Strategic policy for science*

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The primary issue of science policy, in Piganiol's (1961) sense of "policy for science", has been since its inception, the allocation of resources to research. This has been managed by two essentially distinct, though occasionally inter-related, processes.

The first of these processes is concerned with the allocation of resources between researchers, predominantly within their own fields. It operates at the micro-level and is deeply embedded in the social system of quality control for science. The same social mechanisms which are used to ensure the quality of information which is a candidate for the status of knowledge (Ravetz, 1971) are also used to make judgements about the quality and reputation of a researcher and the likely "success" of a proposed project. The process which is often referred to as peer review, is essentially private and consensual.

The adequacy of this peer review process has been the subject of close examination recently (see, for example, Mitroff and Chubin, 1979). Criticisms have focused mainly on issues of lack of consistency, the operation of a "Matthew effect" favouring established researchers, and the restrictions such an inherently conservative system places on innovation. There has been little suggestion that peer review does not provide the basis for an adequate system of quality control.

The second process has dealt with resource allocation at the macro-level, concerning the budget of science as a whole, and that of particular disciplines or specialities eg. high energy physics or molecular biology. This realm of decision-making is strongly marked by contest, and is conducted in accord with the familiar processes of the accretion and exercise of power that operate in other realms of human and social competition *viz* politics.

In most instances the micro and macro levels of resource allocation have relatively little interaction. The most common link is when the intellectual standing and promise of fields and researchers generated in the "first world" is used as supporting arguments, ie. tools or weapons, in the "second world". Less frequently, the promises of return generated in the second world has been applied as guidelines for allocation of resources in the first.

This separation of the first, internal world of autonomous scientific control from the second world of politics can be considered as one of the most remarkable balancing feats of the institution of science and in a large way responsible for science's significant political standing (eg. Haberer, 1969). Senior scientists emerged, qualified and sanctioned to play the political game while the remainder of scientists were free to pursue their research largely immune from political control, or even the need for political awareness (see, for example Cockburn and Ellyard, 1981).

However, the rules of the game are changing. Blume (1985) has analysed the development of science and technology policy in terms of three phases of development: the 1960s which rested on the assumption of "science as the motor of progress", the 1970s when science became primarily regarded as a problem solver, and the 1980s, when science became the source of strategic opportunity. With these phases came new approaches to resource allocation and an increasing pressure on the traditions separating the worlds of science and politics.

Ziman has characterised the transition of the research system to one of "science in a steady state" whereby the dynamic of science no longer operates with an ever-expanding budget, but is forced to operate within a fixed envelope of resources.

> *There are more researchable ideas and competent researchers than there are resources of finance and infrastructure to support them (Ziman, 1987.)*

The notion that there are limitations to the resources that can be committed to research is of course not new. Derek de Solla Price, who could be regarded as the founder of science policy analysis wrote on this subject from the early 1960s:

> *the most immediate international problem of science is not that of the under-developed nations; but that of the few over-developed nations; they have to end a long era of continued scientific adolescence and settle down to some sort of maturity. It is by no means the end of science and technology, for we can expect them to increase boundlessly and inconceivably, but it is the end of the social system of science and technology and the conditions of research to which we have become accustomed after 300 years of a tradition that seemed changeless (de Solla Price, 1985).*

The crisis would be particularly acute, Price predicted, because of the concurrent shift from an industrial to a service and high-technology society. Using a particularly powerful metaphor, he wrote "Science and technology were once the condiments of our civilisation. . . more recently they have been regarded as vitamins, tiny quantities of which could prevent stunted growth and enable us to absorb our industrial nourishment. Now they must be reckoned as the very meat and potatoes of our economy." (de Solla Price, 1965)

As Cozzens (1988, p. 365) has pointed out:

> *In 1968, Price saw what he interpreted as a first sign that the difficulties of saturation had arrived in the United States before it was prepared to deal with them. He saw the Mansfield Amendment as dangerous and wrong, and he denounced the tendency to support the science that seemed immediately useful and abandon what did not have foreseeable application. He told the Daddario committee in 1970. We have pruned . . . in such an arbitrary fashion that we shall approach saturation in wild and wasteful oscillatory swings below and above the reasonable marks as first one side wins the argument and then the other. My burden originally in predicting the onset of saturation conditions by about 1970 was to warn that we had very little time to prepare a sound science policy that could cope with the quite new state of the country. Now we have no time left at all and by action or neglect a new generation of science policy must arise.*

As an interesting aside, Price noted that it was important to get science policy out of the hands of scientists:

> *One must remember that scientists care passionately about their work and have necessarily a huge emotional investment in its proper continuation and use. They have to have this for without it nobody would bear such chagrin and weariness from Mother Nature. Because of this drive . . . scientists are most unwilling to give up any of their autonomy or bear criticism or control from outside. One result of this drive and resistance is that for many scientists — including some of the most respected leaders of the scientific community — "science policy" is just another name for the art of getting more money and support for the academic work that appeals to them. (de Solla Price, 1970).*

Acknowledging Price's vision and the fertility of Ziman's concept of science in a steady state, there remains a need to determine the nature of the "new rules", and what forces are shaping their development. There is strong and growing evidence that logistic limitations of resources are not the critical determinant of the new shape and context of science.

Rather it is what Price has identified as the shift from "condiment to vitamin to meat and potatoes", the growing importance of the products of research in establishing and maintaining an adequate level of international economic competitiveness, that is the major determining influence.

Research knowledge as intellectual capital

There is a variety of evidence of the growing importance of the outcome of research in the cycle of economic production. The growth in the level of industrial R&D, and of company investment in basic research, has been well documented (eg. see Martin & Irvine, 1989, pp. 1-2).

Similarly the extraordinary growth in the technology intensive sectors, particularly in information technology, which is estimated to constitute 25% of world trade, in terms of value, by 1992 demonstrates the high returns achieved by firms through investing in the strategic development, management and application of intellectual capital.

In Japan it has been estimated that "technological progress has contributed more than 50% of the annual growth in the gross national product since the mid-1970s" (STA, 1987, p. 15). In addition Kodama (1987) has shown that in the 68 major Japanese manufacturing companies, R&D investment surpassed traditional capital investment, on average, from 1987 (Figure 1).

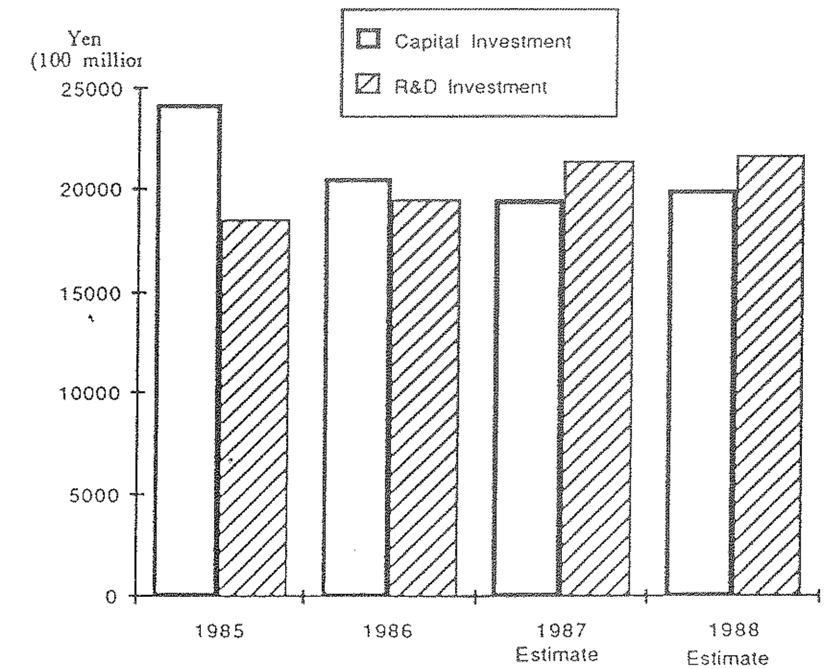
In many of the IT firms, the ratio of R&D to capital investment exceeded 2 to 1 (Table 1). What is more, these figures almost certainly underestimate the extent of expenditure on the application of intellectual and experience-based capital through the shopfloor-based incremental improvements of products and processes.

As Kodama argues

> *This signals a paradigm change; if R&D investment begins to surpass capital investment the corporation could be said to be shifting from a place for production to being a place for thinking. (1987, p. 201)*

This shift has been mirrored in a changing focus by economic analysts from the accumulation of physical capital to less tangible forms of accumulation, in the form of intellectual capital. However, as Geoghiou and Metcalfe (1990) have noted:

Figure 1
R&D investment and capital investment of 68 major Japanese manufacturing companies.



> *while the processes by which physical capital is accumulated and allocated are well understood (sic!) those concerned with the accumulation and allocation of intellectual or knowledge capital are less easy to comprehend and identify . . . the accumulation of knowledge involves indivisible investment, giving rise to increasing economies of exploitation.*

One consequence, as explored by Geoghiou and Metcalfe (1990) is the need for an appropriate exploitation infrastructure in order to capture the economic return from investments in strategic science and technology development.

Scott-Kemmis et al (1988) emphasises the importance of formal and informal learning in the accumulation of intangible assets:

> *Intangible assets are acquired through learning processes, involving an array of formal (R&D, licensing, training, hiring) and experience-based mechanisms. This latter type of learning involves, in particular, learning about the problems and opportunities inherent in a technology; learning about the firm's strengths and weaknesses with respect to particular technologies and markets; and learning about the external environment — about competitors, markets and suppliers. Such knowledge is gained as a result of direct experience in dealing with problems and interacting with other firms and new technologies. (p. 26)*

With this growth in the importance of R&D and intellectual capital there has come a dramatic increase in the level of the competition to develop, capture and apply these intellectual products. Many of the changes that have been occurring in the structure and management of the research system can be explained in terms of a response to these increasing levels of competition.

Thus, if research is critical to economic performance and the level of competition to develop and capture exploitable knowledge is high, it becomes important to attempt to identify the areas and types of knowledge which are most likely to lead to substantial economic returns. This has led to substantial investment in attempts to identify research areas of potentially high pay-off, through forecasting and foresight activities.

Because of the high level of competition for appropriate research results, the potentially high returns to be obtained from their application, and the high resource cost of producing exploitable research results ahead of the competition, there is a need to concentrate the resources of skill, infrastructure and money in areas with a high estimated return. This has led to the development of priority-setting.

The intense competitiveness requires that the research process be managed in a way

Table 1
R&D Expenditure Compared with Capital Investment in major Japanese Companies (Financial Year 1986)

Company	R&D Expenditure (hundred million year)	Capital Investment (hundred million year)
Hitachi Ltd	2515	1008
Toyota Motor Corporation	2500	2970
NEC Corporation	2400	1600
Nippon Telegraph and Telephone Corporation	1775	16,132
Toshiba Corporation	1716	1195
Fujitsu Ltd	1580	846
Nissan Motor Co. Ltd	1550	950
Mitsubishi Electric Corporation	1120	620
Matsushita Electric Industrial Co. Ltd	924	120
Mitsubishi Heavy Industries Ltd	870	707
Mazda Motor Corporation	800	1100
Nippon Denso Co. Ltd	619	870
Canon Inc.	609	540
Sharp Corporation	590	516
Nippon Steel Corporation	550	1650
Sony Corporation	507	504
Sanyo Electric Co. Ltd	506	655
The Tokyo Electric Power Co. Inc.	468	11,302
Fuji Photo Film Co. Ltd	421	424
Kobe Steel Ltd	362	835
Ricoh Co. Ltd	353	182
Takeda Chemical Industries Ltd	349	221
Asahi Chemical Industry Co. Ltd	345	532
Mitsubishi Chemical Industries Ltd	320	380
Komatsu Ltd	312	128
Kawasaki Steel Corporation	307	1049
Ishikawajima-Harima Heavy Industries Co. Ltd	300	137
Isuzu Motors Ltd	300	296
Sumitomo Metal Industries Ltd	288	823
Victor Company of Japan Ltd	281	198

that maximises its efficiency and productivity. This has led to the emergence of new structured methods of research management, based on more explicit *a priori* goal and milestone-setting, careful monitoring of progress towards the designated milestones, and regular review of the continuing appropriateness of the goals.

There is also a need to review, after projects and programs have been completed, how effective they have been in achieving their goals and what lessons can be learnt to achieve more effective management of future projects. This has led to the development of explicit techniques for formal research evaluation and of performance indicators which can be used to regularly assess research performance.

The effective production of potentially valuable research results is, of course, not sufficient to produce economic returns. This process is seen as being so critical in the research-production cycle that a range of new mechanisms and management techniques have been developed to ensure effective linkage and rapid transfer between the two stages. There is continuing experimentation in the development of new and more effective linkages of knowledge production and knowledge exploitation, within firms, between firms and across the public/private sector boundary.

Finally, the value of exploitable research is such that it has become even more important to seek to capture all the benefits of investment in research. This has led to defensive measures, such as increased intellectual secrecy, restrictions on publication, and more extensive intellectual property protection. More offensive approaches have emphasised the development of the "complementary assets" (Teece, 1987) of production, marketing and management of knowledge to ensure rapid and effective exploitation of research results.

There is also a much increased investment in the development and use of a variety of technology intelligence and publication scanning techniques to obtain insights into where competitors are focusing their research, where new "hot" developments are occurring and where relevant public sector research is being performed.

These consequences of the "investment" model of research are summarised in Table 2. All of these new approaches, many of which are being developed and applied relatively independently can be seen as different facets of the investment model of research which views research knowledge as intellectual capital.

We will now proceed to examine just one of these new science policy techniques — forecasting and foresight — in more detail. It is, however, important to note that the development of research foresight has been closely linked with priority setting and research evaluation.

Forecasting and foresight in science

The most extensive analysis of the development, the characteristics, the achievements and the limitations of forecasting and foresight in science have been conducted by Ben Martin and John Irvine from the Science Policy Research Unit at Sussex University.

In two books, *Foresight in Science: Picking the Winners* (1984) and *Research Foresight: Priority-Setting in Science* (1989), they have examined in some detail the development of research foresight and priority-setting in 8 countries — USA, France, FRG, Japan, Australia, Canada, Sweden and Norway — the only notable omission being the UK.

Perhaps the most important theme which emerges is the significance of the shift from rationalistic predictive "forecasting" to the construction of "foresight" and "anticipatory rationales":

> *Anticipation or foresight involves an explicit recognition that the choices made today can shape or create the future, and that there is little point in making deterministic predictions in spheres (including science and technology) where social and political processes exercise a major influence. (Martin and Irvine, 1989, p4)*

Foresight is a:

> *process by which one comes to a fuller understanding of the forces shaping the long-term future which should be taken into account in policy formulation, planning and decision-making (Coates, 1985)*

To paraphrase Martin and Irvine (1989, pp. 5-6) the special features of foresight are that:

- it is a process rather than a set of techniques;
- it is concerned with creating an improved understanding of possible developments and the forces likely to shape them;
- its aim is to outline the range of possible futures arising from alternative sets of assumptions about trends and opportunities;
- it provides a means for clarifying the scope for current action and implications for potential developments;
- it requires a holistic approach;
- the process should be transparent to allow the underlying assumptions, data and analytical frameworks to be scrutinised;
- any single analysis is limited and hence a set of complementary approaches should be adopted.

Table 2
Consequences for Science Policy of the Investment Model of Research

1. Need to direct research to areas of high potential — hence forecast and foresight.
2. Need to concentrate resources to increase probability of an effective outcome — hence priority setting
3. Need to manage the research process to ensure greater productivity and efficiency — hence new management methods.
4. Need to determine if research is being conducted effectively and improve productivity — hence research evaluation and performance indicators.
5. Need to ensure the products of research are protected — hence intellectual property protection and surveillance.
6. Need to ensure effective exploitation of research — hence linkages of knowledge, production and application.

The basic inputs to foresight with regard to research are: awareness of potential research opportunities, likely trends in socio-economic needs and demands for research, national strengths and weaknesses in R&D and the domestic capacity to exploit the results of research. (Martin and Irvine, 1989, p. 2).

Foresight processes and techniques are at a fairly early, immature stage of development. The majority of the reports are descriptive, portraying a variety of experiments each one deeply embedded in the cultural and institutional history of the particular nation. Empiricism dominates in a context where theory apparently has very little to offer.

The generations which Martin and Irvine are able to offer, reflecting on this stage of disparate experience, are largely of the organising, classificatory type which mark the early development of scientific fields. Thus, a typology of the key features and distinguishing characteristics of research foresight is constructed, with seven different dimensions (p. 28).

Likewise, the structure of the foresight process is analysed, and a complex flowchart involving twenty distinct elements is presented. This is not a criticism of Martin and Irvine's analysis; still less grounds for off-hand rejection of research foresight as invalid, resting on dubious assumptions, or of limited value.

Rather it serves to emphasise the early stage of development of the theory and practice of research foresight. Two important implications follow: first, the need to develop and enrich the practice, the assumptions and the theoretical underpinnings of research foresight; second, the need to apply research foresight with considerable caution, not demanding too much or too soon from its emerging, but immature structure.

Ravetz' general analysis of immature science offers considerable insights, addressing the issues of both theoretical development and practical application. Thus,

> *. . . the difficulties of working in an immature or ineffective field are serious and manifold. Added to the basic difficulties of trying to do research in a field where the pitfalls are still unidentified, there are the social constraints forced by the pretence of maturity. The situation becomes worse when an immature or ineffective field is enlisted in the work of resolution of some practical problem. In such an uncontrolled and perhaps uncontrollable context, where facts are few and political passions many, the relevant immature field functions to a great extent as a "folk-science". This is a body of accepted knowledge whose function is not to provide the basis for further advance, but to offer comfort and reassurance to some body of believers (Ravetz, 1971, p. 366).*

He examines in detail the conditions for the production of reliable knowledge. The results of research in immature fields are by Ravetz' definition, generally weak:

> *This condition prevails even in fields where the leaders and their associates spare nothing in their endeavours; but the absence of a body of appropriate methods of inquiry nullifies their efforts. For it is through such methods, ranging from the techniques of production of data, to the judgments of adequacy on an argument, that pitfalls are identified, and ways around them are charted. Because of the subtlety and sophistication of scientific inquiry, these methods are a craft knowledge, built up by successful experience. But an ineffective or immature field has no such experience; and so the improvement of its methods is not a straightforward operation. The weaknesses in the social aspects of inquiry also contribute to the self-perpetuating condition of ineffectiveness. The mechanisms for the processing of results, and for the exercise of quality control, cannot be stronger than the materials on which they operate. For social reasons it is necessary to give the formal authenticity of publication to masses of results which are very weak; and so the effective standards of quality cannot meet those of a matured field (pp. 369-370).*

There are considerable grounds therefore for caution in the development and application of research foresight. However, the conditions of immaturity can be overcome through the development of effective mechanisms of quality control, and the attraction of interest, and intellect, to the field. There are various signs of emerging institutionalisation, such as specialist conferences, journals and even professional associations emerging, which give promise for the achievement of maturity.

Nevertheless, and allowing for the fallibility and arrogance of immaturity, there are useful insights, and a powerful momentum, arising from various national experiments with and experience of research foresight.

Thus, from Japan, whose culture and tradition of consensual decision-making has facilitated the strongest development of research foresight, there emerges the importance of developing organisational structures which reconcile the tension of top-down versus bottom-up foresight processes, of integrating the views of interested parties and the results of systematic analysis, and of encouraging the development of an effective division of labour.

The French have recognised, after a long period of learning, the importance of developing an effective infrastructure for "anticipatory intelligence" and the means to achieve and use it. Foresight and evaluation need to proceed hand in hand, each complementing the other.

Countries with less experience of consensual decision-making, such as Australia and Canada, emphasise the development of a foresight culture, relying on systematic approaches to research policy, incremental introduction of foresight strategies, and the importance of an independent broker in achieving acceptance of the results of foresight. In contrast, in a small country like Sweden with a long tradition of planning, the emphasis is on global monitoring and development and use of early warning indicators.

Finally, the pluralist nature of the US political and research institutions has not prevented intense efforts to identify research opportunities, but these activities have not been closely integrated with policy-making or budget-setting.

Martin and Irvine (1989, p. 335) conclude that "*authority, legitimacy* and *credibility* are fundamental to success in foresight."

But without a culture that supports the political and intellectual assumptions, little success is likely to be achieved. A hostile culture nourishes those self-fulfilling prophecies that nothing can be done about the future. The essence of the combination of foresight with supportive culture and organisational structures is captured in the conclusion:

a policy of nurturing the scientific winners offers a rather better chance of success than one merely emphasising picking the winners (Martin and Irvine, 1989, p ix)

Much of the research of social studies of science and science policy is focused, understandably, on issues related to effective development and implementation of the new investment model of research. However, this view also raises quite serious, indeed fundamental, questions about the nature and operation of the research system, and long-accepted assumptions upon which it is based. These require careful examination.

Key issues for science policy raised by the investment view of research.

i The dynamics of resource allocation

Resource allocation is frequently viewed as something separate from the practice of research — something done by bureaucratic processes or committees of bosses or experts. However in the past the allocation of resources to research has been as much a part of the social system of quality control for science as referees of manuscripts submitted for publication or assessors of research proposals. The extensive analysis provided by the sociology of scientific knowledge has clearly revealed the range of interconnected social mechanisms which have been established to ensure the quality of the product — in this case knowledge, or at least candidates for the status of knowledge.

The allocation of resources is made on the basis of collective judgements about the quality and reputation of the researcher, or the research team, and the likely success of the proposed project according to an elaborate set of frequently implicit criteria themselves derived to a significant extent from the context of the relevant knowledge field, i.e. what seems possible, what might open up a new avenue of inquiry, etc.

The interconnections of this evaluation system has been one of its great strengths. It also has produced its casualties, whereby researchers socially determined as deviant or maverick have such a reputation reinforced by every element of the system. The chaos of discovery and intuition is restrained by the strong conservatism of tradition and precedence.

Of course such a system has been much less effective in determining resource allocation between disciplines — e.g. whether biology deserves more than physics. As we have noted, this has been conducted in a different realm, essentially in accord with processes of power accretion and application, i.e. politics, as applies in any other realm of human and social competition.

There has been competition for resources to be sure; and intense competition for the "glittering prizes", as Latour and Woolgar documented in their *Laboratory Life*. But the competition was for honour and glory of the individual, or team, and perhaps some relatively modest financial reward. The notion that the national research system is a vital component of national economic competitiveness and should be seriously treated as such has not been seriously entertained.

To return to the issue of resource allocation, one of the great fears of researchers has been that through priority-setting or other means decisions on research will be turned over to bureaucrats without the necessary understanding of research and its dynamics and even less of the promising lines of development in a particular field.

But there is a more fundamental question worthy of examination: If the broad patterns of resource allocation are established outside the dynamics of the research system, regardless of who makes the decision, or how it is made, what effect will it have upon the social system of quality control in research?

ii The effects of research management on research productivity and quality

The traditional collegial model of decision-making in research emphasises the autonomy of the individual to decide what research problems to pursue. This view has been somewhat eroded by the increasing amount of group research. It may also be regarded as something of a romantic view when seen from the viewpoint of the graduate student. Nevertheless it is this collegial model of decision-making that provides the basis of the much esteemed concept of scientific freedom.

In this view each researcher is effectively free to pursue their research to what they consider to be a satisfactory outcome, governed only by the quality control system, which determines whether the literary produce of research should be published, and the need to provide the employer with sufficient evidence of work and progress to retain employment or gain promotion.

The requirement for the management of research to ensure that resources are allocated to projects of the highest potential return and to maximise efficiency and productivity is one that is seen by many researchers as anathema, and antithetical to the creative freedom necessary for effective, rather than efficient, research. It carries connotations of the Weberian mechanistic bureaucracy, dominated by position and hierarchy, with all decisions to be made at the top of the organisational pyramid, and with those below doing no more than carrying out orders.

What is quite remarkable is that this threat, or perceived threat, to introduce bureaucratic control is occurring at a time when there is a very substantial shift in theory, and private sector organisations, towards quite a different model, emphasising flat organisational structures, devolved and decentralised decision-making and channelling of information to those best placed to make particular decisions (e.g. Peters, 1988).

This raises a second research question: What procedures, methods, structures and organisational forms are likely to be most appropriate to meet the twin needs of effective management to attain competitiveness, and maximum system flexibility necessary to cope with and capture the opportunities presented by the uncertainty of research?

iii Resource strategies — concentration through networks

There is a very strong push to achieve concentration of resources in order to generate and apply knowledge competitively. However, it may be that the importance of research group size (as measured by the number of people in a particular location) in achieving output is a product of previous organisational capabilities which has decreased with the growing internationalisation of science and improved telecommunications.

Where once it was necessary, or at least advantageous, to concentrate researchers and the equipment they needed in one location to get effective and rapid production of scientific knowledge, this may no longer be necessary. If appropriate networks are established, with effective communication procedures, it may be possible to establish effective research "groups" with members scattered around a country, or the globe.

Indeed there may be many advantages in such a mode of operation, in terms of access to knowledge, expertise and equipment. It would also be particularly relevant to countries like Australia and the US where research resources are geographically scattered. Hence there is a need to examine the role and importance of resource concentration in meeting the competitive pressures of the investment model of research, and the contribution that telecommunication-based networking could make to desired economies of scale and energy.

iv Conditions for effective linkage to users

The traditional model of the research system has been based on a high level of separation between researchers and potential users. According to the "serendipity" model, the performance of creative research required that researchers, and the research system as a whole, be left free from direct pressure in order to follow the unexpected leads which in the past have generated totally new fields of knowledge, with great economic consequences. If users or uses become involved, their interests would inevitably lead to a focus on shorter-term problems and to research of an "applied" nature.

The emergence of the concept of strategic research can be seen as something of a compromise between the previous views. It allows that research can be directed in a very general way by defining broad national or economic objectives to which research can contribute. It also leaves the choice of the particular direction of research and choice of research project to the social control mechanisms of the research system.

However, while the rationale for strategic research is plausible in science policy terms, the interplay between the dynamics of the research system and the requirements for effective capture of research results are yet to be examined.

Our previous preliminary studies (Bartels and Johnston, 1978, Coward and Franklin, 1986) have suggested that strategic research is likely to be most effective, in terms of both meeting external goals and maintaining the quality of research, if the links to potential users are complementary to, rather than substitutes for, traditionally established research networks.

Conclusions

The world of research, and of science, is irredeemably changed. The knowledge which is produced by the research process has become a strategic resource in international industrial competitiveness. The rapid and effective development and application of knowledge has become a key source of international comparative advantage at the level of the firm and the nation.

This new and steadily increasing level of competitiveness raises in turn new demands for the development of more effect means for the management of the research process. It is these demands which are behind the emergence of a new array of science policy techniques and the extensive level of experimentation occurring in most industrialised countries.

These techniques include research foresight, priority setting, research targeting, research evaluation, development of research performance indicators, intellectual property protection, and mechanisms for more effective linkage of knowledge production and application.

While an extensive level of experimentation is appropriate to develop and assess these techniques, it is important that they be applied with considerable caution. The new science policy techniques are still at an early stage of development and are insufficiently well-proven and robust to justify a heavy reliance of science policy decisions upon them. It is not only a matter of their application distorting or misdirecting a research effort.

There is also the need to create the environment in which these new techniques can be developed, tested and refined to a level where they will be of genuine value to science policy. Moreover, this development needs to be carried out with the important objective of overcoming or at least reducing the hostility of the research community to these new methods for the "management" of research, and of involving them in their refinement.

There is also a need for extensive research in those disciplines dealing with the dynamics of the research process itself, particularly (but not exclusively) the social studies of science. The investment model raises many critical questions for the operation and "health" of the social system of science. What adaptations should, or can this social system undergo? Where might the implications of the investment model threaten the quality of output, or even the very existence of the socially based research system? Questions of this kind provide a new and important challenge for scholars of science.

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