reducing the resources required to satisfy human wants: either by increasing productivity directly or by increasing the quality of various products. The benefits will accrue over a number of years and will reflect the rate at which the new products and processes are absorbed into their economic niches. In calculating the benefit stream it is important to take account of the following points:

a) The benefits must be calculated net of the costs of translating knowledge into products and processes. In many cases the acquisition of the knowledge base is the least part of the costs of innovation.

b) Not all of the benefits and costs will be realised in market transactions. Externalities must be valued and included and will be of particular relevance to science programmes concerned with health and environment. Much of the technical apparatus of cost benefit analysis is concerned with the valuation of extra-market costs and benefits.

It is inherent to the investment view that the costs and benefits are generated at different dates in the future, and this means that the effect of timing also affects their value to today's decision makers. If the rate of interest (and thus the cost of capital) is positive then the further into the future an item of benefit lies, the lower will be its present value. That is, future benefits and costs have to be discounted. This is not a trivial matter for science policy. The effect of discounting becomes greater in proportionate terms the further into the future one goes, indeed at interest rates of 10% and above no benefits received beyond a twenty-year horizon will be given a significant economic weight. Justifying science in terms of its future utility is not a straightforward matter. Of course, the choice of interest rate and the incorporation of risk premia to discount the future are not always clearcut. However, it is generally agreed that some discounting element is desirable to protect the interests of the present population. The force of this consideration should not be underestimated. It is not sufficient to justify current expenditure on science by vague reference to future benefits over an indeterminate future. The future is valued less than the present in any calculation based upon economic wealth, and there is nothing to be said in favour of expenditures which impoverish the present under the false impression that they will benefit future generations.

Rather than end this section on a pessimistic note we point out that even with the rigours of discounting, science projects can be remarkable in their wealth creating potential. A study of the hybrid corn research programme in the USA conservatively estimated a rate of return of benefits over costs of 35% (5) and a similar study of polio vaccine research a return of 11.5% (6). Even at a 10% return the wealth invested in a project will have doubled in value after seven years!

6. INVESTMENT APPRAISAL

To calculate the economic worth of a strategic programme one calculates its net present value (NPV) according to the following formula.

$$NPV = \sum_{t=0}^{T} \frac{R(t)}{(1+r)^t} - \sum_{t=L}^{H} \frac{B(t)}{(1+r)^t}$$

where, r is the rate of discount (interest), R(t) are the annual research costs and B(t) are the annual net benefits. It is assumed here that the research is completed at date T, is translated into innovation at date L and generates benefits until the time horizon H.

If the budget is unlimited all projects with a positive NPV should be funded. With a budget constraint they should be ranked by their NPV and funded in descending order of priority until the budget is exhausted.

All this is purely formal routine. The real issues concern the quality of the data which is fed into the calculations. It is inevitable that much of this data will be of uncertain status for the future can only exist in the imagination. However, this is not a vital criticism of the investment view. Firstly, because it applies to all *ex ante* appraisal methods economic or otherwise, and secondly, because the investment view can still act as a filter even if the costs and benefits can only be placed within subjective bounds.

An example will best illustrate how the investment view provides a consistent framework for asking relevant questions about a research programme. Suppose that, on the best estimates a research programme will take five years (T) to complete and that the present value of the costs, at 10%, is £1 million. The best estimates also suggest that it will take a further five years before the first economic application. The question to ask first is what scale of benefit would give this project a positive NPV. One way of tackling this is to find that benefit annuity which just makes this investment worthwhile. To calculate this we need also to assume an investment horizon (H). The calculation is then straightforward. If the horizon is twenty years away from the present then the required annuity equivalent benefit is circa £362,000. With a horizon extended to thirty years the annuity becomes circa £272,000, and at a zero rate of interest these figures fall to £100,000 and £50,000 respectively.

Armed with this knowledge the next task is to enquire whether such figures have any likelihood of being realised. Answers to the following questions then become appropriate.

a) What products and processes will flow from this knowledge base?

b) When will they be applied to the economy and what advantages in performance and cost will they offer relative to established methods?

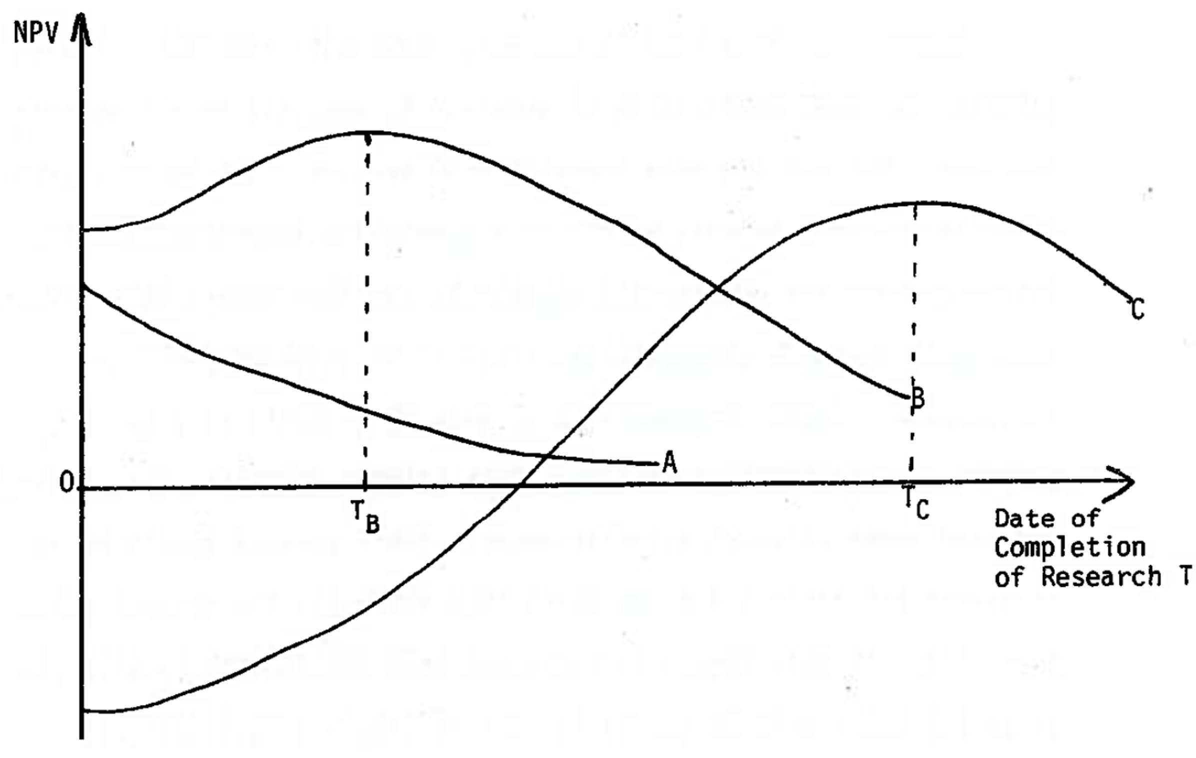
c) Given the scale of improvement, what current products and processes will be anticipated by the new methods? What niches are available and how quickly will they be achieved?

d) Are there prospective developments in other technological areas, standing in a complementary or competitive relation with the programme under investigation? What implications follow for its market niche?

Analysis of this kind is not only internally coherent but also permits the ready application of sensitivity analysis to the various outcomes. Nor are the data impossible to acquire. New technologies generate economic benefit by displacing existing technologies. The latter provide the appropriate yardsticks and they always have directly observable economic characteristics within an existing economic environment. Moreover, generation of this data should be a primary purpose of collaborative strategic work between industry, universities and government research establishments. Their purpose should be to construct plausible scenarios which form the basis for science policy judgement. In this regard it would not seem implausible to allocate 1% of the total science budget to such strategic investigations.

7. OPTIMUM TIMING AND THE TIME-COST TRADEOFF

The previous section has presented the traditional view of investment in which today's budget is to be allocated to a number of projects each completed by a specified date. In taking as given the terminal research date it has omitted consideration of an important aspect of science policy choice, namely, the best date to complete a research programme. Ideally the research should be completed at that time which maximizes its contribution to economic wealth. The important point here is that the NPV of any project will not be independent of the dates it is completed and applied to the economy. Assuming for our purposes a fixed lag between completing the research and applying it to the economy, we may illustrate the possibilities in the following diagram:



Three programmes are shown, and if they had to be completed today it is project B which dominates project A, with C ruled out because of its negative NPV. But today is not the best time to carry out project B, for delay will increase its contribution to wealth up to date TB which is the optimum date to complete this research. Indeed, A should be the priority project since its NPV declines continually with delay. Although C is ruled out for completion today, delay brings about a positive NPV with the optimum completion date at TC. In this sense good timing is at the heart of investment in science. Projects can be too early or too late with damaging implications for their contribution to economic wealth. It is thus not sufficient to justify expenditure on science today by reference to its future benefits. The correct policy is to time the completion of science so that it makes its greatest contribution to the economy.

The reasons which cause the NPV of a project to vary with the completion date are complex and would justify a separate essay. However, we may note that two factors which bring the optimum innovation date closer to the present are a larger market for applying the scientific knowledge and a more productive time-cost trade-off.

Again the time-cost trade-off appears in a central role, the optimum date to complete a project involves choice of the appropriate point in this trade-off. Selectivity in the funding of science is a policy aimed at the optimum timing of research. Precision in these matters cannot be expected but neither should the timing argument be ignored.

8. TURNING PROSPECTS INTO REALITY

While the investment framework is a coherent basis for decision making in science, its most important implications concern the way scientific research is organized. For the procedures are mere rhetoric if they are not matched with a commitment to achieve the return which is promised. Now it is obvious that many considerations are involved in the effective exploitation of science some of them properly beyond the influence of science policy makers. Others, however, are within the proper boundaries of science policy. Here the primary consideration is to ensure a sufficient degree of interaction and collaboration between researchers and industry to turn *ex ante* promise into *ex post* reality. Close collaboration is the only effective basis for implementing the investment view, for generating the appropriate data bases and shared assessment of prospects, and for achieving the efficient transfer of knowledge into commercial application. A policy framework in which science creates and industry applies will not provide a long-term foundation for the public support of science, however much science is claimed to generate future economic value.

What can be done to apply investment criteria? It is to the structures for determining priorities that we should turn, for industrially relevant research does not have an independent existence. Unless there is a mechanism for the transfer and application of that research it is not relevant. Such linkages may occur within an organization (between a firm's laboratories and production divisions) or between organizations. It is in the latter category that transfer of knowledge from universities or research council laboratories to industry would fall. While science policy makers cannot and should not attempt to second guess industry as to which areas are of future economic relevance, they can with more confidence affect the operation of these linkages. This approach yields the benefit of ensuring that the channels for exploitation are open. However, this also provides too linear a view. The structures which should be sought after are interactive, fostering exchange and collaboration between researchers and industry. From such an interaction, priorities will naturally emerge, a shared vision of the feasible investment options. There have already been several institutional innovations aimed at the generation of these links. The Directorates of the Science and Engineering Research C represent an attempt to execute a programme of focussed research in areas of national need. The problem they addressed was that the traditional disciplines in the universities do not correspond to the boundaries of the areas of need as defined by industrial areas or sectors. Thus, for example, the task of the Marine Technology and Polymer Engineering Directorates was to draw upon elements of several scientific and engineering disciplines and try to create linkages between them by means of an orientation towards the offshore industry and polymer industries respectively. This demonstrates the important point that traditionally structured departments, designed to meet teaching needs, frequently are incapable of providing an appropriate base from which to transfer knowledge.

A more radical institutional initiative came with the Alvey Programme which seeks to direct university research through the medium of collaborative projects with industry. Knowledge transfer is the essence of these arrangements but the insistence that intellectual property rights are agreed as a condition of funding has resulted in considerable difficulties and delays.

A system for the establishment of priorities based upon the investment criterion also requires an evaluation mechanism at all stages: *ex ante* for programme selection; monitoring during the course of the programme to refine and update priorities; and *ex post* to ascertain whether the criterion has been met. There are, however, severe practical difficulties. In particular, there is a problem of timing the evaluation so as to yield results which are usable. It may be many years before economic returns are realised and the uncertainties inherent in the innovation process should be reflected in any evaluation.

The evaluation mechanism for the assessment of programmes aimed at yielding economic benefits needs to address these structural issues, concentrating in particular upon the linkages. If these do not exist the economic return is unlikely to be achieved.

9. SUMMARY

This essay is written at a time of significant and justifiable disquiet about public levels of science funding in the UK, in particular with regard to fundamental research. Many arguments can be used to justify scientific research but no doubt the one which will enjoy increasing influence is that which identifies a proportion of the science budget for strategic research. Research programmes which are directed at future technological development and commercialization of knowledge. We have argued that this utilitarian view can only be treated seriously if it is supported by appropriate data gathering activities and by a commitment to effective exploitation of research. The two we think are inseparable and require close collaboration between researchers and industry if the utilitarian view is to be justified. That the utilitarian investment view separates science from the performing and intellectual arts should not be forgotten.

The central policy problem is thus one of creating appropriate organizational arrangements to implement the investment view. Success could well imply far greater funding for science. The consequences of failure are perhaps too unpleasant to contemplate.

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