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RESEARCH FORESIGHT AND THE EXPLOITATION OF THE SCIENCE BASE

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PREFACE
CHANCELLOR OF THE DUCHY OF LANCASTER
WILLIAM WALDEGRAVE

This is a comprehensive survey of research and technology foresight in the United Kingdom. It was commissioned by the Office of Science and Technology (OST) in 1992 and has contributed useful insights to the discussions on the forthcoming White Paper on science and technology (S&T).

I welcome its publication by the OST as one of a series of papers intended to generate debate about the issues in S&T policy. I believe that both domestically, and in the wider context of the European Community research and development programmes, we must consider carefully how foresight methods may be employed to build consensus about research and technology strengths. They can help to identify areas of strategic research likely to produce the most pervasive economic and social benefits. They can be used to inform funding priorities in the private and public sectors.

I look forward to a stimulating discussion on this topic in the months to come.

William Waldegrave

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The author of this report is Mr Ben Martin of the Science Policy Research Unit (SPRU), University of Sussex. The study was commissioned by the Office of Science and Technology (OST) in the Office of Public Service and Science at the Cabinet Office. The views expressed are those of the author and are not necessarily shared by the OST.

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The author also thanks those officials in government departments, Research Councils and elsewhere who gave up their time to be interviewed and provided much useful information, as well as those experts overseas who supplied written material or took part in telephone interviews. Lastly, he is grateful to his colleague, John Irvine, who participated in previous SPRU studies of research foresight and with whom he developed the conceptual framework for research foresight drawn upon here.

Contents

EXECUTIVE SUMMARY

1. This report describes a one-month study to review research foresight activity (RFA) in the UK in both public and private sectors. It also examines recent examples of RFA in other Western countries. It attempts to identify the key criteria for selecting generic technologies and concludes by outlining the scope for future work on research foresight in the UK.

The United Kingdom

2. In 1986, the Advisory Council for Applied Research and Development (ACARD) called for the establishment of a process to identify priorities for publicly funded research in order to stimulate its effective exploitation to the UK's benefit. They argued that the creation of a forum to manage this process was a 'national priority'.

3. Two years later, the Centre for Exploitation of Science and Technology (CEST) was set up, funded primarily by companies. After one rather unsuccessful attempt to combine science-push and demand-pull perspectives, CEST has concentrated less on identifying exploitable areas of science and more on helping companies to exploit existing technologies. Whether this orientation is likely to change while CEST remains dependent on industry for 80 per cent of its funds is uncertain, although the new 'Environmental Foresight' project may herald a shift in emphasis.

4. Since 1987, the Advisory Council on Science and Technology (ACOST) has had the task of advising Government on general priorities for science and technology (S&T). The ACOST Emerging Technologies Committee has produced four reports on individual technologies. Some have made a reasonable attempt to link science-push and demand-pull factors. However, there has been little systematic foresight, no overview of the whole of British S&T, and little success in generating commitment in the scientific community and industry to exploit the Committee's findings.

5. Elsewhere in the public sector, there have been some attempts at research foresight. The Advisory Board for the Research Councils (ABRC) has instituted an annual procedure for discussing longer-term scientific opportunities but has been unable to develop a unified overview of British science. Individual Research Councils have engaged in a certain amount of foresight in preparing Forward Looks and Corporate Plans but have not adopted a particularly systematic approach.

6. The Department of Trade and Industry (DTI) has set up the Longer Term Studies Unit and the Longer Term Steering Group in an attempt to stimulate longer-term thinking. These have begun to encourage a more systematic approach to preparing the Department's Forward Looks. There have been other medium-term studies in which the DTI has been involved such as the strategy prepared by the Biotechnology Joint Advisory Board. Although confined to the area of biotechnology, this was nevertheless quite a successful foresight exercise.

7. The Energy Technology Support Unit (ETSU) has developed a systematic approach for appraising the prospects for different energy technologies, although it perhaps relies too heavily on in-house analysis by ETSU staff. As for other Government Departments, in the Ministry of Agriculture, Fisheries and Food (MAFF) and the Ministry of Defence (MOD), there are a number of foresight-related activities, while the Department of the Environment has recently launched an

interesting foresight exercise in collaboration with CEST, but they are all still some way from having an explicit, systematic and comprehensive research foresight system.

8. Among large UK science-based companies, around half those interviewed have undertaken a medium or longer-term foresight exercise in recent years. One common feature is an explicit set of criteria to assess different S&T areas. Another is the use of experts on both science-push and demand-pull to assess all the research and development (R&D) options in terms of those criteria. Companies may also employ a formal system to score each possible S&T field against the criteria. Such a process generally leads to clear priorities.

9. In addition, science-based companies devote much attention to monitoring external research activities, using a variety of 'science watch' procedures. This is seen as vitally important because the great majority of research is conducted elsewhere. The aim is to obtain an early warning of important S&T developments.

10. Consultancies and academic groups are also engaged in RFA. In particular, what is probably the most systematic example of research foresight in the UK is a study by the Unit for Policy Research in Science and Medicine (PRISM) on cardio vascular research.

11. In summary, although there are interesting examples of research foresight in the UK, these are carried out in isolation. There is no interaction between foresight at the macro and micro-levels or between foresight in different sectors. In particular, there has been no attempt to produce a holistic overview of S&T. Yet without this, it is impossible to analyse the interactions between different technologies and to identify the potential for technological 'fusion'.

12. There may also be too much reliance on the private sector to carry out all the foresight needed. Furthermore, there is no national forum for identifying priority areas of exploitable science in a systematic manner and generating a process to exploit them. ACARD's aspirations of 1986 remain largely unfulfilled.

RFA Overseas

13. Four years ago, there was little enthusiasm for research foresight in Germany and the United States, two countries with a decentralised approach to S&T policy. In both, there has since been an upsurge of interest in foresight, although for different reasons. RFA is now much more extensive than in the UK and is taking place at several levels.

14. In Germany, research foresight is still at an early stage. However, as a result of one exercise, the Federal Government is contemplating a fundamental shift in the balance between physical and biological sciences. Two other foresight exercises are just beginning. Both are experimental and both could be tried out in the UK.

15. In the United States, there have been several attempts to draw up a list of critical technologies using an explicit set of criteria. Industrial associations led the way, followed by government agencies. The approach adopted is by no means perfect. There is little use of empirical information and the mechanism is too 'top-down' in orientation. Nevertheless, the studies have generated increased interest in priority-setting. A more strategic approach is now spreading to other agencies such as the National Institutes of Health.

16. Research foresight is also flourishing in other countries. The Netherlands and Australia recently completed large systematic foresight exercises and another is under way in New Zealand. A common feature of these is a clear set of selection criteria. In the latter two cases, the criteria employed are virtually identical with those identified here (see paragraph 17) as central to research foresight.

Strategic Research Criteria

17. The lessons from previous surveys of research foresight, from UK companies and from experience overseas all suggest that there are perhaps five main types of criteria to be considered in selecting strategic research priorities and generic technologies: (1) demand-pull opportunities; (2) factors affecting a country's ability to exploit those opportunities; (3) science-push opportunities; (4) factors affecting a country's ability to take advantage of those scientific opportunities; and (5) costs.

18. In individual foresight exercises, some of these broad criteria may need to be broken down more finely. However, if there are too many criteria, it becomes more difficult and time-consuming to obtain all the data needed. One needs to strike a balance between comprehensiveness and practicality.

Options for Change

19. As regards the options for future work on research foresight, the first possibility is to embark on a national critical technologies exercise. One approach might be modelled on the current German exercise with project groups of the Federal Ministry of Research and Technology (BMFT). The aim would be for the DTI to tap the knowledge of its advisory committees. Such an exercise would need to be carefully co-ordinated, with participating groups being provided with a methodology, criteria and relevant data.

20. Secondly, Britain could join Germany and Japan in conducting a long-term forecast of S&T using a 'Delphi' survey of a large sample of active researchers in industry, government and universities. This would be relatively inexpensive and enable Britain to learn from the experiences of others.

21. A third suggestion is that the ABRC might set up a committee to review the science base and the balance of effort in UK research. This could be modelled on another BMFT exercise. However, such a committee would need to be independent of the existing Research Council structure.

22. Fourthly, the DTI (and other Departments) could encourage the Joint Advisory Boards and other advisory groups to produce sector-wide strategy documents similar to that prepared by the Biotechnology Joint Advisory Board (BJAB). There could also be some attempt to integrate the conclusions into a more macro-level strategy.

23. A fifth option would be to establish a national science-watch 'observatory'. Its task would be to organise the collection, synthesis and dissemination of information needed in foresight exercises. The first step here would be to review science-watch procedures in other countries and in companies.

24. A sixth option would be to create a group specifically responsible for research foresight and the identification of emerging technologies. This might either be a new organisation or build on an existing science and technology policy centre like CEST. Such a foresight group would develop foresight methodologies and provide guidance to others. It might also conduct 'holistic' foresight exercises of the type previously lacking in Britain.

25. Finally, work is required to establish the best means available to government for stimulating research foresight elsewhere, especially by groups of companies. This would again involve looking at experience abroad. Responsibility for foresight cannot be left entirely to the private sector. Foresight is difficult to carry out successfully, especially the first time it is attempted. There is a long 'learning curve'. The task is to determine how government can best get companies and others over the initial barrier so that research foresight ultimately becomes self-sustaining.

RESEARCH FORESIGHT AND THE EXPLOITATION OF THE SCIENCE BASE

1. Introduction

The overall objective of this 'scoping' study has been to review progress in the UK since 1986 in using research foresight activities (RFA) to help identify exploitable areas of science, and hence to establish the scope for future work in the area. Here, the term 'research foresight' is used as a shorthand for systematic attempts to look into the longer-term future of science and technology (S&T) with a view to identifying areas of strategic research likely to yield the greatest economic or social benefits.

The study has a number of specific aims:

(a) to analyse the main developments in the area of RFA and the exploitation of strategic research since the 1986 report by the Advisory Council for Applied Research and Development (ACARD) on the subject, looking in particular at the achievements of the Centre for Exploitation of Science & Technology (CEST) and initiatives by the Advisory Council on Science and Technology (ACOST), the Department of Trade and Industry (DTI), other government departments, the Advisory Board for the Research Councils (ABRC) and the Research Councils;

(b) to assess how present arrangements measure up to the aspirations of 1986;

(c) to examine important foresight initiatives in a few leading Western nations since the mid-1980s, and to point to any lessons for the UK, especially concerning criteria for selecting generic technologies;

(d) to review RFA in large private-sector corporations and highlight any conclusions for the public sector;

(e) to outline the scope for future work on research foresight in the UK.

A three-fold approach has been adopted. The first part involved analysing reports (both published and confidential) on RFA and on S&T priority-setting more generally. Some material had already been collected by the author in previous work on RFA, some was supplied by the Cabinet Office, and the remainder was obtained from interviews, library searches or by contacting the organisations involved.

The second component consisted of interviews with officials from CEST, the DTI, the Ministry of Agriculture, Fisheries and Food (MAFF), the Ministry of Defence (MOD), the Energy Technology Support Unit (ETSU), the Science and Engineering Research Council (SERC) and the Unit for Policy Research in Science and Medicine (PRISM) (see Appendix 3 for the full list). In some cases, these interviews identified other individuals with whom telephone interviews were later conducted. The Office of Science and Technology (OST) also provided a summary of interviews with eight science-based companies in the UK.

The third task was to obtain information on RFA in other countries. The author wrote to nearly 30 foreign contacts asking for examples of RFA and then conducted short telephone interviews to elicit details of more interesting foresight initiatives.

In what follows, the report looks first at the background to the study, in particular the ACARD report on *Exploitable Areas of Science*, subsequent institutional changes in the scientific and technological responsibilities of the Cabinet Office, and the establishment of CEST. The next section summarises the results of previous analyses of RFA and relevant lessons from recent science policy research on the nature of the links between S&T. The main empirical findings are reported in sections 4-8 which deal, respectively, with RFA for national direction-setting (eg ACOST), basic research (ABRC and Research Councils), strategic research and basic technology (eg DTI), and RFA carried out by consultancies and in manufacturing companies. Recent examples of RFA in other Organisation for Economic Co-operation and Development (OECD) countries (in particular, Germany, and the United States) are summarised in Section 9, while Section 10 considers the lessons concerning criteria for selecting generic technologies. Finally, Section 11 draws together the main conclusions. It also outlines possible next steps to develop the use of research foresight and thereby bring about more effective policies in the UK for the exploitation of the science base.

2. Background to the Study

In 1983, the ACARD launched a study on 'Promising Areas of Science'. The objective was "to survey current scientific developments and advise the Council on work which showed commercial and economic promise in the medium to long term"[1]. The ACARD Study Group commissioned various studies from the SPRU[2] and others, and met with companies and research organisations. Their findings were published in 1986 in a report entitled *Exploitable Areas of Science*. Recognising that future national economic success will be built increasingly on the foundations of scientific knowledge, the authors concluded that

> a process is needed to prioritise and guide a substantial proportion of that part of the national scientific resource be it Research Councils, Ministry of Defence or Department of Trade and Industry, and to stimulate its effective exploitation to the benefit of the United Kingdom. ... We do not have a forum in the United Kingdom where we can manage [this] process ... It is, we believe, a matter of national priority that such a forum be established.[3]

ACARD were, however, rather wary about suggesting a role for government in this process. They were apparently not convinced by the conclusion of the SPRU report on RFA in other countries that, in order to set such a process in motion, the government has to take the lead. (That report had cited the example of Japan where, even though three-quarters of R&D is funded by industry, the government still recognises that it must help initiate and catalyse RFA.) Instead, ACARD proposed "that industry itself should set up the mechanisms for undertaking long-term research forecasting on a permanent and routine basis".[4]

What reaction was there to this call for the creation of a process for identifying exploitable areas of science? The 1987 Conservative election manifesto took up the theme that a country of the UK's size could not afford to do everything in science and technology - clearer priorities were needed. Shortly afterwards, ACARD was replaced by the Advisory Council on Science and Technology (ACOST) with expanded responsibilities. Of its four terms of reference, the first was "to advise the Government on the priorities for science and technology in the United Kingdom".[5] Section 4 examines what progress ACOST and its Committee on Emerging Technologies have made towards this objective.

In 1988, the CEST was set up primarily with industrial funding. Many assumed that this would act as the forum sought by ACARD for identifying and prioritising exploitable areas of science. There have also been some attempts to adopt a more strategic approach to decision-making in Research Councils and Ministries. In addition, the recent institutional changes moving responsibility for the Science Vote from the (former) Department of Education and Science (DES) to the OST have brought with them a realisation that a longer-term overview of the whole of British science and technology is needed. Now is therefore a good time to take stock of what progress has been made in implementing the ACARD recommendations. First, however, it is worth briefly reviewing previous studies of research foresight and academic studies of the links between S&T. This will provide a framework for assessing the individual foresight exercises described in subsequent sections.

3. Lessons from Recent Science Policy Research

3.1 SPRU studies of research foresight

Two of the most detailed studies of research foresight have been carried out by John Irvine and Ben Martin. The first was for the ACARD Study Group in 1983. It looked at different approaches to research foresight adopted by ministries, research councils, science-based companies and consultancies in France, Germany, Japan and the United States. The second and more extensive study[6] was for the Dutch Ministry of Education and Science and was completed in 1989.[7] Several conclusions emerge from this work.

The first is a widespread recognition of the growing importance of new technology for economic competitiveness and social progress. With research costs rising and the number of scientific opportunities expanding, no organisation or country can afford to do everything. Choices have to be made - and research priorities selected. In the past, those choices tended to be made tacitly (they just 'emerged' from the policy process) or in an unsystematic manner. The question now is whether countries should continue with this approach or attempt to devise a more systematic procedure for research priority-setting.

Second, technological forecasting, after enjoying some popularity in the 1960s and early 1970s, fell into disrepute. During the second half of the 1980s, interest instead focused on foresight or *la prospective*.[8] This has a different philosophical starting-point than that of traditional extrapolative forecasting. The latter assumes that there is one, unique future. It is then the task of the forecaster to predict, as accurately as possible, what this will be. By contrast, foresight assumes that there are numerous possible futures. Exactly which one will obtain depends upon the choices made today. In other words, foresight involves a more 'active' attitude towards the future; countries have the power to shape the future through the decisions they take today.

Third, research foresight needs to be carried out at several levels ranging from bodies responsible for the co-ordination of overall national S&T policy down to individual research fields or technologies. Thus, some foresight exercises need to be 'holistic' in scope, others more micro-level. Furthermore, the foresight activities at different levels should be fully integrated, the results from higher and/or lower levels of foresight being fed into the process, and the results in turn feeding into subsequent foresight efforts at higher or lower levels.

Fourth, successful foresight involves counter-balancing several 'intrinsic tensions'. The first requirement is to balance science-push and demand-pull factors. At least for strategic research,[9] these need to be given approximately equal weight. A second tension concerns striking a balance between top-down and bottom-up approaches. The third relates to the extent to which responsibility for foresight is allocated to an interested party (involved in funding or performing research or in exploiting the results) or to a more neutral 'third party'. The former approach helps when it comes to implementing the foresight results but brings with it the risk of falling prey to vested interests (for instance, from established scientific disciplines or from sectors of industry).

Fifth, although the aim of research foresight is often to help set research priorities, it can perform other functions. These include national direction-setting or creating a shared vision of the future, anticipatory intelligence, generating consensus, advocacy (eg defending an existing R&D programme), and communication and education (eg about research opportunities or potential industrial benefits).

A sixth and closely related point is that research foresight depends for its success on involving a wide variety of people - scientists, industrial researchers and R&D managers, policy-makers in government and funding agencies, even perhaps the general public. The lesson here is that the **process** involved in research foresight is generally more important than the immediate outputs (forecasts, priorities or whatever). Those aspects of the research foresight process which are most important can be summarised as 'the five Cs':

(a) **communication** - bringing together disparate groups of people and providing a structure within which they can communicate;

(b) **concentration on the longer-term** - forcing individuals to concentrate seriously and systematically on the longer-term;

(c) **co-ordination** - enabling different groups to co-ordinate their future R&D activities;

(d) **consensus** - creating a measure of consensus on future directions and research priorities;

(e) **commitment** - generating a sense of commitment to the results among those who will be responsible for translating them into research advances, technological developments and innovations for the benefit of society.[10]

Another important contribution from previous SPRU work on research foresight has been the construction of a conceptual model for the process. This is summarised in Figure 1. The model distinguishes three main phases - pre-foresight, foresight and post-foresight. One conclusion from the empirical work was that many foresight efforts failed because insufficient attention was given to the pre-foresight or post-foresight phase. Details of the main factors to be taken into account during these two phases can be found elsewhere.[11] Instead, it is worth focusing here on the heart of the foresight process - the 'box' labelled strategic analysis in Figure 1.

As mentioned above, foresight for strategic research involves balancing science-push and demand-pull considerations. At the same time, one needs to take account of emerging opportunities and threats, on the one hand, and internal strengths and weaknesses, on the other.[12] This two-by-two combination means that there are four main inputs to be assessed in the strategic analysis:

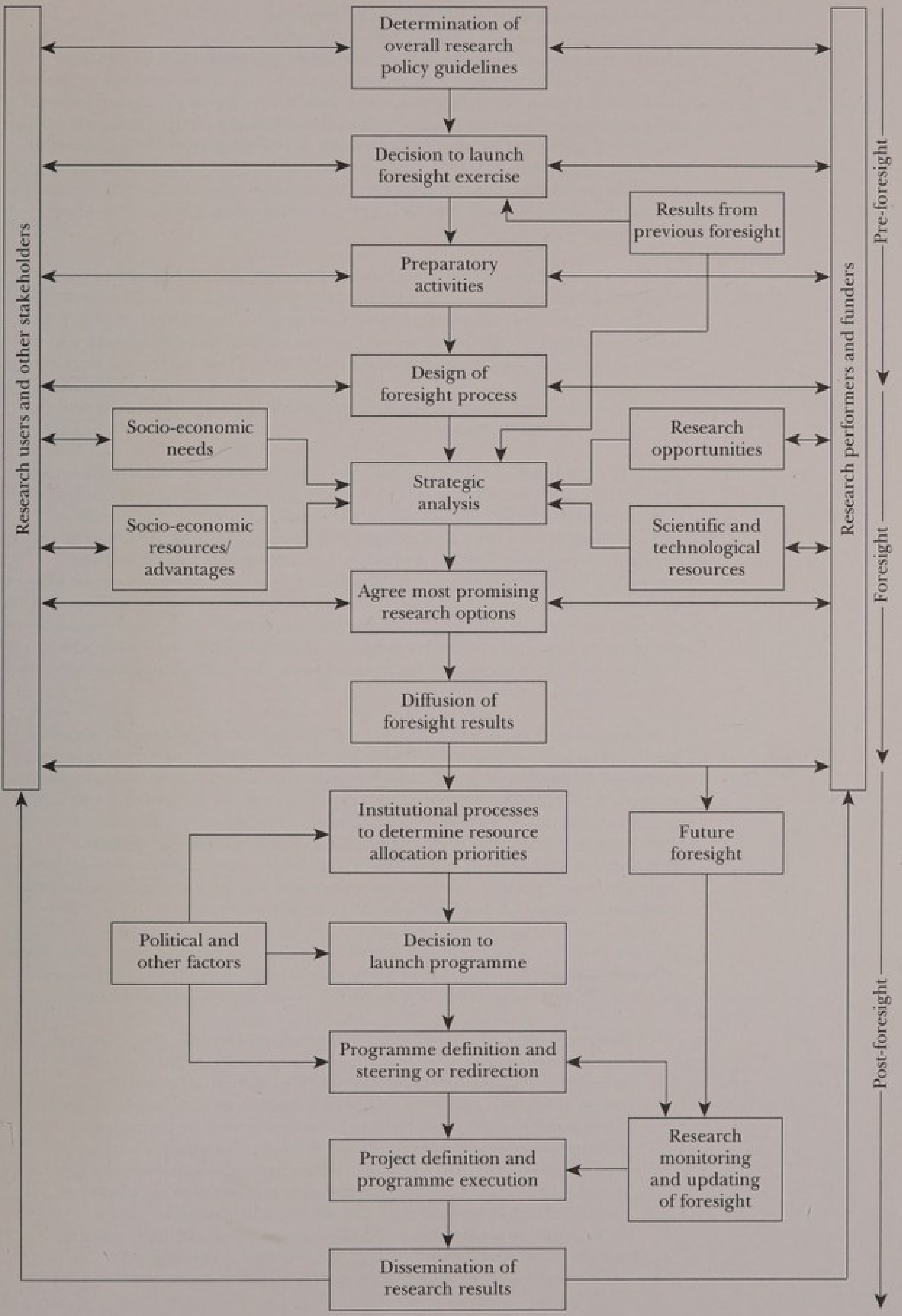
(a) evolving economic and social (eg health, environment) needs and threats;

(b) emerging scientific opportunities;

(c) comparative industrial strengths and weaknesses and other factors affecting a country's capability to exploit the potential economic or social benefits of the new technology;

(d) relative scientific strengths and technological capabilities and other factors influencing the ability to take advantage of the scientific opportunities - for example, the skills available, the financial resources likely to be forthcoming and the strength of the scientific infrastructure.

Figure 1 Elements and stages in foresight oriented to priority-setting (including implementation)



3.2 Other science policy research

Besides this work on research foresight, several recent academic studies have focused on the links between science and technology and on the needs of industry in relation to the science base. There has been much debate over the relative importance to industry of scientific advances in the form of codified knowledge compared with the availability of skilled researchers. In the view of 600 US industrial R&D directors surveyed by Nelson and Levin, three-quarters of the most important contributions of academic research to technological development have been in the form of uncodified (tacit) knowledge and skill transfers, while only one quarter involved codified knowledge.[13]

Other science policy researchers such as Rosenberg[14] and Senker and Faulkner[15] have also produced evidence that the benefits of basic research tend to be localised because they involve the transfer of tacit knowledge. That transfer process is best achieved through people working together, the implication of which is that there needs to be mobility between institutions and sectors. This has been summarised as

> the first law of technology transfer: the best mechanism for transferring technology is through the movement of people.[16]

Another contributor to the debate is Pavitt. His starting point is the difference in approach between basic research and technological development. In the former, the scientist tries to simplify, creating 'ideal' laboratory conditions. In contrast, technological development is concerned with making products, processes or systems perform in a world of multiple technical, economic and social interactions. Inevitably, the product, process or system will be too complex for its performance to be predicted from scientific theory. Instead, the main economic contribution of basic research lies in the skills it generates, the problem-solving methods of researchers, their instruments and informal networks of professional contacts - all of which may be drawn upon by the industrial researcher confronted with the complex problems of technological development.[17]

This body of work has brought about a shift in emphasis regarding the notion of strategic research. No longer is it sufficient to think solely in terms of the creation and exploitation of strategic areas of research. Instead, at least as much importance needs to be attached to ensuring adequate supplies of the well-trained researchers needed by companies and other organisations. Foresight must therefore take account of skill formation and human capital as well as scientific advances. Furthermore, any resulting science policy needs to give due emphasis to promoting networks, especially linkages between universities and basic research, on the one hand, and industry and technological activities, on the other.

One other science policy research contribution deserves mention, since it has been especially influential in the United States and Japan.[18] Kodama has distinguished two main types of approach to R&D and the exploitation of new technologies.

> Either a company can invest in R&D that replaces an older generation of technology the 'breakthrough' approach - or it can focus on combining existing technologies into hybrid technologies - the 'technology fusion' approach. The former is a linear, step-by-step strategy of technology substitution ... Technology fusion, on the other hand, is non-linear, complementary and cooperative. It blends incremental technical improvements from several previously separate fields of technology to create products that revolutionize markets.[19] For example, ... fusing mechanical and electronics technologies produced the 'mechatronics' revolution, which has transformed the machine tool industry. In a world where the old maxim 'one technology - one industry' no longer applies, a singular breakthrough strategy is inadequate; companies need to include both the breakthrough and fusion approaches in their technology strategies. Relying on breakthroughs alone fails because it focuses the R&D effort too narrowly (say, within one electronics specialty),

> ignoring the possibilities of combining tec hnologies ([e.g.] innovations in mechanics and electronics). Yet many Western countries still rely almost exclusively on the breakthrough approach.[20]

The implication here is that some RFA must be sufficiently broad in scope to enable potentially fruitful interactions between previously separate research areas or technologies to be foreseen.

4. RFA and National Direction-Setting

4.1 The Cabinet Office and ACOST

The replacement of ACARD by ACOST in 1987 was one of the main planks in the Government's response to a report on civil R&D by the House of Lords Select Committee on Science and Technology a year earlier. ACOST's task was to review research priorities across the board and to provide more co-ordination of Government science policy. In this section, the efforts by ACOST and the Science and Technology Assessment Office to advise the Government on research priorities for the UK are examined. The focus is on two initiatives - an attempt by the Assessment Office to develop a model to aid decisions on priority setting, and the reports on individual technologies produced by the ACOST Committee on Emerging Technologies.

4.1.1 Technology matrices

In 1987, the Assessment Office developed a model to help identify pervasive technologies relevant to industries with growth potential where the UK has a comparative advantage. The model built on the matrix approach developed within the DTI for the Technology Requirements Board (see Section 6.1.2 below). This was combined with S&T indicators such as the SPRU database on innovations. The starting point was that there are three levels in the innovation process - science, technology and applications. The goal was then to model the linkages between the three levels, identifying which technologies are more important for the production of different goods and services, and which areas of science underpin those key technologies.

To test the value of the model, the Assessment Office carried out a case-study on the industrial instruments sector. Three questions were addressed:

(a) can the technologies relevant to this industry be identified?

(b) can areas of science underpinning these technologies be identified?

(c) can the UK's relative strengths in the international instruments market be related to British strengths in underlying S&T?

The approach involved visiting instruments companies to obtain information, and discussions with trade associations, universities, the SERC and government departments. This information was then combined with results from the science and technology indicator databases.

The case-study showed that agreement can be reached on the technologies relevant to this particular industrial sector and on their relative importance - the data needed for this proved relatively easy to collect. Much more difficult to identify were the links from these technologies to areas of science, except in very general terms. The patent and bibliometric databases were quite useful, producing results in line with private sector views on British technological and scientific strengths. The indicators demonstrated in broad terms Britain's capabilities in science, technology and applications, but they were insufficient to establish a causal relationship between the three levels. Nevertheless, those consulted judged that a systematic approach like this could be valuable and capable of aiding judgements on the direction of support for science.[21]

A comparison of the instruments study with the foresight model outlined above shows that it covered some of the key elements - industrial needs, and scientific and technological strengths - although there was much less emphasis on science-push opportunities. Where it was far from successful, however, was in establishing a **process** for ensuring fruitful and continuing interactions among scientists, technologists and industrialists.

After the instruments study, the Assessment Office gave less emphasis to work on S&T priorities. Partly, this reflected a belief that they did not need a complex model; a general map of the links between technologies and the underlying sciences, together with a check-list of indicators to measure UK performance in applications and scientific research, was probably all that was required. In addition, ACOST felt that less effort was needed because "outside bodies such as CEST have now been established to offer advice in this area".[22]

4.1.2 Studies by the Committee on Emerging Technologies

ACOST established a standing Committee on Emerging Technologies (ET) in 1988. Over the next four years, this produced reports on four technologies, as well as briefly considering ten other topics. The first report was on biotechnology.[23] It was carried out by the Life Sciences Sub-group of the ET Committee. They commissioned a dozen papers from experts and sought written views from another ten organisations or individuals. As regards the four main inputs to RFA identified above, the report covers most of them reasonably well. It analyses present and future markets. It considers the UK's capacity to exploit the potential economic benefits.[24] There is a good analysis of research opportunities. And it uses international comparisons to help pinpoint areas of UK scientific strength. Although some conclusions are rather general,[25] it does succeed in identifying a small number of specific research priorities (eg embryonal stem cell biology), unlike other ET reports described below. It also points out that skill shortages are impeding the exploitation of biotechnology.[26]

By comparison, the ACOST report on advanced manufacturing technology is much less impressive. This was again prepared by a sub-group of the ET Committee. Part of the problem may have stemmed from the composition of this group which failed to strike a balance between science-push and demand-pull.[27] As regards the approach, the only substantial external input acknowledged in the report is a discussion with six industrialists and two other experts. On the economic demand side, the analysis of UK manufacturing is very sketchy and the only data presented are some general balance-of-trade statistics. The discussion of research opportunities is little better. There is no analysis of the capability of British industry to exploit the new technology, nor of British research strengths and weaknesses. These shortcomings are a little ironic as the report recognises that three key components of a technological strategy are an assessment of competitor performance, financial justification and human factors, yet the ET sub-group failed to follow this sound advice in preparing its report.[28] Few priorities are identified and those that are mentioned are relatively unsurprising.[29] Finally, the concluding recommendations are rather vague[30] although the DTI is now acting on the recommendation to set up a network on best practice in advanced manufacturing technology.

The working group which prepared the report on neural networks struck a much better balance between researchers and users.[31] Their terms of reference were taken from those of the ET Committee and included: "assess[ing] UK activity and its relative strengths and weaknesses" and " prepar[ing] views as to which areas should be given priority"[32] They consulted with a selection of industrial and academic experts.[33] The report analyses the potential demand for neural network technology, identifying a range of applications (for example, in the financial sector). It also considers factors likely to affect Britain's ability to exploit the technology, noting that "any strategy for the UK must accept that, in the electronics sector, the domestic manufacturing base is very weak".[34]

The report gives rather less attention to research opportunities although there is a comparison of UK programmes with those overseas. Where it is weakest is in identifying priorities. No specific priorities are proposed. Instead, the job of identifying priorities is passed to others; the report recommends that CEST be given the task of prioritising applications and exploitable areas. Furthermore, having identified the 'communications gap' between the creators of neural network technology and potential users as a major problem, the working group unfortunately made little progress in establishing a process of dialogue for bridging that gap.

The fourth ACOST report was on advanced materials.[35] An important input was a one-day discussion with 15 industrialists interested in advanced materials. The sub-group responsible produced one of the most detailed analyses of the four ACOST studies. It included: an examination of market trends for different types of materials; an assessment of the competitiveness of firms in the UK, US, Japan and the rest of Europe; an evaluation of current research programmes in different countries; and an appraisal of the associated skill requirements for advanced materials. The analysis pointed to certain weaknesses in the UK science base in materials and in industry as well as a shortage of skilled personnel.

A report was prepared in 1990 which was considered by the Government but not published. The report was subsequently updated by the sub-group aided by experts and it was published in 1992. The exercise proved quite successful in evaluating the demand for advanced materials, the factors affecting the UK's ability to exploit the opportunities, and current research strengths and weaknesses. However, like the neural networks report, the conclusions were rather general (although elsewhere in the report some priorities are mentioned - for example, intelligent processing[36]).

In conclusion, how successful has ACOST been in meeting the objectives of the 1986 ACARD report? Certain ET reports have had some success in linking research opportunities with economic demands. However, the problem is that the approach has been to focus on individual technologies. No attempt has been made to develop an overview. Without this, it is impossible to consider potential interactions between different technologies (for example, to spot possible instances of technological 'fusion') and to establish which technologies may be more generic or pervasive in their influence. Perhaps recognising this, ACOST recently reconstituted the ET Committee as the Working Group on Emerging and Generic Technologies. Its new terms of reference stress the need to take a more strategic approach, with less emphasis on studies of specific topics, but they do not indicate that ACOST will provide the forum for managing the process of establishing R&D priorities sought by the ACARD report. (In addition to the work of the ET Committee, ACOST provides annual confidential advice to the Government on its priorities for R&D expenditure across the board. As these recommendations are not in the public domain, their foresight content cannot be assessed here.)

4.2 CEST

CEST was set up in 1988 as an independent industry-based forum on the exploitation of science and technology. In line with the ACARD recommendation that industry should take the lead in funding, around 80 per cent of its support comes from over 20 industrial members and Government provides the remainder. This pattern of funding has undoubtedly shaped the direction of CEST's work. While its Director recognises that the original "concept of CEST was the identification of exploitable areas of science"[37], the industrial members have inevitably been more interested in the applied end of the research spectrum and in activities aimed at meeting company needs.

In CEST's earliest work, some attempt was made to give more attention to science-push considerations. Two studies were carried out, one on attitudes in industry to the exploitation of S&T and the other on the views of scientists. The former involved an interview-based survey of 50 senior industrialists in leading technology-based companies. The industries covered were grouped into three main sectors and, for each, the most important current and future **generic** technologies were identified. One finding was that environmental technologies were not ranked very highly while information technology was considered to be of declining importance. Interviewees were also asked about current and future **specific** technologies, with the most important ones again being identified. Here, one conclusion was that, although there was consensus that technology is critical for competitiveness, many companies have not identified their key technologies as clearly as their key markets. Another was that the main resource constraint faced by industry is lack of skilled staff rather than funds.[38]

Although the survey produced lists of generic and specific technologies, it is difficult to judge what significance should be attached to them. One problem, as CEST admitted, is that "The survey sample was not intended to be statistically significant in any way".[39] The other is that it covered only demand-pull factors. The science-push perspective was meant to be provided by the parallel study that CEST commissioned from the Science and Engineering Policy Studies Unit (SEPSU).

SEPSU conducted a questionnaire survey of 90 Fellows of the Royal Society and a few Fellows of Engineering. The aims were to identify links between areas of S&T and to highlight research areas of commercial promise. The results generated what SEPSU described as "a richly detailed snapshot of a fast-changing network of links between science, technology and industry".[40] However, even less significance can probably be attached to this miscellaneous list of scientific fields and potential commercial benefits than the one produced by industrialists. The problem is once more the survey sample. Those chosen were members of Royal Society committees - that is, elite and often quite elderly scientists concentrated in universities and working in more basic science.[41] For a survey attempting "to identify specific emerging areas of science and technology of commercial promise",[42] these were unlikely to be the best people to ask. As many admitted, "their professional experience was not a basis for making authoritative judgements about new commercial applications".[43]

In short, this two-fold approach by CEST and SEPSU was flawed in conception. One part centred on academic researchers who, as the SEPSU report observed, "frequently are limited to statements about what they think industry **might** need", while the other focused on senior industrialists who tend not to be familiar with emerging science and technology.[44] As experiences in other countries have shown, it is far better to combine the science-push and demand-pull perspectives in a single survey[45] concentrating on younger active researchers in universities (especially in departments with a slightly more applied orientation), government laboratories and industrial laboratories.

Since then, little of CEST's work can be described as research foresight. It has produced a number of reports on specific themes such as materials. In these, the starting point is future markets and industrial needs, and how technology might be exploited to meet them - in other words, demand-pull is paramount. Once a theme has been chosen, CEST carries out a preliminary analysis of the likely impact and identifies possible exploitable opportunities. The most promising of these are selected and possible projects proposed. For each area, a Technical Advisory Group (TAG) consisting mainly of interested industrialists (both suppliers and users), but also including a few scientists, is set up which holds meetings with experts, perhaps commissions small studies from outside consultants, and extends the CEST analysis. The group defines industrial needs and benefits more closely, examines the hurdles to exploitation and develops a collaborative strategy to overcome them. CEST's hope is that, once its work ends, a group of companies will be left in place to carry the exploitation process forward.

The materials study provides a useful example. The first step was to analyse previous reports on materials to identify external 'drivers' or technological issues influencing the way materials are used by manufacturing industry. A matrix was then produced which linked emerging technological issues with industrial sectors. Economic indicators were used to distinguish between industrial sectors that were strong, growing, weak or declining. CEST also analysed current UK support for materials research, pointing to certain areas of emerging technology which lacked adequate support (eg materials processability).

From the preliminary analysis by CEST, four projects were chosen for further analysis on the basis of the pervasiveness of the technologies and their relevance to the UK industrial base. One of these was adhesives. The TAG identified four projects and in each a small study was commissioned. Although the results were somewhat mixed, the companies involved found the results useful. A consortium of half a dozen decided to establish a centre to co-ordinate research on adhesives technology. A second priority area was surface engineering, but here the follow-up to the CEST work is not so advanced. The results of the materials project were written up in a report,[46] 200 copies of which were circulated to companies, the DTI, the SERC and others.[47]

In the latter part of 1992, CEST and the Department of Environment (DOE) launched an exercise in which foresight figures more prominently. The 'Environment Foresight' project is attempting to "identify and quantify future environmental issues by establishing a process of dialogue between key industry and government policy experts responsible for environmental management strategies".[48] In the first phase of the project, a national meeting was held to assess social, economic, technical, research and environmental trends. Phase two consists of "a series of issue-specific focus group projects to test and evaluate the observations developed in the initial session".[49] The groups again bring together key participants from government, industry and the research community. The results from the two phases are to be incorporated in a report from CEST to the DOE which will subsequently be published.

How does the CEST approach compare with the model of RFA given earlier? The approach scores highly in terms of analysing market demands and the capacity of British industry to exploit the opportunities. It is also effective in generating a process for bringing together groups of interested companies and scientists at an early stage, getting them to communicate with each other, raising their awareness of the possibilities, generating consensus on the best means of exploiting the technological opportunities and creating the commitment needed to carry that exploitation forward. However, with the exception of the materials study (where CEST did identify six scientific areas needing more research to underpin the exploitation[50]) and more recently the very promising 'Environmental Foresight' project, little attention has been given to emerging scientific opportunities, to analysing British scientific strengths and weaknesses, and to determining research priorities.[51] Thus, the focus has been not so much on the **identification** of exploitable areas of science as the **exploitation** of recently emerged or established technologies. In short, up till now CEST has not provided the forum for carrying out RFA and prioritising strategic research sought by the ACARD report. But nor can it perhaps be reasonably expected to do so while it continues to rely on industry for 80 per cent of its funds.

5. RFA in Basic Research

5.1 ABRC

In 1990, the Advisory Board for the Research Councils (ABRC) decided that they should have a discussion at least annually on "scientific opportunities". This would give the Board a chance to look beyond the planning and funding of research programmes in the immediate future to areas of science where significant developments are likely later in the decade. The hope was that the Board would be able to identify a number of key opportunities.[52] The ABRC therefore invited the Research Councils and the Royal Society to prepare brief submissions.

In the 1990 exercise, the limited time available meant that Research Councils could do little more than draw upon existing Forward Looks and strategy documents and consult internally. In the case of the Royal Society, it did not prepare a submission to the ABRC, arguing that "major advances in basic research are simply not predictable".[53] This seems to have side-tracked the ABRC discussion away from the identification of key opportunities and into a debate on how science progresses and whether there is any point in trying to foresee future advances.[54]

The ABRC repeated the exercise in 1991. This time, Research Councils had rather longer to prepare their submissions and a more thorough approach could be adopted. In the case of the SERC, for example, a small group consisting of the four Board Chairmen and the Programmes Director was set up to prepare a report. Each Board was asked to put forward six new areas, drawing on committee and Board strategy documents and other material. The group then met, discussed the proposed topics and narrowed them down to three or four per Board. To do this, they first agreed a set of criteria. These were quite broad (eg "considerable potential for new developments"[55]) and there was no attempt to quantify how well each topic scored on the various criteria.

The submissions prepared by the five Research Councils differed appreciably in terms of covering the main elements of foresight. While all dealt with research opportunities, only the Agricultural and Food Research Council (AFRC) and the SERC explicitly took into consideration UK research strengths (eg in protein molecular science). As for potential economic and social demands, the AFRC and the Economic and Social Research Council (ESRC) identified certain opportunities but the Natural Environment Research Council (NERC), the Medical Research Council (MRC) and especially the SERC gave this less emphasis. None of them appears to have assessed Britain's ability to exploit opportunities in different areas, and only the SERC specifically mentioned the issue of training scientists and engineers as a criterion. Some Councils (in particular, the AFRC and ESRC) highlighted specific new priorities (eg genome mapping, human organisation), but others tended to list existing programmes and the NERC explicitly refused to give priorities on the grounds that the direction of scientific progress cannot be foreseen and the country needs a strong research base across the whole of environmental sciences.[56]

At a subsequent meeting convened to discuss the Research Council submissions, the ABRC went through each paper in turn. In doing so, there was some discussion of areas of overlap between Councils. They also attempted to identify UK research strengths (eg plant molecular biology) and weaknesses (eg modelling of physical-biological system interactions). Some attention was given to cases where the application of new techniques to other fields might prove fruitful (eg non-linear mathematics) and to interactions between different fields. On the demand-pull side, there was also some mention of areas where UK industry is strong or weak (eg Information Technology (IT) devices).[57]

However, having gone through the five submissions, the ABRC apparently had little opportunity to consider the science base **as a whole** (including the Higher Education Institutions (HEIs)). The problem with this approach to research foresight is that it relies almost entirely on inputs from Research Councils. Inevitably, the existing Research Council structure (itself a product of priorities from the distant past) then constrains the discussion of future research opportunities.[58] As experience in other countries has shown, foresight should not be shackled by existing institutional structures. In addition, there is a certain lack of enthusiasm in some Councils about the utility of these exercises.[59] They would perhaps prefer a more detailed and rigorous prospective analysis carried out less frequently - say, every three years so that it could be more closely embedded in the three-year Forward Looks.

5.2 Research Councils

Besides preparing their annual submissions to the ABRC on research opportunities for the next ten years, the Research Councils engage in a considerable amount of medium-term strategy formulation as part of their Forward Look or Corporate Plan efforts. These activities are carried out at all levels - from Council through boards down to committees and subcommittees.

To take one example, the SERC has an annual rolling three-year Forward Look procedure. To help prepare this, committees are encouraged to look every two years or so at where research in their area is heading and to prepare a strategy document. Each committee approaches this differently. In a few cases, they may adopt a longer-term perspective. For example, in 1987, the Ground-Based Programme Committee (for astronomy) set up a working group to prepare a plan for the period up to 2010. The group consulted widely within the astronomy community. Although they took account of astronomy projects around the world, there was no systematic analysis (for example, of funding in different countries) and members instead relied on existing knowledge and information. The resulting plan included some priorities (although these were not as specific as those in the large US study on the future of astronomy referred to in Section 9.2.2 below).[60] The astronomy committee is about to repeat the exercise.[61]

Most SERC committees, however, adopt a much shorter-term perspective. The approach again generally involves widespread consultation but rarely anything more formal in the way of comparing research opportunities on the basis of UK scientific strengths, economic opportunities and Britain's ability to exploit the results.[62] The strategy documents prepared by committees are then drawn upon by SERC Boards, and their Forward Looks are in turn synthesized by Council. SERC strategy and priorities are therefore arrived at very much through a bottom-up process. There is no independent foresight effort within the SERC to help arrive at decisions on the allocation of resources across fields,[63] nor is much use made of the results from other foresight efforts (for example, by ACOST or CEST[64]).

The process of producing an annual 'corporate plan' is particularly well developed in the NERC.[65] These five-year plans are produced by Council, advised by various committees. Over time, they have become more systematic in approach. The 1992 version[66] contains many of the essential elements of foresight. It discusses general trends (eg from analysis to prediction of the environment) and specific research opportunities over the 1990s (eg the application of non-linear mathematics). It distinguishes areas of UK research strength (eg ocean circulation), even making use of research performance indicators.[67] It discusses national needs in relation to environmental research[68] - for skilled manpower as well as new knowledge.

Another input to the Corporate Plan is the five-to-ten year strategies for NERC's main scientific areas. These are prepared following wide consultation with the scientific community and highlight selected opportunities and priorities.[69] The 1992 Corporate Plan sets various corporate objectives (eg exploitation of research results) together with more specific targets (eg develop marketing aids). It also reviews progress towards achieving those targets since the previous Plan (eg training courses for NERC staff).[70] The NERC plans are published and widely circulated. Other Research Councils could perhaps look at NERC experiences to see whether they might benefit from adopting a similar approach.

6. RFA in Strategic Research and Basic Technology

6.1 Department of Trade and Industry

Within the DTI, the group most concerned with foresight-related activities is the Longer Term Studies Unit (LTSU). The Unit's remit is (i) to ensure that longer-term thinking contributes to all Departmental policy-making; (ii) to monitor social, economic and technological trends over the next 10-20 years; (iii) to identify technological or market opportunities with potential for exploitation; and (iv) to contribute to international programmes which assist in long-term policy-making.[71] The Unit conducts analytical studies, either in the DTI or in conjunction with other bodies,[72] on selected areas of technology or industry as well as some on general S&T policy. For all these studies, a 'customer' is needed, normally a DTI Division. In addition, Divisions conduct studies in their own areas, with strategic inputs coming from their advisory committees and the Innovation Advisory Board.

The work of the LTSU is overseen by the Longer Term Steering Group (LTSG). Their role is primarily to monitor the quality of proposed LTSU studies and LTSU output. In addition, the LTSG participated in the first two rounds of the Forward Look process (described below) by commenting on the drafts produced by DTI Divisions.

Subsequent sections look first at the Forward Look procedure within the DTI and the extent to which RFA forms a part. Next, a number of studies are considered that have involved a foresight component, one for the Technology Requirements Board, another by the Biotechnology Joint Advisory Board and a third currently being carried out for the Information and Manufacturing Technologies Division. Lastly, recent DTI work on generic technologies is described.

6.1.1 Forward Looks

Within the DTI, most R&D funds come from either the Innovation Budget or the Measurement, Technology and Standards (MTS) Budget. All R&D now has to be commissioned on a customer-contractor basis, whether carried out internally or externally. The adoption of the customer-contractor principle in 1990 raised worries that it might encourage a short-term approach. To offset this, Divisions were requested to prepare five-year Forward Looks. The aim was to set a framework for requirements over the medium term, to promote continuity in the development of programmes, and to send consistent signals about the future.

This system was introduced for the MTS Budget in 1990. In the first year, Divisions prepared Forward Looks largely on their own, although the LTSU and LTSG then scrutinised them to identify gaps or areas of overlap and to assess the quality of the proposed work. This first exercise did not prove very successful. The documents were very disparate in nature. Some were just extrapolations of Divisional one-year plans. Few if any contained a clear rationale for moving in a given direction. It was concluded that in future Divisions needed more time to prepare, that a standard format should be introduced for the Forward Looks, that the procedure should be more iterative with bilateral discussions between each Division and the LTSU, and that the Divisions should be asked to identify priorities.

These changes were incorporated in 1991.[73] A list was circulated to Divisions of the elements to be included in their Forward Looks and a workshop held to outline the principles of strategic planning. Each Division was given an LTSU contact person to work with during the preparation process, together with an external consultant. After bilateral discussions between Divisions and the LTSU, all the Forward Looks were discussed at a meeting to determine the distribution of the MTS Budget. These second-year Forward Looks were generally much better than the first set. There was more longer-term thinking, although certain Divisions are still weak at this. Some failed to provide an ordered list of priorities as requested. Further refinements are therefore planned for next year, including provision for contingencies in the form of budget cuts or increases.[74]

Although the Forward Looks involve extensive consultation, they do not represent a very systematic foresight approach and the science-push element is notably weak. (Some Divisions have advisory committees which provide a science-push component to their thinking but the resulting Forward Looks often obscure this factor.) In fairness, however, this is only the second year into what is evidently a gradual learning process and it would be interesting to re-assess the situation in two or three years time.

6.1.2 Technology matrices

In a study in 1987 for the Technology Requirements Board, the Research and Technology Policy (RTP) Division investigated a matrix-based approach to identifying technological priorities. Four areas were chosen for case-studies (eg specialty chemicals). Two were carried out by RTP with advice from Divisions and external experts, and two by outside consultants (with the help of industrial and academic experts). Among the tasks were to identify key products, to assess world markets and the UK's likely market share, to establish which British companies are involved, and to analyse UK strengths and weaknesses in relation to the underlying science, key technologies and companies.[75] Those who carried out the studies, besides consulting extensively with companies, academics and others, also drew upon scientific literature databases, patent indicators and market research reports. Using all this information, they produced matrices that linked scientific fields to technologies, technologies to products, and products to markets.[76]

What conclusions were drawn from this exercise? The matrices were judged to "provide a disciplined means of gathering and displaying information required to assist in decision-taking", generating a "useful picture of ... likely trends and key areas for policy consideration".[77] However, the final report admitted that

> the matrix does not appear to provide a ready solution to the need for a means of helping to decide priorities between broad areas of technology. Matrices are either too broad and superficial to be of much help ... or require too great an effort to be applied across all product areas.[78]

Although the report then went on to advocate a rolling programme of matrix studies focusing on priority areas or technologies[79] with CEST perhaps playing a role, the final DTI assessment seems to have been that this approach was not worth pursuing.[80]

6.1.3 Joint Advisory Boards

The DTI is represented on a number of Joint Advisory Boards, which are established when more than one government department or agency is involved in a particular technology. The Boards are expected to produce strategy documents on a regular basis. The only one that has yet done so[81] is the Biotechnology Joint Advisory Board (BJAB).[82] The first of its terms of reference is to "advise on the broad objectives, the strategy and balance of funding for support of R&D in biotechnology".[83] The Board is serviced by the Biotechnology Unit of the DTI's Chemicals and Biotechnology Division.

The first step Laboratory in preparing the BJAB strategy was to divide biotechnology into eight sectors (eg pharmaceuticals). In each, meetings or workshops were held with researchers and industrialists. The aim was to obtain their views on the innovation requirements of industry and to identify areas of cross-sectoral and interdisciplinary research which could yield benefits. To structure these discussions, the Biotechnology Unit or a consultant often prepared a discussion paper in advance. For each sector, an attempt was made to assess the relative standing of British companies, identify what research was required, analyse the position of UK research and establish what needed to be done. On this basis, a set of emerging strategic research topics was identified, from which a small number of research priorities were selected.[84]

The reports from all the biotechnology sectors were then reviewed by the BJAB who looked for horizontal themes - that is, broad areas of research important for a range of technologies. Among the criteria employed were the pervasiveness of the technology, whether it would draw upon an area of UK research strength, the relative standing of British companies[85] and their ability to exploit the new technology. In this way, the BJAB identified 11 cross-sectoral areas of strategic science (eg genome sequencing).

This exercise scores quite highly in achieving a balance between science-push and demand-pull.[86] It took into account economic opportunities, the ability of British companies to exploit those opportunities, and the relative strength of UK research. It also considered the associated manpower and skills issues. From all this, it generated a list of specific research priorities. It received a favourable response from the DTI, Research Councils and industry, which is perhaps not surprising as they had all been involved from the start. Subsequently, an action plan has been drawn up to implement the strategy. The plan is to repeat the strategy exercise after two years, although probably on a more modest scale.

6.1.4 The IT Futures Study

Another study of interest is currently being conducted for the DTI Information and Manufacturing Technologies (IMT) Division. The aim of the 'IT Futures Study' is to obtain a users' perspective on how IT will be used in five or ten years from now. The results should help define what the IMT Division ought to be doing to ensure that the UK IT infrastructure is in a healthy state in five years time. A consultant has produced a framework for collecting and categorising information on the main business objectives of seven industrial sectors (eg retailing). From a list of 40-50 examples of the ways in which IT could contribute, the consultant identified the 6-10 most important technological developments along with the driving forces and the factors inhibiting technological development. As part of this, a matrix approach was used to link the requirements of industry with technologies and to show what further information needed to be collected. An analysis of the various matrix cells suggested that some technologies are more capable of meeting user needs than others.

This work has enabled the DTI to build up a database on priority IT applications, driving factors and 'inhibitors'. The intention is to use it to highlight generic themes (cross-cutting several sectors) and hence priority target areas for support or regulation. Before that, however, the data must be validated by talking to companies in each sector (in interviews and workshops). The process has only just begun but the first results are encouraging.

The main goal in this exercise has been to establish user needs rather than to analyse technology-push. The research foresight content is therefore modest, the aim being to identify generic **issues** (eg electronic trading) rather than generic technologies or important areas of strategic research. In addition, although the original intention was to look up to ten years into the future, in practice it has proved difficult for users to look much beyond five years in such a fast-moving sector. Nevertheless, the IMT Division is hopeful that the scenarios developed in this exercise will prove useful in their efforts to improve the dialogue with industrial users and to enhance the effectiveness of technology transfer.

6.1.5 Generic technologies

During 1992, the LTSU, as part of an interdepartmental working group, began to respond to the Ministerial proposal for a 'modest enhancement' of pre-competitive research on generic technologies. The aim is to produce a list of key technologies and research areas important for the UK over the next ten years, together with some justification for their inclusion. Several steps need to be taken. The first is to agree a procedure and produce a set of selection criteria. The second is to assemble a list of generic technologies. Among the problems here are the definition of 'technology', the 'width' (or granularity) of each technology, the definition of 'generic', and how the technologies should be grouped. The third step will then be to apply the criteria to identify key technologies and research areas. The remaining tasks will be to determine which areas should receive government support and finally to decide the most appropriate support mechanisms.

The LTSU recognises that to carry out this foresight exercise properly, one needs to involve industry and the scientific community. The former are essential to produce information on commercial opportunities and the ability of British companies to exploit them, and the latter to point to research opportunities and to areas of scientific strength and weakness in the UK. Moreover, both groups need to be involved from an early stage in order to generate commitment to the foresight process and to ensure that the results are implemented. However, such a process will be lengthy and expensive. The interdepartmental working group has therefore opted in the first instance for a within-Government exercise carried out by a working group of DTI officials and a few expert consultants and involving only limited consultation with industry and other technology users through DTI advisory boards and related agencies.

6.1.6 Lessons from the DTI

What conclusions can be drawn about the various efforts of the DTI in the area of longer-term strategies for R&D and technology? The first point to stress is that, throughout the 1980s, there was a general nervousness within the Government towards engaging in activities which could be interpreted as the beginnings of an interventionist industrial policy. This is probably one of the factors that has held back the development of RFA in the DTI.

Second, despite the endeavours of the LTSU and LTSG, there is still little long-term thinking within the Department. The remit of the LTSU includes monitoring social, economic and technological trends over the next 10-20 years, but in practice they work mainly with a five-year horizon. Partly this reflects the short-term perspectives of politicians, industrialists and others in British society at large. However, another problem has been that staff tended to be seconded to the LTSU for a period of two years only. Although this has the benefit of bringing in a flow of new ideas, the life-cycle of a longer-term study means that two-year appointments are not a very efficient way of carrying out the work. However, this problem has been recognised and recent secondees to the LTSU stay for three years.

Third, with the exception of the recent response to the 'modest enhancement' proposal, most of the prospective analyses by the LTSU and others in the DTI have focused on individual technologies or industrial sectors. There have been few attempts to take a systematic overview of all the R&D and technological activities coming under the Department's ambit.[87] Yet without such an overview, it is difficult, if not impossible, to consider interactions between different technologies.

Fourth, one lesson from previous SPRU studies of RFA overseas is that another vital input to foresight is an assessment of existing research programmes. Partly at the instigation of the Assessment Office, there is now an established evaluation group within the DTI. Evaluations of past schemes are taken into account when proposals for new schemes are assessed. If the DTI decides to develop a systematic strategic planning process, there is obviously scope for incorporating the results of such evaluations into that process.

Lastly, in most of the foresight-related activities described above, the emphasis has been very much on demand-pull rather than science-push. To a large extent, this reflects the division of responsibility between the DTI and Research Councils (and the SERC in particular) for more applied and more basic research. The consequence is that the latter are primarily interested in science-push and the former in demand-pull. Until the recent generic technologies initiative, there has not been a forum where these two sets of factors are given equal weight. Yet overseas experience suggests that this is one of the essential ingredients for successful foresight to identify emerging areas of strategic research.

6.2 Department of Energy (now incorporated mainly in the DTI)

Most of the longer-term strategic studies of energy technologies for the former Department of Energy have been carried out by the Energy Technology Support Unit (ETSU).[88] Of most interest here is the series of energy technology appraisals conducted at roughly five-year intervals. The first of these was a fairly simple assessment of possible energy technologies, the results of which were published in 1976 as Energy Paper 11. This was followed in 1979 by Energy Paper 39, which contained a description of the technical potential of various energy technologies but did not attempt to assess their economic potential. Nor was their any appraisal of R&D programmes.[89]

6.2.1 The 1986 appraisal

The 1986 appraisal was a more extensive exercise in which far greater attention was given to the economic competitiveness of different technologies. It was commissioned by the Advisory Council on Research and Development (ACORD) which at that time advised the Secretary of State for Energy on energy R&D. The objective of the appraisal was

> to assist the Council in preparing its advice by providing a framework for the comparison of Research, Development and Demonstration (RD&D) relevant to the whole range of energy technologies from production, through distribution and supply to use.[90]

As regards coverage, the aim was to include all energy technologies relevant to the UK over the period up to 2030, although in practice the coverage of energy efficiency technologies was rather poor. ETSU[91] collected information from the literature and from a wide range of experts, and assessed each technology in terms of three criteria. The first was technical feasibility, with technologies being classified into one of four categories (eg demonstrated, speculative). The second was economic potential, with each technology being evaluated against three Department of Energy price projections. These econometric forecasts had been prepared in 1980 for the Sizewell Enquiry. By the time of the appraisal, it was clear that they covered much too narrow a spectrum of possible futures and two other scenarios (one assuming constant 1985 prices and the other low oil prices) had to be added.[92] According to how well they fared under these different scenarios, technologies were classified into one of three categories (economically attractive, promising or unpromising).[93] The third criterion consisted of a set of strategic considerations (eg security of supply). The results were summarised in a large table similar in format to those appearing in *Which* magazine, in which each technology was given a rating for the various criteria.[94]

The other main component of the appraisal was an assessment of the R&D programmes of the Department of Energy and of the nationalised energy industries. Again, a range of criteria were used including the motives for undertaking the R&D (eg economic, safety), the cost-effectiveness of the R&D (in relation to the five price scenarios) and other factors such as export potential. The results were once more summarised in a large '*Which*' table.[95]

The findings from the appraisal were published as Energy Paper 54 in 1986.[96] The first conclusion was that the majority of current and planned R&D programmes appeared cost-effective under most scenarios. Second, many of the most cost-effective R&D programmes involved energy utilisation rather than energy-supply technologies.[97] Renewable technologies also emerged as very promising while one particularly controversial finding was the non-cost-effectiveness of the fast reactor.

Several criticisms can be levelled at the 1986 appraisal. First, ETSU was constrained to use a narrow range of scenarios based on the Sizewell price projections even though doubts were already surfacing about how realistic they were.[98] Second, at that time ACORD was essentially a group of lobbyists for each energy industry.[99] The advantage was that this ensured ETSU had good access to information; the drawback was that it was subject to vested interests. This is a good example of the intrinsic tension between using an interested party or a more independent 'third party' to carry out foresight. It may have been one reason why no very clear priorities emerged, the Energy Paper being described as "a source book" rather than "a statement of strategy".[100] A third and related problem was the heavy reliance by ETSU on experts for technical information. Those experts often tended to be 'champions' for that technology and were therefore prone to give optimistic data. Fourth, the treatment of energy efficiency technologies was rather poor. Finally, all the assessments were based on individual energy technologies rather than the UK energy system as a whole.

6.2.2 The 1992 appraisal

By 1992, ACORD had decided that a new appraisal was needed, there having been several major changes in the intervening years including privatisation, growing environmental concern and international developments such as the single European market and the Gulf war. ETSU was commissioned to "provide a framework for the comparison of research, development, demonstration and dissemination (RDD&D) in the full range of energy technologies deployed in, or under consideration for, the UK." [101] The results should help ACORD decide whether current RDD&D expenditure is justified by the technological prospects, and whether additional effort or new programmes are needed.

ETSU's starting point for the 1992 appraisal was an internal assessment of the weaknesses of the 1986 exercise. Although the 1992 approach is broadly similar, efforts are being made to overcome most of the problems listed above. In particular, energy efficiency and conservation technologies are covered in the same depth as energy-supply technologies.[102] Second, ETSU is developing six scenarios[103] spanning a wider range of possible futures to identify technologies with the greatest resilience to future uncertainty. Third, the study includes a more detailed assessment of environmental impact. Fourth, there are formal procedures for checking and validating the data supplied by experts. Fifth, ETSU is using a computerised energy-system model[104] to allow the interactions between technologies from different sectors to be taken into account. Sixth, a more formal approach to the evaluation of RDD&D programmes is planned[105] in order to identify gaps or opportunities for government support.

The appraisal is being carried out mainly by ETSU staff although, where there are gaps in expertise, they are commissioning external consultants to provide information.[106] An ACORD sub-group is overseeing the exercise. The technologies covered have been divided into eight groups, with one individual allocated responsibility for each.[107] Their task is to produce the necessary information (either from the literature or from experts in industry and elsewhere), analyse it and ensure that it is validated by peer-review. For each technology, a 'module' will be produced describing the technology, its development status and any constraints on its uptake, data on costs, performance and environmental impact, an assessment of the technology, and an appraisal of the associated RDD&D programmes. The modules will follow a common format and will be reviewed and validated by experts before being published.[108]

The eight sets of technologies will be linked together using the energy-system model. From the data in each module, estimates will be made of the potential contribution of each technology in each scenario. This will enable the technologies to be categorised according to the scale of their potential contribution and how robust that contribution is under different scenarios. As in the 1986 appraisal, the results will be summarised in '*Which*' tables showing the strengths and weaknesses of each technology for a range of criteria. An overview report will be published and widely disseminated, and more technical reports on the scenarios and different technologies will also be circulated.[109]

In summary, the approach in the 1992 appraisal is more systematic and quantitative. As a foresight exercise, it covers demand-pull considerations in a rigorous manner along with the various factors - both economic and social (eg environmental pressures) - likely to affect the development and exploitation of different technologies. Although an attempt is being made to assess existing RDD&D programmes, there is less attention to science- or technology-push factors. The emphasis is very much on technologies that already exist, perhaps at an infancy stage (eg fuel cells), rather than those which are in the process of emerging. The justification for this is that the latter are unlikely to be widely applicable over the next 30 years.[110]

There could also be more emphasis on international comparisons to identify UK strengths and weaknesses, and greater use of the results of foresight by others.[111] Furthermore, by relying primarily on ETSU staff, the approach can perhaps be faulted for not doing enough to generate a dialogue among all the stakeholders involved in energy technology. Lastly, although there is much greater use of data than in the previous appraisal, different technologies and RDD&D programmes are only being assigned to broad categories of potential benefit. There will be no ranked set of priorities. The intention again is to provide ACORD and others with background technological intelligence rather than a detailed strategy for energy R&D. Nevertheless, despite these shortcomings, the 1992 appraisal is a considerable advance on the 1986 exercise.[112]

6.3 Ministry of Agriculture, Fisheries and Food

The Ministry of Agriculture, Fisheries and Food (MAFF) funds strategic and applied research in support of MAFF policies. The Ministry is divided into three divisions which are, in turn, divided into sub-areas. Some 20 Policy Groups are responsible for devising strategies or Forward Looks for their areas. Like the DTI, MAFF recently implemented the Rothschild Principle by which all R&D must be paid for by a 'customer'. As a result, the Policy Groups now have their own budgets for R&D,[113] with the Chief Scientists' Group (CSG), headed by the Chief Scientific Adviser, providing overall co-ordination and guidance.

The allocation of R&D funds is largely determined through a bottom-up process. Individual Policy Groups review the situation in their area and make recommendations on the R&D needed for policy purposes which they incorporate in their overall strategy. The MAFF R&D Committee discusses the R&D components of those strategies, in theory on the basis of what is presented, although in practice discussions will have taken place earlier between the Policy Group and the CSG. As part of the Public Expenditure Survey (PES) process, bids for expenditure in a given area are agreed between the Policy Group and a member of the CSG. This generates a set of proposals which are submitted to the R&D Committee together with a covering paper from the CSG.[114] That paper is influential in determining the allocation between areas, providing the Chief Scientific Adviser and the CSG with an opportunity to shape the overall strategy. Once the funds have been agreed by the Treasury, they are distributed using the Rationale-Objectives-Appraisal and Evaluation (ROAME) system for individual programmes.[115] All programmes are reviewed every four years or so, and from this may emerge new ideas or a revision of the strategy.

Foresight-related activities in MAFF centre mainly on the CSG and the Priorities Board for R&D in Agriculture and Food. In the case of the CSG, a number of activities can be distinguished. First, the scientific liaison officers who deal with contractors engage in general intelligence-gathering about research trends. Second, the involvement of CSG staff in the preparation of Policy Group strategies introduces a 'policy-pull' dimension, pointing to areas of research needed to meet policy requirements. Third, the programme review system, which is based on review by peers and other experts, yields information on the best strategy for a given programme area.

A fourth area of foresight-related activity concerns the CSG paper to the R&D Committee on Policy Group bids. This procedure was only introduced two years ago and caused some debate when it suggested significant policy shifts. Initially, the paper has been largely reactive, but the intention is to become more pro-active, with the CSG developing an over-arching research strategy that takes account of such factors as the UK skills base and how industry and others respond to scientific opportunities.

A major feature of MAFF's reorganisation of research-commissioning arrangements in 1990 was the commitment to make more explicit the policy basis for its research programmes. It was therefore decided that strategies should be prepared and updated annually. Responsibility was given to the Priorities Board for Research and Development in Agriculture and Food[116] which was at the same time restructured. The Board's remit is

> to advise UK Agriculture Ministers and the Chairman of the Agricultural & Food Research Council on priorities for research and development in agriculture and food and on the allocation of their research and development budgets.[117]

The Board is supported by six Advisory Sectoral Groups (ASGs) whose members include representatives from industry and government research-funding bodies. Their task is to review private as well as public-sector R&D programmes and analyse how well they are aligned with each other, pointing to areas where government research is not being exploited by industry or to topics which are not being addressed by either sector at present. After discussions and consultation, the ASGs prepared and circulated an initial set of strategies in 1990. These were then updated and submitted to the Priorities Board. The Board synthesized the conclusions and in turn prepared a report to the Agriculture Ministers. This was subsequently published, followed shortly after by three reports bringing together the R&D strategies for crops, animals and the environment.[118]

This first attempt at formulating a research strategy has not proved wholly successful. One problem is that the remit given to the ASGs is not very precise[119] and they tended to go about their task in different ways. Their success depended to some extent on the nature of the industrial sector with which they were dealing, some being relatively open, others more secretive. As regards the foresight elements included, there was reasonable coverage of industrial needs and current research programmes, but much less on emerging research opportunities and the ability of the UK to exploit the opportunities. The sectoral R&D strategies are also rather short-term (they cover the period 1990-92). In addition, the Priorities Board report for 1991 does little more than summarise the ASG submissions. It certainly does not put forward a coherent overall strategy and it fails to identify research priorities as requested. Whether the 1992 report currently being prepared will move further down the foresight learning curve remains to be seen.

To sum up, RFA within MAFF is, as yet, in a rather underdeveloped state. While there have been some attempts to develop an overview of the major areas in agricultural and food research, there is much work to do before these attempts amount to a systematic foresight system. One constraint is that, even if an interesting new idea does surface, MAFF must first identify a customer to take it further and some parts of the food-processing industry have been rather reluctant to be persuaded by MAFF's analysis. Another problem has been a series of structural changes to the Ministry - what one official described as "six years of cultural revolution". This has left little time for longer-term strategic thinking.[120]

6.4 Ministry of Defence

In addition to its large applied research programme, the Ministry of Defence (MOD) has a strategic research programme.[121] The role of the programme is to explore potential scientific and technological contributions to the MOD in the longer term. Foresight is part of the management process with a combination of top-down and bottom-up procedures. The former is provided through the Strategic Research Committee which consists of MOD scientists and independent academics. Their task is to assess the overall balance of the strategic research programme, relating it to changes in technological opportunity and defence demand. 'Bottom-up' initiatives take the form of proposals for new strategic research projects which are submitted to panels of the Strategic Research Committee. The panels rate the proposals in terms of their innovativeness, likely benefits and the quality of the work. MOD staff then combine the recommendations from different panels and put them before the Strategic Research Committee.

There are several other foresight-related activities within MOD. One takes the form of 'concept papers' which analyse the military requirements and possibilities over the next 25 years, and identify concepts for future military operations and the equipment needed. The concept papers include technology forecasts and the conclusions are one of the inputs used in determining the balance of the MOD research programme. A second form of RFA involves reviews of individual technologies.[122] Here, the aim is to assess the investment that the MOD needs to make to meet its requirements, having regard, *inter alia*, for work in the civil sector. The findings are forwarded for comment to the Defence Science Advisory Council which is made up of independent researchers from industry and academia as well as MOD staff. The technology reviews are carried out by MOD officials with the assistance of staff from the Defence Research Agency. Although the main focus is on obtaining an MOD perspective, some attempt is also made to incorporate a UK industry perspective (including, for example, an assessment of the strengths and weaknesses of British industry).

In short, a wide range of foresight-related activities is conducted within the MOD. Two points should be stressed about these exercises which distinguish them from the approach adopted by the US Department of Defense (DOD). The first is that RFA in the MOD is mostly conducted on a continuous and routine basis. With one exception,[123] there has been no attempt to carry out the large 'one-off' foresight exercises favoured by the US DOD for obtaining a systematic overview of future scientific and technological developments as they relate to defence. Second, MOD foresight activities are geared towards meeting defence needs and not, for example, with also increasing the competitiveness of UK industry. This is in direct contrast with the DOD 'Critical Technologies Plan' produced in 1990 where 'strengthening the industrial base' was explicitly included as one of five selection criteria used in drawing up the short-list of critical technologies (see Section 9.2.1 below).

7. RFA by Consultancies

7.1 PRISM

The Unit for Policy Research in Science and Medicine (PRISM) is a centre for analysis and advice in biomedical science policy. Funded by the Wellcome Trust, it also carries out commissioned studies for external agencies. One of its main areas of work is research foresight. It is developing new analytical procedures for generating visions of alternative futures for scientific fields which are desirable and achievable. It is currently carrying out a major foresight project on cardiovascular (CV) research. The study combines a quantitative analysis of the current state of CV research internationally, a large-scale interactive (or Delphi) survey of expert opinion, and a series of workshops to discuss alternative scenarios with researchers and research 'users'.

The CV project arose in response to a report in 1990 by the MRC Working Group on the Biology of Cardiovascular Disorders.[124] The objective is to build on that report

> by providing quantitative information, and by systematically surveying a wide range of interested parties in order to arrive at robust visions of possible futures [for CV research].[125]

It is also a pilot study to evaluate the use of foresight techniques in priority-setting exercises for biomedical science.[126] The work is being carried out mainly by PRISM, although some bibliometric data are being provided by CHI Research. It will require around three and a half person-years of effort and cost £140,000.[127] The project is being overseen by a Steering Group consisting of the original MRC Working Group.

The project has several components. The first involves collecting data on funding and manpower from the MRC, foundations and other organisations.[128] This 'macro-view' will be complemented by a 'micro-view' derived from an interview-based survey of funding in a sample of 20 research groups. PRISM is also exploring what data are available on CV research in other countries.[129] In addition, an analysis of publications and citations is being conducted by PRISM and CHI Research to compare the output and impact of UK research with that in France, Germany, Japan and the United States.[130]

A second part of the project consisted of interviews with 21 experts including clinical and non-clinical researchers, industrial scientists, research administrators and Health Service planners or managers.[131] The aim was to identify key issues affecting the future of the field which would then be addressed in the Delphi component of the project. The approach was to ask a set of seven open-ended questions.[132] The issues raised in the interviews were then analysed by PRISM and classified into 11 categories (eg research topics, infrastructure, exploitation).[133] Although the survey included a wide range of research funders, performers and users, it revealed some consensus among the different groups on key issues (eg the need for better dissemination of research results).

The third component consists of a two-stage Delphi questionnaire survey of around 400 basic, clinical and industrial researchers. The sample has been chosen using the results of the publication analysis mentioned above,[134] thereby ensuring that it covers active researchers in the field.[135] The questions reflect the results from the interviews[136] and can be divided into three groups: (i) identifying promising fields, assessing the likely timing of developments and gauging the potential impact; (ii) views on the current organisation of CV research in Britain, possible changes, and the main external influences; and (iii) an assessment of issues raised by research 'users' in the interview phase.

The findings from the first round of the questionnaire survey will be analysed by PRISM and sent to respondents in the second round, asking them if they wish to modify their views in the light of the aggregate results. One aim here is perhaps to generate greater consensus on future priorities. In addition, where there are divergent views, PRISM intends to identify distinct 'clusters of opinion'. From these, they will choose experts to form the panels used in the next stage of the project.[137]

The task of the panels is to carry out scenario analyses of those future developments which will most influence the direction of CV research over 1990-2010. The scenarios will explore the future organisation and infrastructure of CV research, the research opportunities and the fields ripe for advance in the UK. Four or five panels are to be set up, each with six to eight members, to look at key areas of CV research. They will include scientists, industrial researchers, health planners and research administrators, together perhaps with a couple of 'maverick thinkers'. Using data from earlier phases of the project, they will explore three scenarios[138] for their subfield. They will meet three times, the first occasion being an open-ended creative discussion. They will later use a voting system to weight and score different factors and thus arrive at a semi-quantitative picture of possible futures.

Finally, PRISM, aided by the Steering Group, will synthesize the results and prepare a report. PRISM also intends to write a separate report on the process employed in this foresight exercise, looking at how successful it proved in stimulating cooperation among funding agencies and discussion within the CV research community.

Although the project is still at too early stage to judge its success, certain comments can nevertheless be made. Overall, this is by far the most systematic example of RFA encountered in the UK at least outside the private sector.[139] It has taken on board most of the lessons from earlier studies of foresight. It strikes a good balance between science-push and demand-pull with the latter being well represented in the interview phase and in the panel discussions.[140] The precise needs in relation to CV research and the likely scientific opportunities have first been identified in the interviews and are being investigated systematically in the Delphi survey. The same is true of UK research strengths and weaknesses, although here data from the bibliometric analysis of different subfields of CV research as well as information from the earlier MRC report are also to be used. One element which is perhaps missing is an explicit assessment of Britain's ability to exploit the scientific and technological advances. However, the recent record of leading British pharmaceutical companies and the fact that they are investing heavily in R&D on CV diseases[141] suggests that any opportunities are indeed likely to be successfully exploited.

As a process for generating interaction, co-ordination and commitment, the project scores highly, with the full range of stakeholders in CV research involved from an early stage. Some thought has also been given as to how the work may best be organised. The Steering Group of senior officials from funding agencies and leading scientists should ensure that the project is seen as **authoritative** and that its results are eventually implemented. The involvement of research users and a large sample of UK CV scientists creates an atmosphere of participation in decision-making and thus gives the project **legitimacy**. Lastly, PRISM's background in biomedical research and track record in science policy analysis should ensure that the data, methodology and results have the necessary **credibility**.

7.2 PSI

The Policy Studies Institute is engaged in studies covering a wide range of areas of public policy (eg employment, social security, new technology, the environment). In 1991, it published a major report entitled *Britain in 2010*. This summarised the results of a two-year project funded by a consortium of Government departments (eg the DTI) and companies. It was carried out by an interdisciplinary team of nine PSI researchers, with additional papers commissioned from external experts. The study examined a broad spectrum of issues including the international context[142] as well as various aspects of life in Britain in 2010 such as population, employment, the environment and the economy. One chapter deals with S&T which is why a brief discussion of the study is included here.

The task that the PSI authors set themselves was not dissimilar to the starting point for foresight:

> what is involved is not **predicting** a future that is fixed, but making possible better-informed **choices** so that the future can be made better than it would have been otherwise.[143]

The approach adopted is described as "problem-oriented, ... empirical and pragmatic".[144] In addition, the researchers chose "to rely mainly on the information already available in each area".[145] In the case of the chapter on S&T, this seems to have meant drawing primarily on articles in newspapers or popular science journals.[146] The chapter begins with sections covering developments in the main technologies (eg IT, biotechnology). These contain a fairly predictable description of existing technological trends (eg further miniaturisation of microchips).

The chapter then discusses some general issues, including the convergence between scientific disciplines and technologies, the rapid pace of technological change, and constraints on technological exploitation. There is next a discussion of the likely effects of S&T on different sectors (eg agriculture, manufacturing, households). There is little new knowledge here, with certain of the developments already starting to appear (eg 'smart' cards and home-banking). The discussion then concludes with some overall implications. One is that successful exploitation of technology is associated with effective R&D, an area where many British companies are rather weak. Second, specialist skills are seen as a key factor in all new technologies. Third, in a number of areas, government regulation and intervention are vital to successful exploitation (especially in telecommunications).

Beyond circulating drafts of their findings, PSI researchers seem to have had little interaction with the scientific community or with industrialists and other research 'users'. Instead, they relied on existing literature, much of it of a 'popular' nature. Furthermore, their approach was rather uncritical and in places sloppy.[147] In short, while this is perhaps a useful synthesis for the general public, there is little of interest in relation to research foresight.

7.3 Other consultancies

Several other consultancy organisations have science policy skills and have engaged in studies with a research foresight element. Commercial organisations include PA Technology, Scientific Generics and Segal, Quince and Wicksteed (all of whom have carried out studies for the DTI). Overseas consultancies have also been involved in UK projects, for example CHI Research and SRI International. Lastly, there are various academic or non-profit science policy groups. Besides PRISM (see Section 7.1), these include: PREST at Manchester University who have participated in DTI R&D strategy discussions; SPRU at Sussex University who carried out the two international comparisons of research foresight described in Section 3.1 and have also been involved in the DTI Forward Look procedure (see Section 6.1.1); and SEPSU whose work for CEST was mentioned earlier in Section 4.2.

8. RFA in UK Companies

As part of this study, interviews were conducted with eight private-sector firms to discuss their arrangements for research foresight and for monitoring scientific developments. The companies were selected on the basis of prior evidence that they had instituted more or less formal 'science watch' activities. All companies were promised that their contributions would remain anonymous. In what follows, their activities are divided into medium or longer-term prospective analyses and shorter-term early-warning efforts.

8.1 Medium or Longer-Term Prospective Analyses

Around half the firms interviewed had conducted a thorough foresight exercise in the recent past. In one case, this took the form of a 'Delphi' survey carried out in 1989.[148] The aim was to see how the company's research programme measured against independent perceptions of new business opportunities over the next two decades. Fifty internationally respected academics critically analysed the research programme and suggested where there were significant omissions. Over the following six months, the company's research managers synthesized the critiques and recirculated them to the academics. There was thus another opportunity for the academics and company research managers to arrive at common conclusions about the balance of the research portfolio. The result was a concentration of 80 per cent of the firm's research programme into four strategic areas, one of which had previously been overlooked. (The remaining 20 per cent is spent following up long-term and more opportunistic leads.) In this and previous foresight exercises, the company has used several criteria in selecting key research areas. These include: (i) where areas of unmet medical need exist; (ii) where developments in science suggest possible breakthroughs; (iii) where the company has derived benefits from science in the past; (iv) where the company has leading-edge skills; and (v) where it has demonstrated commercial success and foresees further potential.

A second company conducted an extensive review of technology development options between 1985 and 1989. They began by classifying broad technological areas into five categories (pervasive, resource, product, process and life). Each was broken down into a number of quite finely disaggregated critical technologies (eg lubrication), and these in turn were related to scientific disciplines (eg molecular modelling). Each of the S&T fields thus identified was then appraised in terms of several criteria including pervasiveness, position of the field on the S-curve (running from more basic research through to development), the company's position (in absolute terms and relative to competitors), future potential of the field for the company, skill availability, chance of success and applicability.[149] Each field was scored in terms of these criteria,[150] although the scores were then integrated into three groups of broad criteria: (i) importance to the company; (ii) feasibility; and (iii) probable cost (in person years). Senior research managers reviewed the rankings and examined the implications for the company's R&D programme. The final output from the review consisted of a classification of each S&T field into one of four categories: (i) withdraw from research; (ii) monitor developments; (iii) participate in a research programme matching external efforts: or (iv) aspire to lead in a technology. In general, the more critical the technology, the more likely the chosen option would be to aspire to lead.[151]

The third company visited has carried out a strategic review of technologies and the scientific disciplines underlying them to determine broad areas of research on which to concentrate its efforts. The approach involved a series of formal meetings of researchers and business divisions. Again, several factors were considered in selecting the scientific disciplines including: (i) the strength of the company in the discipline: (ii) scientific areas where the company lags behind competitors; (iii) which industrial competitors or 'science base' laboratories are world leaders in the discipline: and (iv) possible company responses (for example, grouping company resources to strengthen a discipline, increased funding, or forging strategic research alliances). The analysis resulted in the identification of four broad S&T areas: physical and chemical sciences (in particular, organic chemistry), biosciences, process engineering, and 'consumer sciences' (ie sensory and behavioural studies). The company recently established an Exploratory Research Council to oversee strategy and research management.

A fourth company engages in reviews of strategic technologies at several levels. Every two or three years, business divisions conduct a strategic review in which new technology is an integral part. Typical questions asked include the following: are the R&D resources consistent with business aspirations? Are the key skills in place? Is the R&D balance right between short, medium and long-term needs? Are changes in customer needs addressed? What is the strength of the company compared with competitors? Is there a need to engage in more collaborative research? The review will then lead to a decision either to cease work, keep a watching brief, expand or contract the research, or attempt to take a leading position.

At the centre of the company, efforts are made to watch S&T developments across all the businesses, in particular to assess emerging technologies which are not 'owned' by any of the business divisions. There are periodic reviews of key technologies in which the fundamental question addressed is: 'Which technologies are critical to addressing the firm's markets in the year 2000 and beyond?' The current output of this review process stresses four prime technologies (eg synthesis of molecules) and four enabling technologies (eg IT). The former are loosely associated with individual businesses whereas the latter straddle the interests of several businesses. A Science Programme Committee is responsible for organising a 'Science Forum' or set of annual discussions at various levels in the company. At these, outside experts including academics take part in generating a fresh perspective on the overall balance of the research portfolio. The Committee first identifies some emerging areas of science (by scanning the literature, attending conferences and so on). These are discussed at the meetings, taking into account such criteria as the quality of the work being carried out, whether it is at the frontier of knowledge, relevance to the company businesses, and whether there is a sense of excitement about advances in the field.[152]

In all the above cases of medium or longer-term foresight, the purpose of the exercise was to decide upon the broad strategic direction of the company's research programme. In most cases, the companies do not intend to organise another major foresight exercise in the near future unless there is a significant change of circumstances or the firm's R&D position is seen to be slipping.

8.2 Short-Term Research Steering

Having decided upon the general areas where they wish to specialise, companies then have other foresight mechanisms to help adjust or 'steer' programmes in the shorter term. Some of these form part of a regular procedure (every year or so) to determine the allocation of research funds. Others relate to the efforts to monitor scientific developments outside the company. Each of these will be considered in turn.

Once the broad research strategy has been established, companies have a procedure for determining how the research budget is spent. Normally, this combines top-down and bottom-up signals. Two of the early inputs are often a call for proposals for new projects and a review of existing programmes. A set of criteria[153] is generally used to assess the competing alternatives. These may include some of the following: future market needs; in-house skills and research capabilities; the strengths of competitors; technical feasibility or risks; costs; the time to application; whether the results will benefit several of the company's products or businesses; ability of the company to exploit the R&D; and the 'fit' with overall company strategy. The various options are analysed in the light of the criteria, perhaps in discussions in which company researchers, business divisions and other company functions are brought together. Finally, decisions are made on the distribution of research funds to programmes and projects. Progress will then be monitored and some fine tuning may take place before the exercise is repeated.

Most of the companies also have formal or informal mechanisms for ensuring that emerging external science is adequately monitored. These 'science watch' activities are often focused around the key technology areas used to group the company's strategic research efforts. However, it should be emphasised that a majority of the firms interviewed consider much of their in-house science to be superior to that outside; it is better funded, more finely tuned to the company's needs and often ahead in terms of specific results.[154] Even so, external 'science watch' mechanisms are seen as a necessary complement to in-house efforts.

One company has appointed 'science leaders' in each of the main scientific disciplines identified within four broad S&T areas. These individuals are responsible for distilling from a wide variety of sources (eg published literature and conferences) ideas for emerging topics likely to constitute a suitable project. The company also has a senior advisory group (of four eminent scientists) and an external research committee to co-ordinate outside research work, supervise the search for collaborative partners and channel the views of university scientists and others into company research.[155]

In another company, links with the science base are formally enshrined in a 'Memorandum of Understanding' which regulates five Research Initiatives. Each Initiative focuses on a longer-term and growing technology area, with programmes on specific research topics supported by Cooperative Awards in Science and Engineering (CASE) awards, contracts and consultancies, all under the umbrella of the Memorandum. The results from each Initiative feed into one of the company's longer-term research projects. The firm recognises that some external science will be exploited first by rivals so it also has a Competitive Analysis Group which analyses trends in competitors' patenting to determine their strategies.

A third company has a large number of Data Exchange Groups which act as clubs in generic technology areas. The groups meet regularly to exchange and analyse information gleaned from continuous networking with external scientists which may be of relevance to the firm's four key technology areas.

In the fourth firm visited, there are eight Science Strategy Groups covering science in four 'prime' and four 'enabling' technologies. These meet three times a year to discuss proposals for strategic research. Their remit is to advise the Research and Technology Director on the relevance and quality of science both within and outside the company. Each group also sponsors several science clubs in fairly narrow fields judged to be of potential interest to the firm's businesses. Finally, there is a Science Programme Committee with the task of evaluating emerging science (and scientists) by scanning the literature, attending conferences, reading club reports and so on.

A fifth company has set up a Strategic Information Group whose task is to interpret published information and in particular co-citation data[156] to discover where new research effort might be directed. One of the group's responsibilities is to analyse the citation impact of leading scientific laboratories around the world, establishing where the best expertise is to be found. Together with company scientists, the group attempts to foresee where an emerging scientific area might lead in terms of new products or processes. The company also has a number of scientific advisory boards and consultants to provide an independent review of internal proposals.[157]

A sixth firm does not perform much in-house R&D (using the strict 'Frascati' definition). However, it employs a significant number of scientists and engineers who, it assumes, are well integrated into the wider research community and are therefore able to monitor developments in their fields. It plans to supplement that source of information with a subscription to the Industrial Liaison Program of the Massachusetts Institute of Technology which entitles the company to 'equal-first' notice of emerging research findings in relevant fields. The firm also subscribes to a similar French service. However, it recognises that a more in-depth assessment of emerging technologies is now needed.[158]

All the companies interviewed believe that they employ leading researchers in the scientific areas of most relevance to their businesses. As such, they feel well placed to network with the external research community. They all attend conferences and seminars; many of them exchange personnel with universities (and other centres of research excellence) at positions up to professorial level; they all commission research projects at universities and sponsor studentship schemes; and, finally, many of them have more or less formal systems for scanning the latest scientific and technical literature.

The firms stressed that these medium and short-term foresight activities are integrated with the research allocation process described above. There are obvious benefits derived from the 'science watch' efforts, particularly in respect of training research personnel: research knowledge has to be replenished and skills resharpened by continual exposure to new methods. However, 'science watch' is not regarded as a free-standing activity. All the companies have formal means of reporting science-watch findings to research committees or research directors who, in turn, are responsible for adjusting or confirming company research priorities. In many cases, this entails appointing someone in each area to collate information and advise on the balance of the research programme.

To sum up, the private-sector companies interviewed regard their intra-mural research as of primary importance. This resource is supplemented by a variety of formal and informal means of accessing the external research community. In most cases, the fruits of science-watch efforts are closely integrated with the allocation process for research funds. In some instances, the science-watch activities are structured on the basis of occasional in-depth analyses of company research strategy over the medium term. However, there is apparently no great 'well' of longer-term research foresight currently to be found within companies that could be drawn upon for national policy-making purposes (even among these leading science-based firms which were, after all, chosen precisely because they were believed to be more heavily engaged in 'science watch' activities). The hope of the ACARD report that "industry itself [would] set up the mechanisms for undertaking long-term research forecasting on a permanent and routine basis"[159] remains just that - a hope. There would seem to be no alternative but for the Government to take the lead and at the very least act as a **catalyst** for research foresight.

9. Recent RFA Overseas

In the short time available for this study, it was impossible to cover more than a couple of countries in any detail. It was decided to concentrate on Germany and the United States. This is partly because both these have a decentralised approach to science policy not unlike the UK. Their experiences may therefore yield more relevant lessons for the UK than, say, those of France or Japan. In addition, in both cases research foresight has begun to excite a lot more interest in the last two years. Following sections therefore look at each of these in turn. There is then a short summary of individual examples of recent RFA in the Netherlands, Australia and New Zealand.

9.1 Germany

Three years ago at the time of SPRU's review of RFA in eight countries, Germany was probably the least enthusiastic about foresight and systematic approaches to research priority-setting.[160] The reasons for this included: the constitutional stipulation that "science ... shall be free";[161] the complex division of responsibility for research between federal and state governments; and a belief that the level of public investment in R&D was sufficiently high that there was little need to concentrate resources on selected priorities - Germany did not need to specialise like Sweden, for example. All this has changed, primarily because of the costs of re-unification. Public spending on R&D is growing at no more than the rate of inflation[162] while the number of researchers to be supported has increased by some 15 per cent with the incorporation of the five Eastern states into the Federal Republic. Hence, research funding levels *per capita* have dropped sharply. Whereas Germany in the past was rich enough to cover most areas, now it recognises that expenditure will have to be reduced in certain fields. Choices therefore have to be made. In the last two years, one important foresight-related exercise has been completed and two more have just begun.

9.1.1 The BMFT committee on basic science

In 1990, the Federal Ministry of Research and Technology (BMFT) set up a committee to analyse the balance of BMFT spending on basic science and determine whether new priorities were needed, taking into account re-unification and increasing collaboration within Europe. One reason for establishing the committee was a rapid rise in the proportion of BMFT's R&D budget devoted to basic research.[163] Another was that the heavy investment in 'big science' areas of physics had peaked a couple of years earlier which meant that it was perhaps time to consider other priorities, for example in biology. A committee of 14 was appointed, six of whom were life scientists.[164] The approach involved a series of 'brain-storming' meetings in which experts on large scientific facilities, clinical research and industrial R&D amongst others presented their views.[165] At various sessions, the Chairman asked participants to submit lists of what they saw as important new areas, together with some accompanying justification, and he subsequently synthesized those lists.

The committee gave some consideration to the criteria to be used in deciding which basic research fields should be supported. Among those identified were the need for long-term continuity,[166] that originality and quality should be given more weight than immediate applications, and that special attention should be focused on new combinations of fields (or the boundaries between fields). However, the main criterion in the committee's view was 'Where are the best scientists working?'[167]

Using these criteria, the committee drew up a list of research topics to be given greater priority. Although these were only intended to be illustrative examples,[168] it is significant that seven of these were in life sciences and five in environmental or earth sciences, while there were just two in IT and computer science and none in physics.[169] This is consistent with a main theme of the committee's report - the need for an increased commitment to life sciences (and to environmental sciences), with new support mechanisms similar to those for physical sciences being established.[170]

The report was distributed to different branches of BMFT, responses collected, and the recommendations are now being implemented. The main area of controversy has been over nuclear and particle physics. The Minister has made it clear that current projects in these areas are unlikely to be curtailed but requests for new equipment[171] may well be turned down. The BMFT view is that now is the time to exploit existing large-scale equipment rather than rushing to replace it with new machines.[172]

As a foresight exercise, the first point to note is that the committee's emphasis was much more on science-push than demand-pull.[173] While the approach to assessing the former was not very systematic, the committee did attempt to involve the scientific community and they used a set of explicit criteria to produce a list of priorities. Although these were only intended to be illustrative, they nevertheless demonstrated that priority-setting is in principle possible. The committee was also not afraid to recommend a fundamental shift in emphasis from physical to biological and environmental sciences.

9.1.2 Studies by ISI Karlsruhe for BMFT

In a second foresight initiative, the Institute for Systems and Innovation Research (ISI) is collaborating with the National Institute for Science and Technology (NISTEP) in Japan. The Japanese Science and Technology Agency (STA) has recently embarked on the fifth of its 30-year forecasts of S&T. As before, the approach involves a 'Delphi' survey of several thousand researchers in industry, government and universities to obtain their views on the main developments in S&T over the next 20-30 years, when they will probably occur, how much impact they will have on the economy or society, the likely constraints, and the best means of promoting them.[174] With BMFT funding, ISI is conducting an identical survey in Germany. It has agreed with NISTEP to exchange the German responses in return for using the Japanese questions. This will enable comparisons to be made between the views of German and Japanese experts to see if the answers depend on national research and innovation systems.

The 1000 or so Japanese questions, which took approximately a year to prepare,[175] have been translated into German but it remains to be seen how appropriate they will prove in a European context.[176] A start has also been made in drawing up a list of 1000 experts in industry, government and universities to take part in the survey.[177] The hope is that, at the very least, the exercise will generate discussion and interest in foresight. If the experiment is successful, it may also help to develop a common vision of the future of S&T, and of the respective roles of government, industry and universities.

Work on the project only began in April 1992 so it is too soon to judge how successful it will prove. However, as has been argued elsewhere,[178] the approach rates highly in terms of bringing together science-push and demand-pull factors and examining them in a systematic manner. It also meets the 'five Cs' requirement of a foresight process described in Section 3.1. This is an experiment that Britain might look at with some interest.

The other foresight initiative again involves ISI but was initiated by BMFT.[179] The Ministry subcontracts the administration of some BMFT projects to a number of agencies (*Projekttraeger*) often located in national laboratories.[180] In this exercise, the agencies are to examine a list of approximately 100 emerging technologies likely to prove important over the next 10-15 years. Drawing upon the confidential information available to them, they will attempt to identify the most important ones for Germany. They will use an explicit set of criteria including such factors as timing, economic importance and non-economic benefits. ISI's task[181] is to co-ordinate the exercise, providing a scoring and evaluation system to produce a systematic assessment of potential usefulness. It is hoped that the procedure will generate a list of perhaps two dozen critical technologies together with an evaluation of **why** they are important. Although this study is still at an early stage, it too may hold some lessons for the UK, combining widespread discussions with experts with a systematic scoring procedure based on agreed criteria. The danger is that, at this time of financial stringency, the results will be used to cut BMFT programmes selectively, with the result that foresight will be seen in a negative light by the research community. It is clearly preferable to undertake RFA when there is scope for at least some enhancement of research funding.

Research foresight in Germany is by no means confined to BMFT. Following a recent report by the Science Council which commented upon the lack of co-ordinated foresight in Germany, the Council has just set up a committee to address this issue. In another initiative, the Chairman of the Commission of Enquiry on Technology Assessment in the Federal Parliament has proposed the establishment of an organisation like the US National Research Council to supply S&T policy advice to politicians. Among its tasks would be an assessment of individual disciplines and the most promising areas within them, and an evaluation of scientific developments and their likely impact. Industry is also increasingly active in this area. The Chemical Industry Association (VCI),[182] for example, carried out a foresight study earlier this year which identified promising fields of chemical technology. In short, foresight is starting to take place at a variety of levels in Germany, with increasing scope for each exercise to benefit from and contribute to the others.

9.2 The United States

The SPRU review in 1989 of research foresight in the US began by noting that at that time there was "no coherent policy" for federal research.[183] The government philosophy was to create a favourable climate for technological innovation and then let market forces prevail (although there were exceptions in the case of R&D related to specific federal missions like defence and agriculture). Since the 1960s, there have been many warnings that this approach is inadequate, but to little effect. By 1990, however, the evidence that US world market share and relative technological capabilities were being eroded was inescapable.[184] That year, the Office of Science and Technology Policy (OSTP) published *U.S. Technology Policy*, the first official statement of technology policy by the Executive Office:

> It breaks [new] ground by acknowledging federal responsibility to "participate with the private sector in precompetitive research on generic, enabling technologies that have the potential to contribute to a broad range of government and commercial applications".[185]

This gradual recognition that the US needs to have a coherent technology policy (with all that this implies in terms of selecting priorities) largely explains the great upsurge in RFA over the last few years. This section will first examine three exercises by government agencies to identify a short-list of critical technologies. It will also briefly mention similar exercises by industry and other analyses of longer-term priorities in government research.

9.2.1 Lists of critical technologies

In 1989, the Department of Defense prepared its first *Critical Technologies Plan*. This identified 22 technologies essential for the long-term superiority of US weapon systems. The criteria used in drawing up the list were whether the technology would (i) enhance the performance of conventional weapons systems, (ii) provide new military capabilities, or (iii) improve the availability, dependability or affordability of weapons systems. Some of the technologies are very specific to national security, others (eg micro-electronics) relate to the general industrial base. The exercise has since been repeated annually. In 1990, two further criteria were included - pervasiveness and strengthening the industrial base. The resulting list of 20 technologies differed slightly from that for 1989, with some technologies being merged and two new ones added. Estimates were also included of current R&D support levels for each critical technology. In addition in 1990, technologies were classified into three levels of priority.[186]

In 1990, the Department of Commerce also published a list of 12 emerging technologies offering substantial economic benefits for US industry by the year 2000.[187] Technologies were included if they had the potential: (i) to create new products and industries with substantial markets; (ii) to provide a large increase in productivity or quality of existing products; and (iii) to drive the next generation of R&D and produce spin-off applications. Two important elements in the exercise were an analysis of the 'driving forces' for emerging technologies and an attempt to assess the relative position of the US, Japan and EC countries with respect to their ability to develop and commercialise these technologies. The report also identified 13 policy areas where actions were needed to improve the climate and capability for exploiting emerging technologies (for example, regulatory constraints and engineering training).[188]

Under legislation enacted by Congress in 1990, the Executive Office is required to submit a biennial report on the nation's critical technologies up to the year 2000. OSTP set up a panel to identify up to 30 national critical technologies "essential for the long-term national security and economic prosperity of the United States".[189] Its members were drawn from industry and government agencies. Their first step was to review recent studies on critical technologies, especially the criteria and methodology for selecting the key technologies. They then drew up a comprehensive list of approximately 100 technologies and used a systematic method for narrowing this down using a number of evaluation criteria. There were three sets of criteria: (i) 'national needs', which included international competitiveness, defence, energy security and quality of life; (ii) 'importance or criticality', which covered the opportunity to lead the market, performance/quality/productivity improvement and leverage; and (iii) 'market size/diversity', which incorporated vulnerability, enabling/pervasive nature and size of ultimate market.[190] The panel gave primary consideration to technologies that could be incorporated into commercial products, processes or defence systems over the next 10-15 years.

Using this approach and with extensive inputs from the private sector and government, the panel arrived at a list of 22 critical technologies grouped into six broad areas: materials (5 technologies), manufacturing (4), information and communications (7); biotechnology and life sciences (2), aeronautics and transport (2) and energy and environment (2). Almost all of these were seen as essential to national security as well as economic prosperity so there was a considerable overlap between this list and the one produced by the Department of Defense (as well as the Department of Commerce list). The panel did not, however, attempt to rank the 22 technologies on the grounds that many support or enable other critical technologies. The panel's report also contains a profile of each technology, comprising a description of recent developments, reasons for its selection, current status and emerging trends.[191]

Industry has been engaged in similar efforts to identify key technologies. In 1987, for example, the Aerospace Industries Association published a report describing eight technologies crucial to the future of that industry. They employed four criteria in making the selection: dual-use (ie military and civil); enabling and high leverage; long-term, generic and high-risk; and currently inadequate levels of funding. Subsequently, the Association's National Centre for Advanced Technology prepared development plans for the chosen technologies. A 'lead firm' has been appointed in each area to co-ordinate a Technology Team responsible for reviewing and updating 'road maps' for technological advance and checking that the necessary resources are available.[192]

A second industrial example is a report produced in 1990 by the Computer Systems Policy Group, a group of 11 chief executives. Using analyses conducted by R&D managers from the companies, they identified 16 technologies critical to the computer system industry. They also examined critical factors which, if improved, would enhance US performance in those technologies.[193]

One other similar exercise deserves mention here. It was carried out by the Council on Competitiveness, a group of industrial and academic leaders. In the first stage, senior technology experts in nine major industrial sectors produced lists of critical technologies for their sector. Senior industrialists and academics then checked the lists. Next, all the sector lists were combined and the results again verified by experts. The end-result was a set of 23 technologies crucial to the performance of the nine sectors over the next ten years.[194]

In a report for the Manufacturing Forum,[195] a research consultant compared and assessed the above six exercises. She noted the broad similarity in methodological approach and the fact that they arrived at very similar lists of critical technologies, which perhaps implies that some consensus is emerging on which technologies are crucial to the future of the US. However, she made four criticisms. First, most of the reports involve little or no original research or data-collection and there is no guiding theoretical framework. Second, the criteria used to narrow down the initial lists are fairly general and limited information is given on how they were applied in practice. Third, the apparent consensus on critical technologies may just reflect the current 'faddishness' of certain areas. Last, the critical technologies identified are so broad that they are perhaps not very helpful in resource-allocation decisions.[196] To these criticisms, one might also add that in all the above cases the list of critical technologies was drawn up primarily by a committee aided perhaps by in-house experts. Particularly in the three government exercises, there was only limited interaction with the external research community and industry (at least until after the list had been produced). The exercises therefore score poorly in terms of establishing a process of dialogue between research funders, performers and users. The level of **commitment** to implementing the results of such exercises is therefore probably lower than if participation had been more extensive.

Nevertheless, these and other foresight initiatives appear to have had some influence on priority-setting in government agencies and on the President's selection of major science initiatives.[197] They have also succeeded in stimulating discussion about the longer-term future for science and technology in Congress and elsewhere. One option currently being pursued by Congress is to set up a Critical Technologies Institute to overcome some of the above criticisms (such as the shortage of empirical information) and provide decision-makers with better analyses of emerging critical technologies.

9.2.2 Other US foresight exercises

Besides the above exercises, there has been a flurry of other foresight studies in the last three years. The National Research Council has produced two more field surveys,[198] one on materials research and the other on astronomy. Both were carried out by committees and subpanels who consulted with hundreds of experts. The materials report continued the tradition of asking for more funds while shying away from identifying specific priorities.[199] The astronomy committee, in contrast, recognised the futility of requesting more federal funds in the current economic climate and instead concentrated on establishing priorities, even though this necessitated some painful choices.[200] Their report is consequently widely acclaimed in Washington as an exemplary model of priority-setting by scientists.[201]

Several federal agencies have also become more interested in longer-term priorities. For example, the Office of Technology Assessment (OTA), in a detailed review of the future for research over the coming decade, devoted one chapter to priority-setting in science, discussing a list of criteria which might be used for evaluating scientific initiatives.[202] In 1991, a Department of Energy (DOE) panel was set up to advise on priorities and "define a strategic vision for the national laboratories".[203] The panel heard representations from laboratory directors and DOE officials. They also received submissions from professional societies and laboratory user groups. In their report, they suggested some priorities for major physics facilities including the abandonment of certain projects. These met with much criticism from the external scientific community who felt that they had not been adequately consulted,[204] while the overall report was condemned by a senior Congressman on the grounds that it contained "very little ... that is either strategic or visionary".[205]

However, perhaps the most dramatic change has occurred in the National Institutes of Health (NIH). A few years ago, there was little apparent interest in NIH-wide priorities over the next decade.[206] In the last 18 months, however, considerable efforts have been devoted to preparing a strategic plan. During 1991, 'Task Forces' of NIH staff held workshop discussions and prepared reports on approximately 20 scientific areas and policy issues seen as critical for the future of biomedical research. A 500-page first draft of the overall strategic plan was then prepared with the intention of presenting it to the wider community. However, criticisms from the Department of Health and Human Services led to this being hurriedly replaced by a 16-page 'Framework for Discussion of Strategies for NIH'.[207] It set out five broad objectives (eg critical areas of science and technology, research capacity and intellectual capital), each of which was subdivided into a number of priorities (for example, the 'critical science and technology' category consisted of four priority areas - molecular medicine, biotechnology, vaccines and structural biology).[208]

The framework was presented to over 1000 scientists and others at five regional meetings where there was much vigourous debate. In June of 1992, a 'National Task Force' of 150-200 extramural scientists met to discuss the critical science areas and further strategic planning meetings are scheduled.[209] Although there is general agreement that more strategic planning in NIH is desirable, the external community has expressed considerable dissatisfaction with the approach adopted. They feel that they were not sufficiently involved from an early stage and that top-down priority-setting is dominating.[210]

To sum up, during the last three years, there has been a marked increase in RFA in the United States as in Germany. There is now a 'critical mass' of foresight so that efforts at one level or in one sector can feed upon and contribute to those in others. In both countries, it has come to be recognised that an explicit, coherent technology policy is essential for economic and other reasons, and that foresight can play some role in identifying which areas of science are likely to contribute most to emerging generic technologies.

9.3 RFA in other countries

This subsection looks briefly at examples of RFA in the Netherlands, Australia and New Zealand. There was no very systematic rationale for the choice of these countries other than the fact that an interesting research foresight exercise has recently been carried out and a written description is available.[211]

9.3.1 The Dutch Ministry of Economic Affairs

In 1988, the Ministry of Economic Affairs[212] in the Netherlands launched a 'Technology Foresight Experiment'. The overall objective was to provide information about emerging technologies with a broad range of potential applications in Dutch industry over the next five to ten years. More specific aims included the selection of priorities for technology policy, the formation of networks (especially between firms and universities) and the provision of information on new technologies. Within the Ministry, there was no prior experience of research foresight. It was therefore decided that an experimental approach based on 'learning by doing' was most appropriate. In addition, because there was a worry that a large holistic foresight exercise might result in 'paralysis by analysis', it was decided to opt for an incremental approach.[213]

The chosen process involved several stages. In the 'pre-foresight' stage, preliminary interviews were held with important stakeholders in research institutes, companies and elsewhere. The aims were to assess the usefulness of the proposed foresight experiment, to examine possible methods, and to generate support for such a project. A high-level steering committee was next set up along with a project secretariat. There were further consultations with experts on possible emerging technologies and plans for the exercise were discussed with senior industrialists. This widespread discussion helped to generate consensus on the proposals and ensured the involvement and commitment of stakeholders to the experiment. The price to be paid was one of time - the pre-foresight stage took nine months - but this was felt to be outweighed by the benefits. At the end of the pre-foresight phase, the Minister was asked for an explicit decision as to whether the exercise should proceed.[214]

In the main foresight stage, 20 senior R&D managers and scientists were interviewed and a list was drawn up of 15 emerging technologies with a broad range of expected applications during the 1990s. Experts with an overview of those technologies were then identified. Together, these experts and the steering committee formulated five criteria for assessing the technologies: (i) application potential (or pervasiveness); (ii) relevance to smaller companies; (iii) availability of a 'critical mass' of effort in industry, research and education; (iv) the network potential of the technologies; and (v) the multidisciplinary character of the technologies.[215] The interaction between the experts and the steering committee generated consensus on priorities, with mechatronics, adhesives technology, and chip-cards and electronic labelling being chosen for further analysis in case-studies.[216]

The next step was to formulate the work to be done in case-studies by consultants. Among the questions included were the 'state of the art' of the technology world-wide, the potential impact on Dutch companies compared with competitors, the international position of the actors involved, whether R&D was needed to build up the technology, the consequences for industrial employees and for training, the state of the teaching and research infrastructures, and the degree to which the actors involved were committed to meeting the challenge. Each of the case-studies[217] was supervised by a committee of six to ten stakeholders (drawn from firms, research institutes and universities). Besides monitoring progress, these acted as a sounding board for discussing interim results. The results of the studies were published in reports which were circulated widely.[218]

Each report formed an input to the next stage in the process - a strategy conference attended by approximately 80 experts from a variety of backgrounds. The main objectives of the three conferences were to inform opinion-makers and those likely to exploit the new technology on the findings of the exercise, to obtain a second opinion from experts on the results, to contribute to the formation of networks, and to generate a process of collective 'brain-storming' on follow-up actions. Each conference was organized jointly with a key player (for example, a bank in the case of the meeting on chip-cards) who could later function as a 'change agent' in that technological area. This main foresight stage was completed in the summer of 1990.[219]

In the subsequent post-foresight stage, a menu of policy implications was drawn up with around 30 possible follow-up actions. The steering committee selected some of these for government action. Further efforts were made to disseminate the foresight results to different industrial sectors and others. The experiment was also evaluated through a series of interviews with participants.[220] The consensus was that foresight should continue with two or three new case-studies being undertaken each year, a recommendation which was accepted by the Ministry.

How successful has the Dutch experiment proved? Overall, it represents one of the most systematic foresight exercises in Europe over recent years. It demonstrated that foresight is a useful tool for strategic management, helping to identify and assess technological fields of particular interest to a country. It also proved effective in stimulating discussion between different stakeholders and achieving the 'five Cs' described in Section 3.1 - communication, concentration on the longer term, co-ordination, consensus-generation and commitment.

An analysis by one of those centrally involved drew five lessons from the experiment. First, a 'learning' approach is better than deterministic forecasts or blueprints for the future. Hence, it is essential to foster interaction between stakeholders so that they co-produce the results. Second, no single agency can carry out foresight on its own. The production and transfer of knowledge is a two-way process. If all the stakeholders are involved, they produce shared visions of the future that can become self-fulfilling prophecies. Third, a foresight exercise needs to have the necessary legitimation and authorization right from the start. In the Dutch case, this was achieved through the participation of high-level experts and decision-makers. Fourth, one needs to avoid foresight that produces 'average' results. Deliberate efforts must be made to include 'wild card' possibilities. Last, foresight should not be conducted in such a way that the government is seen as the 'helmsman' steering industry and society. **Governments operate more successfully when they jointly produce policy with other stakeholders.**[221]

9.3.2 CSIRO, Australia

Another large-scale foresight exercise was conducted in 1990 by the Commonwealth Scientific and Industrial Research Organisation (CSIRO) in Australia, a country where previous attempts at foresight had met with mixed success.[222] The aim was to construct a framework to assess, in an objective manner, the potential benefits to Australia of research in different areas and to identify factors enabling or hindering the achievement of those benefits.[223] CSIRO developed a fairly simple assessment framework based on four criteria: (i) potential economic and social benefits (eg size of markets, contribution to increased productivity, import replacement, benefits to health and the environment); (ii) ability of Australia to capture the benefits; (ii) R&D potential and costs; and (iv) capacity of Australian researchers to deliver the results.[224]

The next step was to assemble a group of senior CSIRO staff who discussed the proposed criteria and the factors underlying them. They were given briefings on different areas and provided with supporting information (for example, on funding, CSIRO staff resources, and the views of potential users on the strengths and weaknesses of the different research areas). Using that information, they assigned a score (on a 10-point scale) to each research area for each criterion. The scores for the four criteria were then combined to give two measures: (i) 'attractiveness' which represented a combination of potential benefits and the ability to capture them; and (ii) 'feasibility' - a combination of R&D potential and R&D capacity.[225] This was followed by an iterative discussion of individual scores by participants before the final values were agreed.[226]

The attractiveness and feasibility scores were plotted on a two-dimensional figure enabling comparisons to be made between the research priority for each area. Among those ranked highly on both criteria were mineral engineering and animal production. However, it is important to stress that the interpretation of the scores as a basis for future action depended on exactly what factors were seen as underpinning a high or low score. In other words, a high score did not necessarily mean that greater emphasis was recommended for research in that area.[227] The results have since been used within CSIRO to reallocate resources, with support for IT, for example, being reduced because of the conclusion that this is one area where Australia has proved poor at appropriating the research results.

What did the CSIRO exercise achieve? First, the assessment framework incorporating clear selection criteria enabled CSIRO to gain a better insight into the performance of its research support activities. The systematic approach minimised the influence of prejudice and certainly contributed to more informed decision-making (for example, regarding the Cooperative Research Centres Program[228]). In addition, it provided a language of communication across the research-policy divide and a means to arrive at consensus on specific priorities which could be incorporated in CSIRO funding decisions.[229] Where it was weaker, however, was in bringing potential research users into the dialogue. In particular, it tended to rely rather heavily on the views of CSIRO staff. How committed Australian companies and other users are to the results remains open to question.

9.3.3 The New Zealand Ministry of Research, Science and Technology

Finally, brief mention should be made of a similar but more recent initiative in New Zealand. Here, the aim was to identify priorities for the 'Public Good Science Fund' (PGSF)[230] over the next five to ten years. Again, a formal assessment framework was used. This had four elements: (i) determining the scale of PGSF funds likely to be available; (ii) examining how those funds should be distributed among 40 scientific fields; (iii) identifying 'thematic' scientific and technological priorities (relating to the purposes of the research); and (iv) establishing 'structural' priorities (relating to the way research programmes are structured and supported). To determine the distribution of PGSF funds, five selection criteria were used: (i) strategic importance and socio-economic benefits; (ii) ability to capture the benefits (through technology adoption and diffusion); (iii) research potential; (iv) research capacity; and (v) appropriateness of government funding.[231] Information was compiled for each scientific field and each criterion.

The starting point in developing a suitable process for carrying out the exercise was

> the key principle ... that priorities should be developed in a rational, systematic, transparent and comprehensive manner using a structured methodology based on inputs from both community-wide consultation and comprehensive analysis of national and international information on science and technology.[232]

The first stage in the process consisted of widespread consultation with important stakeholders (research funders, performers and users) within individual sectors. In each sector, a convenor was appointed to facilitate the process and develop an 'information package' on priorities for government research funding in that area. The information packages were all presented to an expert panel at a 'Priorities Forum' in April 1992. The panel's task was to synthesize the information, hold discussions with sector groups and arrive at a set of recommended priorities. Based on the work of the panel, the Ministry of Research, Science and Technology prepared a summary document for circulation and public discussion. Once all the feedback has been obtained, the Ministry will produce a priority statement to be submitted for approval through the political process.[233]

Although this last exercise is not yet complete, certain comments can be made about the approach. First, like the CSIRO study, it uses a set of explicit selection criteria covering the main inputs to foresight identified in Section 3.1. Second, it recognises that the foresight process **is at least as important as the results. In particular, it has encouraged widespread consultation with stakeholders, thereby contributing to the development of consensus on longer-term research priorities. Finally, it avoids the danger of linking foresight too closely with funding decisions.**[234]

10. Criteria for Selecting Generic Technologies

This section tries to synthesize the lessons which emerge from this brief analysis of RFA in other countries (and indeed from the examples of foresight in the UK) on the criteria to be used in selecting emerging generic technologies. The discussion falls into two main parts: the first is a preliminary attempt to clarify exactly what is meant by such terms as 'generic', 'emerging' and indeed 'technology'. Second, the types of criteria that must be taken into account in any foresight exercise are considered.

10.1 Definitions and conceptual distinctions

In attempting to assimilate the lessons from all the above examples of RFA, one is faced, first of all, with a problem of definitions. Those engaged in different exercises have used a vast variety of adjectives to describe or categorise important new technologies. These include:

- emerging (or emergent or new)
- generic
- pervasive
- critical (or key)
- strategic
- enabling (or facilitating)
- fundamental (or basic)
- core
- pre-competitive
- dual-use (ie applications in both military and civilian sectors)

and so on. A further complication is that different people may use the same adjective in different ways. For example, the term 'generic' can have at least three distinct meanings:

(1) a class of closely related technologies;

(2) a technology the development of which will have implications across a range of other technologies;

(3) a technology the exploitation of which will yield benefits for a wide range of sectors of the economy and/or society.

One particular technology may happen to meet, say, definitions (1) and (3) but this does not necessarily mean that all **type (1) technologies will also fall into category (3).**

In short, before any attempt is made to identify 'emerging generic technologies', a degree of conceptual clarification is called for, along with the drawing up of explicit definitions to be adhered to by all the participants in the foresight effort. Of the three possible definition of 'generic' put forward above, for example, one could argue that definition (1) represents the strict or traditional definition of 'generic'. If so, alternative adjectives must be attached to definitions (2) and (3). The most appropriate for definition (2) is perhaps 'enabling' (or 'facilitating') while for definition (3) one might use the term 'pervasive'. However, it seems to be becoming increasingly common to attach definition (3) to the term 'generic' and this convention will therefore be adhere to here.

As far as the UK Government's 'modest enhancement' exercise is concerned, what is probably of most interest is category (3). One reason for assuming this is that these will tend to be more disaggregated or specific than category (1) technologies and therefore more useful for policy purposes. In addition, it is precisely for technologies likely to yield benefits across a range of sectors that the 'market failure' rationale can be used to justify some government support in the early stages of their development.

The next definition to be considered is that for an 'emerging' technology. Here, there is perhaps less ambiguity in the way the term has been used by different people. A possible working definition is as follows:

> An emerging technology is one in which the research has progressed far enough to indicate a high probability of technical success for new products and applications that might have substantial markets within approximately 10 years.[235]

An important point to note about this definition is that, if exploitation and commercial innovations are some ten years off, then such a technology is almost certainly at a 'pre-competitive' stage. Again, therefore, the 'market failure' rationale can be invoked to justify government support. Public sector support might be used to stimulate companies to collaborate with each other until prospects for specific new products or processes emerge and the work ceases to be pre-competitive.[236]

The third definitional question to be addressed here is perhaps the most difficult - namely, what is a 'technology'? In particular, what should be the degree of disaggregation or 'granularity'? If the end-result is to be a list of technologies on which to concentrate research resources, the granularity must be fine enough to yield specific policy implications. But it must not be so fine that it generates an inordinately long and complex list and requires excessive amounts of effort to compile all the information needed to appraise each and every technology. So the 'right' degree of disaggregation is partly a question of what is practical with the resources and time available for the foresight exercise.

In addition, however, it will depend on the nature of the exercise. For an attempt to develop a national or 'holistic' overview of the entire range of science and technology, then experiences in the US and more recently in Germany suggest that one needs to start with a list of the order of 100 technologies (say, to within a factor of two). If so, it then becomes a task of breaking up technology into units of such a size that about 100 of them will cover the entire spectrum. In the subsequent evaluation procedure, the goal might then be to narrow that down to one fifth this number (as in the US exercises). However, for foresight covering a narrower range of technology (for example, by an industrial association) or for a smaller country, then the number of priorities to be eventually chosen might be substantially smaller.

10.2 Criteria for selecting emerging generic technologies

In the foresight exercises in other countries described above and in those conducted by British companies, an enormous variety of criteria have been employed. To add to the confusion, even where similar terms have been used, they have perhaps been based on different definitions. The task of the analyst is to attempt to bring order to this confusion by classifying the various criteria into a number of more manageable categories. Here, the model first proposed by SPRU and since used by others such as CSIRO and the New Zealand Ministry of Research, Science and Technology appears as good as any.

According to this (as was seen in Section 3.1), in order to identify emerging technologies and the areas of strategic research on which they will be based, one needs to consider both science-push and demand-pull. Each of these can then be split into two sub-categories corresponding to (i) external opportunities and threats and (ii) internal strengths and weaknesses. One thus arrives at four main sets of criteria:

(1) What are the economic or social demands/opportunities that will be met (or the benefits that will be derived) from the new technology?

(2) What comparative strengths or weaknesses will affect the ability of, say, the UK to exploit those opportunities ahead of other countries?

(3) What are the scientific opportunities - the advances that may make possible new developments to the technology?

(4) What are the scientific strengths and weaknesses of the UK (assuming that is the unit of analysis) that will affect its ability to take advantage of the scientific opportunities compared with other countries?

To these four, one should add a fifth, the all-important question of cost:

(5) What will be the cost of carrying out the strategic research and subsequent development needed before one can exploit the economic or social benefits of the particular technology? And what human resources and skills will be required?

These broad criteria, or something close to them, have been used in several foresight exercises. In the CSIRO study, for example, criteria (1) and (2) were combined to yield an 'attractiveness' rating, while criteria (3) and (4) yielded a 'feasibility' index. In the example of the second company described in Section 8.1 above, the three main criteria were 'importance to the company' (somewhat similar to a combination of criteria (1) and (2)), 'feasibility' (criteria (3) and (4) combined), and cost (in person years). In both cases, this enabled the results for different candidate fields to be plotted on a simple two or three-dimensional representation.

The merit of using the above five criteria lies in their relative simplicity. In practice, however, some at least of these broad criteria may need to be subdivided before they can be accurately assessed. For example, criterion (1) might first be split into (a) economic benefits and (b) social benefits. The former might then be further subdivided into different sectors (eg manufacturing, services etc.) or even more finely still (eg chemical manufacturing). Likewise, the latter might be broken down into defence, health, environment, and so on. For each subsector, one would then have to assess the likely magnitude of the benefit from the technology.

Criterion (2) might similarly need to be disaggregated since a wide range of factors may be involved. One is the international competitiveness of different industrial sectors (for example, chemicals and pharmaceuticals compared with IT). A second is current (and future) government policy (towards industry, education and training, environmental regulation, health and so on). Third, what natural advantages or limitations does the country face (eg raw materials, geography)? Fourth, are there any social or cultural constraints or advantages (such as language) that will affect the UK's ability to compete?

To take one more example, criterion (4) can be subdivided into such issues as: (i) existing research strengths and weaknesses; (ii) the breadth and quality of S&T skills available now and in the future; (iii) scientific facilities, instrumentation and capital equipment needs; (iv) the financial resources likely to be available; and (v) the adequacy of the scientific infrastructure.[237]

Clearly, one can keep on subdividing the criteria almost *ad infinitum*. However, one has to be practical. The criteria must be kept to a manageable number - perhaps five to ten. Then, for each of the criteria chosen, a clear definition needs to be drawn up. As part of this, one might indicate some of the possible sub-categories that need to be taken into account even if no attempt is made to score each technology on each of those sub-categories. Provided the criteria cover the main elements listed above, the final set of emerging technologies that they eventually lead to should not be too sensitive to the exact set of criteria chosen. (It might, however, be worth experimenting on a small scale to test this proposition by using two different sets of criteria and definitions, and comparing the short-lists of technologies which they generate.)

11. Conclusions and Recommendations

This final section brings together the main findings from the study. It looks first at what has been achieved in the area of research foresight in the UK since 1986 and compares this with the aspirations of the 1986 ACARD report. Next, there is a summary of developments in other countries and the lessons to be drawn in establishing criteria for identifying emerging generic technologies. The section concludes with some preliminary ideas as to what might be done next in Britain.

11.1 The UK

The report has discussed how in 1986 the Advisory Council for Applied Research and Development called for the establishment of a process to identify priorities for publicly funded research in order to stimulate its effective exploitation to the UK's benefit. The Council argued that the creation of a forum to manage this process was a "national priority". However, it drew back from suggesting that government should take the lead. Instead, it suggested that the task could be left largely to industry.

Two years later CEST was set up, funded primarily (as ACARD had suggested) by companies. However, after one early and not very successful attempt to combine science-push and demand-pull perspectives, CEST has concentrated not so much on identifying exploitable areas of science as on helping member companies and others to exploit existing technologies. In other words, CEST studies have focused on first determining industry's needs and then on finding suitable technologies to meet them. Little of CEST's later work can be described as research foresight or as the determination of priorities for strategic research. Nevertheless, the Centre is certainly very good at generating commitment in industry to implement the results of its work. This orientation seems unlikely to change while CEST remains dependent on industrial members for 80 per cent of its funds, although the recently launched 'Environment Foresight' project may indicate some shift in emphasis.

ACOST, which took over from ACARD in 1987, has the task of advising Government on general priorities for science and technology. Part of this remit has been concerned with monitoring emerging technologies, and ACOST delegated responsibility for this task to the Emerging Technologies Committee. The Committee have produced four reports on individual technologies. Some of these have made a reasonable attempt to link science-push and demand-pull factors. However, there has been little systematic foresight, no overview of the whole of British S&T with a view to identifying the most important emerging technologies and strategic research priorities, and little success in generating the commitment in the scientific community and industry needed to exploit the results of the Committee's work. Instead, there has been a tendency to suggest that others, such as CEST, should be looked to for further efforts. However, the replacement of the ET Committee by the Working Group on Emerging and Generic Technologies may herald a new resolution to take up the challenge of research foresight and establish priorities for the whole of British S&T.

Elsewhere in the public sector, there have been a few, isolated and not particularly successful attempts at research foresight over the last few years. The ABRC has instituted an annual procedure for discussing scientific opportunities for the next decade. However, they have been unable to develop a unified overview of British science, largely because of relying on the five Research Councils for inputs.

Individual Research Councils have engaged in some foresight-related activities in preparing Forward Looks and Corporate Plans, and in a few cases specific priorities have been determined. However, they have not adopted a particularly systematic approach, nor have they devoted much attention to demand-pull considerations.[238]

The DTI has set up the Longer Term Studies Unit and Group in an attempt to instil longer-term thinking in the Department. These have contributed to some interesting developments, for example in moving towards a more systematic approach to preparing the Forward Looks. There have also been other longer-term studies in which the DTI has been directly or indirectly involved such as the strategy document prepared by the Biotechnology Joint Advisory Board. Although a relatively modest foresight exercise, this was nevertheless quite successful in bringing together science-push and demand-pull factors, in linking these both to the ability of UK companies to exploit the new technologies and to the all important issue of skills, and in identifying overall priorities. Furthermore, the approach adopted succeeded in involving all the relevant 'stakeholders' from an early stage, thereby generating the commitment needed to ensure the final recommendations are implemented. Other Joint Advisory Boards might be encouraged to follow BJAB's lead.

The Energy Technology Support Unit has developed a systematic approach for appraising the prospects for different energy technologies. This combines a detailed analysis of demand-pull considerations and the factors likely to affect exploitation of different technologies, on the one hand, with an evaluation of existing R&D programmes to pinpoint their strengths and weaknesses, on the other. However, the approach can perhaps be criticised for relying too heavily on in-house analysis by ETSU and for failing to involve scientists, companies and other stakeholders more fully in the process. As for other Government Departments, the level of RFA is apparently not very great. In MAFF and the MOD, there are a number of foresight-related activities, and the DOE has recently launched an interesting foresight experiment with CEST. However, they are all still some way from establishing an explicit, systematic and comprehensive research foresight system.

Among large UK science-based companies, around half those interviewed have undertaken a medium or longer-term foresight exercise in recent years. The common feature is the use of an explicit set of criteria to assess different S&T areas and identify priorities. Those criteria often relate quite closely to the five main criteria discussed in Section 10.2 - in other words:

(1) market needs or opportunities for new products or processes;

(2) the comparative ability of the company to exploit those opportunities ahead of rivals;

(3) emerging scientific opportunities;

(4) the companies scientific strengths and weaknesses compared with competitors, especially in relation to available skills;

(5) the costs and probability of success.

To assess all the different R&D options in terms of such criteria, companies use experts on both science-push (their own researchers and academics) and demand-pull (eg business divisions). In some cases, they may employ a formal system to score each possible S&T field against the different criteria and then aggregate the results. Such a process generally leads to a clear set of priorities and preferred options for each S&T area (eg withdraw, monitor, participate or lead). The end-result is frequently a concentration of research resources on a small number of areas of company strength.

In addition, companies devote much attention to monitoring external research activities. This is seen as vitally important because the great majority of research is conducted elsewhere. The aim is to obtain an early warning of important S&T developments. A variety of formal and informal 'science watch' procedures are employed to achieve this. The results are then used to steer or fine-tune the company's R&D programme.

Consultancies and academic groups are also engaged in RFA. In particular, what is probably the most systematic example of research foresight in the UK is the study by PRISM on cardiovascular research.

To sum up the situation in the UK, there are certainly interesting examples of research foresight going on. However, these are carried out in isolation from one another. There is no interaction between foresight at different levels. This is not the case elsewhere. For example, in Japan and now Germany and the US, foresight exercises take place within companies (micro-level), within industrial associations (meso-level), within individual government departments or funding agencies (macro-level), and spanning all research ('holistic' foresight).[239] Furthermore, there is some integration between the different levels, with foresight at one level using results from, and feeding results into, foresight at other levels. In Britain, there is no such integration. In particular, there has been no attempt so far to produce a holistic overview of science and technology. Yet without this, it is impossible to analyse the interactions between different technologies - that is, the potential for technological 'fusion'. There is also too much reliance on the private sector to carry out all the foresight needed. And there is no national forum for identifying priority areas of exploitable science in a systematic manner and generating a process to exploit them. ACARD's aspirations of 1986 remain largely unfulfilled.

11.2 Other countries

This study has deliberately focused on two countries where there is a decentralised approach to S&T policy (and policy-making in general) and where there was little enthusiasm for research foresight three or four years ago at the time of the previous SPRU study. In both, there has since been an upsurge of interest in foresight. RFA is now much more extensive than in the UK and it is taking place at the various levels identified above. The motivation for this change in attitude is, however, rather different in the two cases. In Germany, it is largely a question of money, with the unexpectedly high cost of re-unification imposing the need for sharper priorities. In the United States, it is more a question of concern about declining economic and technological competitiveness compared with countries like Japan (and indeed Germany) with their more explicit and coherent technology policies.

In Germany, research foresight is still at an early stage. However, there has already been one exercise as a result of which the Federal Government is now contemplating a fundamental shift in the balance between physical and biological sciences. Two other foresight exercises are just beginning. Both are experimental. And both could be tried out in the UK. In the first, ISI Karlsruhe and NISTEP in Tokyo are collaborating to extend the fifth Japanese 30-year S&T forecast to Germany. There is no reason why Britain could not join in this partnership. As for the other which involves BMFT research agencies, the DTI might also embark on a similar exercise, perhaps even adopting the same methodology (see Section 11.4 below). Interest in research foresight is also flourishing elsewhere in Germany - in industrial associations like VCI, in companies[240] and (very recently) in the Science Council.

In the United States, there have been several attempts to draw up a list of critical technologies. Industrial associations led the way, followed by government departments (Defense and Commerce) and then the OSTP National Critical Technologies Panel. The approach adopted in these is by no means perfect. There is little use of empirical information. The mechanism is too 'top-down' in orientation, with not enough participation of the research community, industry and other stakeholders. Nevertheless, the studies do employ an explicit set of criteria (perhaps not in a very formal way). And they do generate discussion and increased interest in research foresight and priority-setting. A more strategic approach is now beginning to spread to other agencies - for example, to the National Institutes of Health who were previously sceptical or even hostile towards the notion of longer-term foresight (although here again there are some problems because of a tendency to adopt too 'top-down' an approach).

Interest in RFA also appears to be on the increase in other countries. One example is the Netherlands where the Ministry of Economic Affairs has completed a 'Technology Foresight Experiment'. This represents one of the most thorough foresight exercises in Europe over recent years. It demonstrated that foresight is a useful tool for strategic management as well as stimulating communication between different stakeholders. A second example is Australia where previous attempts at research foresight met with mixed success. In 1990, CSIRO carried out a large-scale foresight exercise. Like the Dutch initiative, it incorporated clear selection criteria and arrived at specific research priorities. It also generated discussion between researchers and policy-makers although research users were less involved. A third example is New Zealand where a major foresight study has been launched by the Ministry of Research, Science and Technology. This uses a set of selection criteria very similar to those employed by CSIRO and virtually identical to those listed in Section 10.2. And as with the Dutch and Australian exercises, it is clearly recognised that the foresight process **is as important as the results so widespread consultation with stakeholders is being sought.**

11.3 Criteria for selecting strategic research areas and generic technologies

As was discussed in Section 10, the lessons from previous surveys of research foresight, from the UK companies studied here and from recent experiences in Germany, the US and elsewhere all suggest that there are perhaps five main types of criteria to be considered in selecting strategic research priorities and emerging technologies:

(1) demand-pull opportunities - the likely economic and social benefits;

(2) factors affecting a country's ability to exploit those opportunities - economic and social strengths or weaknesses (eg industrial competitiveness);

(3) science-push opportunities;

(4) factors affecting a country's ability to take advantage of those scientific opportunities - research strengths and weaknesses, especially those relating to skills and the human resource base;

(5) costs.

In individual foresight exercises, some of these broad criteria may need to be broken down more finely - for example, criterion (1) may be disaggregated into economic and social benefits, and each of these may then be further subdivided. However, if there are too many criteria, it becomes more difficult and time-consuming to obtain all the data needed to put them into practice. It also becomes far harder to aggregate the scores and compare different scientific or technological areas. In short, one needs to strike a balance between comprehensiveness and accuracy, on the one hand, and simplicity and practicality, on the other.

11.4 Options for future work on research foresight

This final section puts forward some suggestions for future work on RFA. In the time available, there has been little opportunity to sound out reactions to the possibilities outlined here. They should therefore be regarded as no more than ideas for discussion - a starting point for debate.

The first possibility is to embark on a national critical technologies exercise. Several approaches are possible here. In the US, the approach has been to rely largely on a single, relatively small panel. Those studies are not very systematic and make little use of data. However, they are cheap and fairly quick to carry out. In Germany, by contrast, the approach involves tapping the tacit knowledge of several existing research agencies or committees, using a formal methodology to link all the information together. One option for the UK would be for the DTI to tap the knowledge of its advisory committees, including those for its R&D agencies and laboratories, together with the Joint Advisory Boards. A more ambitious approach would involve bringing in other committees as well - for example the ACOST Working Group on Emerging and Generic Technologies, SERC Boards (to ensure that 'science-push' was fully represented), the MRC Strategy Committee, the MAFF R&D Priorities Board and so on. Such an exercise would need to be carefully co-ordinated, with participating groups being provided with a methodology, criteria and relevant data (as ISI Karlsruhe is doing in the BMFT study).

Second, Britain could join Germany and Japan in conducting a long-term forecast of S&T using a 'Delphi' survey of a large sample of active researchers in industry, government and universities. The advantages are that this would be relatively inexpensive (Japan has already prepared a suitable set of questions) and it would enable Britain to learn from the experiences of others.

A third suggestion is that the ABRC (perhaps with ACOST) might set up a committee to review the science base and the balance of effort in UK research. This could be modelled on the BMFT exercise. However, such a committee must be independent of the existing Research Council structure. Its members would need to be chosen as individuals (with a good overview of science and an interest in longer-term strategy), not as representatives of the five Research Councils. Nor should the committee be overly dependent on Research Councils for information inputs.

Fourth, the DTI (and other Departments) could encourage the Joint Advisory Boards and other advisory groups to produce sector-wide strategy documents similar to that prepared by BJAB. Such strategy exercises need to be repeated every three years or so. There could also be some attempt to bring them all together at a workshop or a series of panel meetings and an effort made to integrate their conclusions into a more macro-level strategy.

A fifth option stems from one of the main lessons from companies - namely, the need for a 'science watch' capability. Britain should begin to adopt a more professional approach to monitoring the 95 per cent or so of the world's research not carried out in this country. What is perhaps required is a national science-watch 'observatory' to organise the collection of this information, synthesize it and ensure it is effectively disseminated to all those who might benefit. The first step here would be to review the science-watch procedures adopted in other countries (for example, in France where there is an Observatory of Science and Technology funded by several departments and an Observatory of Strategic Technologies within the Ministry of Industry[241]) and by companies.

A sixth option would be to create a group specifically responsible for research foresight and the identification of emerging technologies. This might either be a new organisation (as in the United States where they are planning to set up a National Critical Technologies Institute) or it might build on an existing science and technology policy centre (as they have done in Germany with ISI Karlsruhe). Alternatively, CEST could be re-oriented to this task, although this would almost certainly require a change in the way the centre is funded, with government assuming a larger responsibility. Such a foresight group would have a number of tasks. It would develop foresight methodologies and provide guidance and expertise to others undertaking foresight exercises. It might also conduct or co-ordinate 'holistic' foresight initiatives of the type currently lacking in Britain.

Finally, work is required to establish what are the best means available to government for stimulating research foresight elsewhere, especially by industrial associations or more informal groups of companies. This would involve looking at experiences in countries such as Germany, Japan and the US. It is clear that one cannot leave responsibility for foresight entirely to the private sector. Foresight is difficult to carry out successfully, especially the first time it is attempted. There is a long 'learning curve'. This means that there is an initial hurdle to be overcome before companies, industrial associations and others come to see the benefits of foresight and are willing to support it themselves. The task in this last option would be to establish how government can best get companies and others over that initial barrier so that research foresight ultimately becomes self-sustaining.

APPENDIX I

Notes

1. ACARD (1986, p.7).

2. SPRU was commissioned to survey the approaches adopted in France, Germany, Japan and the United States for looking into the longer-term future of science and technology in order to identify promising areas of research. The report to ACARD (Irvine and Martin, 1983) was subsequently published as a book (Irvine and Martin, 1984).

3. ACARD (1986, p.9). The report goes on to outline a framework for the process of generating strategic research priorities. Among the questions to be addressed are: "(i) which areas of generic technology are supported by a particular area of strategic science? Has the UK the scientific resources to advance a particular area of strategic research? For a given generic technology, what new products or processes will become possible within 10-20 years? What are the likely costs of translating scientific knowledge into marketable products and processes?" (*ibid*., p.11) The report also identifies four key elements in the process of identifying exploitable areas of science: "a. the gathering of information on a continuing and permanent basis and its communication to the relevant parties and bodies; b. the evaluation of relevant opinions and information, and the identification of exploitable scientific areas; c. the allocation of resources to the priority areas in science; d. the commitment to exploit the results of science to UK benefit" (*ibid*., p.42). The report notes that "the information necessary to identify exploitable areas of science is acquired at present in a fragmented fashion in the UK. A number of bodies such as ACARD, the ABRC, Royal Society and UGC, together with industry all play a role but rarely do they interact as a combined force to shape policy and direction. ... A structure is required which can gather, analyse, prioritise, and direct relevant information into the decision-making machinery" (ibid.).

4. *Ibid*., p.45.

5. See e.g. ACOST (1988, inside cover).

6. It covered eight countries rather than four and was more detailed and analytical.

7. OECD have also carried out a survey of RFA and priority-setting in S&T. This covers the same countries as the SPRU study together with the UK, Netherlands and Finland. It arrives at broadly similar conclusions. For example, it argues that "the setting of science and technology priorities is essentially a complex political process involving many people who interact with one another. It is not a case of science-push or demand-pull, but a changing combination of the two ... [The] process of selecting science priorities is that of the dialectic between the internal logic of scientific knowledge and that of the needs of the economy and society" (OECD, 1991, p.7). More recently, OECD have produced a discussion paper on 'Research Foresight for Megascience'. This summarises current RFA in the area of 'big science' and puts forward proposals for achieving closer international collaboration (OECD, 1992).

8. The approach of *la prospective* has been pioneered by Godet (eg 1986).

9. 'Strategic research' is defined here as "basic research carried out with the expectation that it will produce a broad base of knowledge likely to form the background to the solution of recognised current or future practical problems" (*ibid.*, p.4).

10. Irvine and Martin (1984, p.144).

11. See Martin and Irvine (1989, pp.29-39).

12. Strategic planning based on an examination of Strengths, Weaknesses, Opportunities and Threats is sometimes termed 'SWOT' analysis.

13. Nelson and Levin (1986).

14. See, for example, Rosenberg (1985).

15. Senker and Faulkner (1991).

16. Schwarz, Irvine, Martin, Pavitt and Rothwell (1982, p.164).

17. Pavitt (1991).

18. Kodama's book, *Analyzing Japanese High Technologies: The Techno-Paradigm Shift*, won the 1991 Sakuzo Yoshino Prize, Japan's highest award for social science books.

19. A similar point was made a few years earlier by Irvine and Martin (1984, p.25) who pointed out that "the synthesis or confluence of previously distinct lines of research" often generates the most important innovations.

20. Kodama (1992, p.70).

21. Cabinet Committee on Science and Technology (1987).

22. ACOST (1989, para.4).

23. An earlier study on optoelectronics was carried out by a Working Group set up by ACARD and 'inherited' by ACOST in 1987 (see ACOST, 1988).

24. It includes an interesting discussion of non-technical factors influencing future developments such as public concern, safety and ethics (see ACOST, 1990, pp.22-29).

25. For example, the first recommendation is that "Government departments should take a more pro-active role in biotechnology", while another is that "Inter-Research Council co-ordination of biotechnology should be strengthened" (*ibid.*, p.viii).

26. See ACOST (1990, especially pp.viii-ix).

27. Only one of the nine members came from a manufacturing company (see ACOST, 1991, p.18).

28. *Ibid.*, p.12.

29. "The areas identified as offering the greatest potential for immediate technical advances are based largely on computers and information technology. They include: continuous flow manufacturing..., computer integrated manufacturing ..., automation and robotics ... and electronic data interchange" (*ibid.*, p.11).

30. For example, "means should be found to bring these contributions together" and "The level of AMT awareness in companies needs to be raised" (*ibid.*, p.15).

31. There were three academics and three industrialists on the sub-group.

32. ACOST (1992a, p.23).

33. For example, a dozen industrialists made contributions at a workshop (*ibid.*, p.25).

34. *Ibid.*, p.13.

35. Like other ACOST reports, it was prepared by a small sub-group of the ET Committee which in this case consisted of just one consultant and two academics (ACOST, 1992b, Appendix C).

36. *Ibid*., p.13.

37. Whelan (1991, p.3).

38. Cheese (1990).

39. *Ibid*., p.1.

40. SEPSU (1990, Executive Summary).

41. Most of the population consisted of university scientists. Only 8 per cent were industrial researchers, a smaller proportion than those who were retired (10 per cent) - see *ibid*., p.2.

42. *Ibid*., p.1.

43. *Ibid*., p.10. Indeed, "a significant portion of the sample showed no interest in commercialisation" (*ibid*., p.25).

44. *Ibid*., p.9 (emphasis added).

45. This was recognised with hindsight by SEPSU, who noted that "the task of mapping research areas onto areas of potential application may best be approached from both perspectives at once" (*ibid*., p.12).

46. Roy *et al*. (1991).

47. Other themes which have since been studied include demography and social change, and environmental technologies. In the latter, the initial analysis focused on 13 environmental problems. For each, CEST commissioned external experts to produce a report identifying products and industries giving rise to the problem, legislative trends, technologies likely to solve the problem and the size of the associated markets. Using a matrix linking environmental issues with industrial sectors, CEST determined which environmental issues are most pervasive. Three of the largest markets were foreseen for 'greenhouse gas' reduction, water quality and waste management. Projects were set up to work with industry in these areas with the aim of gaining an understanding of their perception of environmental problems and helping them to exploit the opportunities. Specialist groups of industrialists, government officials and academics then identified promising technologies and determined how best to exploit them (Good, 1991). Again, some of the groups have generated significant interest such as the one for the automotive sector where companies are now working together to shape legislation and to share results from research programmes.

    More recently, CEST has begun work on a new theme - the interaction of transport and communications technologies. The approach here is slightly different, with CEST doing less preliminary analysis and instead bringing in industrial participants and others right from the start to create a network for discussion (Cheese and Segal, 1992). Three areas have been chosen for detailed analysis (based on the interests of the companies involved and where CEST feels that it can contribute most) and working groups established. The objective is to identify market opportunities and strategies for their exploitation. Science-push considerations scarcely feature in this exercise.

48. Mason (1992, p.1). Although neither research nor the scientific community is specifically mentioned in this objective or in the accompanying list of six sub-goals, researchers are nevertheless being closely involved in both phases of the project.

49. Ibid., p.3.

50. Even there, it rather ducks the issue of research priorities, suggesting that a new foresight body be established to carry out this task (Roy *et al*., p.39).

51. The report on environmental technologies explicitly argues that, because of the wide range of problems and applications, it is pointless to identify a short-list of priorities (Good, 1991, p.26) - indeed, "virtually all branches of science and technology" will be needed (ibid., p.48).

52. ABRC (1990).

53. *Ibid*.

54. ABRC (1991b).

55. SERC (1991a).

56. The five submissions are attached to ABRC (1991a).

57. ABRC (1991b).

58. This is a general problem for the ABRC because of the limited amount of independent information that they can draw upon from outside the Research Councils.

59. In the case of the SERC, for example, the exercise had little impact. Only in a couple of instances did it perhaps raise the profile of a topic (eg liquid-solid interfaces).

60. See SERC (1987).

61. Another example is the 10-year strategy prepared by the particle physics committee.

62. Two recent examples are the strategy documents for mathematics (SERC, 1991b) and electro-mechanical engineering (SERC, 1992).

63. The SERC does have a Strategy Planning Division whose task is to provide Council with information enabling it to take broader strategic decisions. It carries out analyses of funding levels and has engaged in some international comparisons. However, there has been little in the way of formal RFA.

64. There was some criticism of CEST, especially in its earlier years, for failing to consult adequately with SERC in connection with research areas where the Council has a major responsibility.

65. The AFRC also makes use of Corporate Plans. Each year, Council carries out an assessment of current programmes, reviews progress in implementing the current Corporate Plan and identifies new priority areas. Other inputs come from the Forward Looks of AFRC research committees who try to identify priorities for their area. This process lays the basis for the new Corporate Plan and for the Forward Look. The AFRC is unusual in that it has taken the bold step of identifying areas of lower priority (eg descriptive studies unrelated to organism function) in its current Corporate Plan as well as higher priorities - see the AFRC submission to the ABRC (1991a).

66. NERC (1992).

67. The NERC has gradually been adopting a more systematic approach to research assessment, using indicators to complement peer-review (*ibid*., p.35). The evaluation results then represent another input to the corporate planning process.

68. It also gives examples where recent NERC work has helped meet national needs (*ibid*., p.9).

69. These strategies are now approaching their mid-life and the NERC plans to update them shortly (ibid., p.19).

70. *Ibid*., pp.4-5.

71. In addition, the LTSU takes the lead for the DTI in liaising with CEST and ACOST.

72. One example is the study carried out jointly by the LTSU and the Laboratory of the Government Chemist (LGC) on controlled-release technology (CRT). The aim was to identify UK strengths and weaknesses in research, technology, products and markets, together with the main trends and future scenarios. The first step was to establish which are the main companies and research groups working in the area through a literature review and database searches. Interviews were then carried out with a range of research organisations, companies and other experts. Views were sought on the existing research base, current applications of CRT, the dominant market forces, potential growth areas and the problems presently faced (for example, in technology transfer). Information was also obtained from Europe, Japan and the US to assess international trends. The results showed that Britain is strong in R&D on CRT for pharmaceuticals, the sector where the largest markets are to be found at present. However, the highest growth potential may be in non-pharmaceutical applications, an area where the UK is currently weaker. A report (DTI, 1991) containing the results was circulated to approximately 100 industrialists. As with most DTI studies, more emphasis was given to demand-pull than science-push considerations. There was little use of data and a heavy reliance on subjective views. The recommendations were rather general (eg "HEIs, Industry and DTI need to work together to encourage much greater technology transfer" - see *ibid*., p.6) and no clear research priorities emerged.

73. A Forward Look procedure was introduced that year for the Innovation Budget. A similar standard format to the MTS Forward Looks was adopted although Divisions were also asked to give consideration to budgetary cuts or increases of 15 per cent and 30 per cent. The Forward Looks were then discussed at the Innovation Budget Priority Setting Forum. One point to emerge from that meeting was the need for clear selection criteria. RTP were requested to consider possible criteria, taking advice from the Technology Policy Committee and others.

74. One criticism from Divisions was the need to prepare separate Forward Looks for the Innovation and MTS Budgets. The possibility of merging the two exercises is therefore being considered.

75. RTP (1987, para 2).

76. To give one example, the analysis revealed that UK competitiveness was declining in electric motors. It also identified product areas of UK strength and weakness, and showed that the British science base is characterised by a low level of activity, fragmentation and little collaboration between universities and companies (*ibid*., paras 7-10).

77. *Ibid*., para 16.

78. *Ibid*., para 17.

79. This obviously begs the question of how the priorities were to be identified in advance of the studies.

80. Another factor was that the matrix analysis was pushed within the DTI by one enthusiastic individual; when he left, the work stopped.

81. The Information Technology Advisory Board is currently preparing a five-year rolling vision which should be ready at the end of 1992.

82. BJAB was set up in 1989 by the DTI and SERC. The AFRC and NERC joined later, followed by the MRC.

83. BJAB (1991, p.6).

84. In the report on the environment sector, for example, 15 research priorities were identified and ranked in order (in four groups) (*ibid*.,p.25).

85. The original intention was to carry out formal international comparisons but this did not prove possible in the time available. Instead, the final report classifies UK industrial strength in the different sectors into three broad categories (high, medium and low) on the basis of a subjective analysis of the information available.

86. The membership of BJAB is fairly evenly split between academics and industrialists.

87. One exception is the Forward Look process for the Innovation and MTS budgets where plans for all parts of the Department with R&D responsibilities are considered together.

88. Besides the strategic studies, ETSU also conducts energy technology analyses for the Energy Technology Division in the DTI and energy efficiency studies for the Energy Efficiency Office (now part of the Department of the Environment).

89. Department of Energy (1987a, p.1).

90. *Ibid.*, p.v. It was also a response to a recommendation by the House of Commons Select Committee on Energy for a comprehensive review of priorities (*ibid.*, p.1).

91. The bulk of the work was done by ETSU staff including some specifically recruited or seconded for the study. There were other inputs from the Steering Group and Advisory Group. The project took half a dozen people two years to complete.

92. ETSU (1992c, p.1).

93. Technologies were also graded in terms of the time-scale for their uptake, the magnitude of their probable economic contribution and whether they were likely to constrained by institutional, environmental or safety factors.

94. Department of Energy (1987a, pp.14-15).

95. *Ibid.*, pp.20-22.

96. The voluminous background documents on individual technologies were published separately (Department of Energy, 1987b).

97. Department of Energy (1987, pp.23-25).

98. They all assumed energy prices would rise in real terms (ETSU, 1992c, p.1).

99. Since then, the composition of ACORD has changed, with more retired members who are able to take a broader view and are less likely to engage in lobbying. However, the disadvantage is that they can offer less help in arranging access to information.

100. Department of Energy (1987, p.v).

101. ETSU (1992a, p.1). More specific aims are to provide an up-to-date view on different technologies, their prospects of deployment in the UK up to 2025, the environmental impact, current UK and overseas RDD&D programmes, and the value and technical content of those programmes (*ibid.*).

102. Of the eight main technology groups, four deal with energy supply and four with energy efficiency (ETSU, 1992a, p.13).

103. The scenarios were chosen following a workshop of Department of Energy and ETSU staff. They include high oil prices, no drastic change, low oil prices, heightened environmental concern (two variations), and constant 1989 prices (ETSU, 1992a, p.6). A 'shifting sands' scenario incorporating two dramatic, unpredictable fluctuations in fuel prices over the next 40 years will also be used as a sensitivity test of the 'no dramatic change' scenario. For each scenario, a number of parameters have to be defined including fuel prices, demand, environmental concerns, availability of specific technologies and level of UK self-sufficiency.

104. The one chosen is the MARKAL model developed in the 1970s by an International Energy Agency (IEA) group. This allows the user to specify demand, the range of technologies available, fuels which may be imported, environmental indicators and the time horizon. A linear programming routine then selects the optimum mix of technologies (ETSU, 1992a, p.9).

105. Department of Energy programmes are to be subject to a cost-benefit evaluation, and there will also be a qualitative evaluation of other programmes in the UK and overseas in order to identify gaps (ETSU, 1992a, p.6).

106. *Ibid.*, p.3. In addition, because of the lack of previous experience at ETSU with scenario modelling, the help of an energy economist was sought who in turn drew upon the views of other experts.

107. The total effort required is likely to be approximately 18 person-years spread over two and a half years.

108. ETSU (1992a, p.3).

109. The data-collection phase was completed in mid-1992 when modelling began. It is expected that the analysis will be completed and results circulated at the end of 1992 (ETSU, 1992a, p.2)

110. The appraisal is covering potential contributions in the years 2000, 2010 and 2025.

111. There has been some discussion with a multinational company about their experiences with scenario analysis, but worries about commercial secrets limited the utility of this exchange.

112. *Ibid.*, p.17. For another analysis of technologies which could emerge into energy markets during the next decade, see Grubb and Walker (1992).

113. Previously, the Chief Scientists' Group was responsible for the overall R&D budget.

114. MAFF has a Chief Scientific Adviser, two Chief Scientists (for agriculture and food) and a Fisheries Scientific Adviser.

115. Each year, MAFF sets aside 5 per cent of its R&D budget to fund new ideas or areas of opportunity. Proposals are invited from researchers which are dealt with in a responsive mode. Of the 14 new topics funded in 1992, around half address urgent policy problems and the remainder focus on strategically important new research areas.

116. For fisheries R&D, there a different system, with the Aquaculture Group reporting to the Fisheries Group on the balance of the R&D programme and pointing to any gaps or opportunities.

117. MAFF (1991, p.3).

118. MAFF (1992a, b and c).

119. For example, the ASGs were not specifically asked to identify R&D priorities, although some did in fact attempt to do this.

120. One exception was a two-year study commissioned by MAFF in 1985 on the impact of technology on rural land-use patterns over the period 1985-2015. It was carried out by a land economist at Cambridge University. He conducted an extensive literature search, held discussions with experts in agriculture and land use, and visited agricultural research stations in the UK and US. He also discussed his ideas in 50 lectures (attended by over 10,000 people). His main conclusion was that up to 30 per cent of rural land would be released from agriculture by 2015 as a result of new technologies (North, 1987). It is not clear what impact this study had within MAFF.

121. It accounts for approximately 8 per cent of the budget of MOD defence research establishments, and some funds are also given to universities.

122. One technology may cross-cut several sub-components of the MOD research programme.

123. In the mid-1980s, the MOD conducted a review of critical technologies, looking in particular at which technologies needed to be developed indigenously. At the request of the Defence Research Committee, this exercise was being repeated in the latter part of 1992. The work is being carried out internally and draws upon the results of the other foresight-related activities mentioned earlier.

124. Over the last two years, the MRC have begun to give greater attention to longer-term issues and have set up a Strategy Committee. In addition, MRC Boards have been asked to prepare rolling five-year reviews. These are carried out by Working Groups of senior scientists and research managers. The task of the CV Working Group was to consider research priorities in the light of UK strengths and weaknesses. They wrote to British and foreign experts to seek their opinions. However, the group canvassed was somewhat restricted and the resulting report was little more than a collection of opinions, with hardly any reliable data and no analysis of non-scientific factors such as the effects of smoking habits on CV disease. The report did, however, call for a more co-ordinated and systematic approach to strategic planning to be adopted in the future. This provided the stimulus for the PRISM project.

125. Anderson (1990, p.1).

126. More specific aims include the following: (a) generating data on the present distribution of funds and manpower in British CV research and on the published output and impact of that research; (b) comparing these British statistics with those for France, Germany, Japan and the US; (c) constructing visions of possible futures in CV research through systematic surveys of a broad range of interested parties: (d) bringing together top-down and bottom-up approaches to priority setting; and (e) assessing the merit of the above approach as a process for increasing dialogue and cooperation between funding agencies, researchers and research users within the UK (Anderson, 1990).

127. The sponsors are the Wellcome Trust, MRC and the British Heart Foundation.

128. The statistics are being disaggregated by subfield, clinical orientation, support mechanism and institution.

129. It will probably prove impossible, however, to obtain internationally comparable funding data disaggregated to the level of CV research.

130. Preliminary results here suggest that Britain's relative position in CV research has been slipping overall, although it remains quite strong in surgery and diagnostics.

131. They were chosen following a 'brain-storming' session of PRISM staff and the Steering Group to identify the main areas and organisations to include. Various criteria were used to narrow down the list to about 20.

132. PRISM adapted a set of questions previously used by a large company in its scenario analyses.

133. See PRISM (1992).

134. For a description of how the bibliometric analysis was used to define the field of CV research, see Rogers and Anderson (1992). The Steering Group was initially sceptical about the utility of bibliometric analysis but, when they checked how well the publication database constructed by PRISM covered the field of CV research, they became more positive.

135. Another of the original worries of the Steering Group was how the PRISM analysts (none of whom are experts in CV research) could identify the 'right' people to ask about the future of the field. The bibliometric approach

provides a reasonably objective method for doing this, although it does mean that the sample is dominated by people with a science-push perspective.

136. A third concern of the Steering Group when the project was first proposed was how PRISM would know what were the 'right' questions to ask. The inclusion of the pre-Delphi interviews to identify key issues has overcome this worry.

137. Again, this procedure will give a fairly objective basis for choosing participants in the 131panel workshops. The alternative of using a committee to select experts could have introduced a bias into the sample.

138. These may include a 'reference' scenario, one of growth and another of shrinkage.

139. One of the main starting points for constructing the methodology for the study was the SPRU book on research foresight (i.e. Martin and Irvine, 1989).

140. As noted above, the demand-pull perspective is not represented in the Delphi survey because of the methodology used to choose the survey population. This is a potential weakness, although one that may well be overcome by the extensive involvement of 'users' in the interview and panel phases.

141. All three of the UK pharmaceutical companies studied by Taggart and Blaxter (1992) list cardiovascular research as one of their top two or three research priorities.

142. There are chapters on world security, world economy and world environment and on each of the main global regions (US, Japan, Third World, China, Soviet Union and Eastern Europe, and Western Europe) - see PSI (1991).

143. *Ibid.*, p.2 (original emphasis).

144. *Ibid.*, p.xix.

145. *Ibid.*, p.6.

146. 70 out of the total of 90 references cited are of this type (ibid., pp.359-62).

147. To take two examples: first, the report repeats the old canard that "A few years ago a study by MITI ... found that no less than 55 per cent of all the commercially important innovations made in the world since the war originated in Britain" (PSI, 1991, p.225). As Budworth (1986, p.70) showed several years ago, this 'fact' was passed from one source to another in a long chain, becoming progressively less accurate at each re-telling. (The starting point was apparently a study which analysed a number of UK innovations and found that 55 per cent were radical innovations and the remainder incremental.) Second, the authors write that "Half of R&D expenditure in Britain is devoted to defence" (PSI, 1991, p.342). In fact, it is half of government-funded R&D that is devoted to defence; for R&D funded by industry and others, the proportion is much lower, and the percentage for UK R&D as a whole is therefore well below 50 per cent.

148. It had also conducted earlier foresight studies but these had concentrated on specific sectors while the 1989 exercise covered the entire portfolio of R&D.

149. Each of these terms was carefully defined so that participants in the exercise used common definitions.

150. In some cases a scale of 1-10 was used, in others three or four categories (eg ahead, level or trailing). Where possible, evidence was sought to support the assessment score or position (eg from data on patents or scientific papers).

151. At the top of the company, there is considerable use of scenario analysis to investigate different business mixes, market conditions, competitor investments, social conditions and so on. However, the scenario results are not employed directly as guide-lines for research planning. Rather, top management uses the extreme ranges to constrain or even reject business plans, which then in turn may have indirect effects on research strategy.

152. The fifth of the companies visited had conducted a general analysis of the technologies it needed or already possessed. For each of the firm's businesses, the most important technologies were identified and analysed to establish where the company stood in relation to its competitors, what position it should aspire to, and the appropriate action.

153. One of the companies, however, stressed that it uses no very explicit criteria (apart from very approximate 'ball park' impressions of the potential market) when selecting research themes. Only at the development stage is it felt appropriate to adopt more precise criteria relating to likely costs and benefits.

154. It is perhaps significant that fewer than half the scientific publications produced by the UK during the 1980s (and appearing in journals scanned in the *Science Citation Index*) originated in university laboratories. Companies make up an important part of the remainder, although the exact size of their contribution remains to be established.

155. The committee includes two full-time people whose job is to cultivate contacts in the science base.

156. When a pair of papers are included in the reference list of a scientific article, they are said to have been 'co-cited'. According to co-citation analysts, where two papers are frequently co-cited, it can be assumed that there is some close link between them. By analysing citation databases and using clustering techniques, one can generate co-citation 'maps' which, it is argued by proponents of the technique, represent the intellectual structure of different fields.

157. Very occasionally, a group of academics might be invited to organise a symposium in a specific area of promise. Once the academic 'case' has been heard, however, internal review mechanisms determine whether the firm should alter the focus of its strategic programme.

158. Of the two remaining companies visited, one has already been extensively dealt with in Section 8.1. The other has been too busy retrenching in the last few years to give much attention to 'science watch' activities, let alone research foresight.

159. ACARD (1986, p.45).

160. See Martin and Irvine (1989). A description of the decentralised approach to science policy and priority-setting can also be found in Krull (1991), who notes that "priority-setting in Germany is much more a matter of setting up a structural framework than of giving specific thematic guidance to the researchers" (*ibid*., p.44). However, the situation is now changing: "due to increasing global economic competition, policy-makers and industrialists as well as scientists are confronted with the problem of how to select the most promising R&D areas on which to target resources" (*ibid*., p.45).

161. Quoted in *ibid*., p.76.

162. See eg Abbott (1992, p.182).

163. It increased from 26 per cent in 1982 to 40 per cent in 1990 (see Uhlhorn, 1992, p.3), and questions were asked in Parliament about why the Ministry was not concentrating on more practical problems (leaving responsibility for basic research more to DFG and the Max Planck Society). As Uhlhorn (*ibid*., p.4) points out, 19 per cent of Germany's spending on R&D is at present devoted to basic research, far higher than Japan (13 per cent) or the US (12 per cent), while R&D altogether represents 2.9 per cent of GDP, again one of the highest figures in the world. The committee therefore recognised that there was little likelihood of support for basic research being appreciably increased in the foreseeable future.

164. There were also three physicists (including the Chairman), two chemists, two engineers and a geologist (*ibid*., p.5).

165. There was nothing very systematic about the approach - for example, there were no commissioned papers. According to an official involved, this was partly because of the time-scale, with the committee being given only a year to complete their task.

166. A key problem identified by the committee was that basic biological research in Germany was too short-term, too applications-oriented and too dispersed. As a result, biological knowledge has been slow to enter the medical field in Germany compared with English-speaking countries. One section of the committee's report was devoted to clinical research and how it could be improved (see BMFT, 1992)

167. Uhlhorn (1992, pp.10-11).

168. The committee made it clear that they did not have the expertise to cover all areas of basic science. In addition, they deliberately excluded areas where a BMFT programme already existed.

169. There was also one on production technology, another in social sciences and a third in social complex systems.

170. For example, it proposed creating new centres in genetics and neurobiology.

171. For instance, the proposed Large Hadron Collider proposed by the European Organisation for Nuclear Research (Abbott, 1992).

172. Abbott (1992, p.192).

173. Demand-pull was not entirely ignored. The committee certainly recognised that a shift in emphasis towards biological sciences would help in the exploitation of biotechnology and thereby serve to strengthen the German economy.

174. For a detailed description of earlier 30-year forecasts, see Irvine and Martin (1984, pp.107-14) and Martin and Irvine (1989, pp.150-56).

175. A combination of bottom-up and top-down approaches was used to draw up the list of questions. Only 50 per cent of them are the same as in the previous STA forecast five years earlier.

176. Some of them are very specific to Japan and will not therefore be relevant.

177. ISI is attempting to match the structure of the Japanese survey sample (eg in terms of age and research sector). As in Japan, some of the experts will receive only one section of the questionnaire, others two or more.

178. Martin and Irvine (1989, pp.150-56); and Irvine and Martin (1984, pp.107-14).

179. The Minister for Research and Technology was apparently asked at a meeting with the leaders of industrial associations how BMFT identified research priorities. He decided that this was an area where BMFT needed to strengthen its efforts.

180. Until now, the agencies have done little more than administer proposals and projects, and have provided limited feedback to BMFT on policy matters. One aim of the ISI project is to tap the specialist knowledge that they possess.

181. To prepare for this, ISI first reviewed similar exercises to identify a list of emerging technologies. The results of this and other early work by ISI on the project were presented at a BMFT press conference in August 1992.

182. A previous foresight exercise by VCI is described in Martin and Irvine (1989, p.94).

183. Martin and Irvine (1989, p.104).

184. Mogee (1991, p.6).

185. Mogee (1991, p.9).

186. Information on the annual Department of Defense exercises has been drawn from Mogee (1991) because, in the time available, it was not possible to examine the original reports.

187. In 1986-87, the Department of Commerce carried out a similar but much smaller exercise which produced a list of 17 key technologies together with ten barriers impeding commercialisation of the new technologies and suggestions for overcoming them (see Martin and Irvine, 1989, pp.123-24).

188. Department of Commerce (1990).

189. National Critical Technologies Panel (1991, p.121).

190. Each criterion was carefully defined (see *ibid*., p.122).

191. To take one example, for high-performance metals and alloys, there is a two-page description of the technology, followed by half a page on the reasons for its selection (it is an important enabling technology for aircraft). There is also a half-page assessment of the status of the technology and international trends. This notes that the US is matched by Japan, France, Germany and the UK in many areas. For example, Britain is seen as strong in titanium and aluminium alloys with high-temperature capabilities (*ibid*., pp.27-30).

192. Information on this exercise comes from Mogee (1991, pp.22-23).

193. *Ibid*., p.22.

194. *Ibid*., p.23.

195. The Manufacturing Forum provided a means during 1990 and 1991 for policy-makers in government, industry and universities to discuss issues that influence manufacturing industries.

196. *Ibid*., p.37.

197. For example, the initiatives on high-performance computing and advanced materials - see Knezo (1992, pp.4-5).

198. For a description of earlier field surveys, see Martin and Irvine (1989, pp.112-16) and Irvine and Martin (1984, pp.68-76).

199. National Research Council (1989).

200. National Research Council (1991). Besides selecting from among competing large-scale projects, they also had to balance these against investigator-initiated 'small science'.

201. See, for example, Waldrop (1991).

202. OTA (1991, Chapter 5, especially p.141).

203. Sweet (1992).

204. Goodwin (1991). There was apparently just one two-day meeting with scientists (Pool, 1991).

205. This was the view of the Chairman of the House Committee on Science, Space and Technology (quoted in Sweet, 1992).

206. When asked to predict what would be the most important new areas in ten years' time, a very senior official was first completely lost for words and then replied: "I don't find that a helpful question" (Martin and Irvine, 1989, p.118).

207. Greenberg (1992).

208. See NIH (1992).

209. Metheny and Haley (1992).

210. *Ibid*.; Fox (1992); and *Nature* (1992).

211. In the time available for this study, it was not possible to talk directly to those involved nor to check all the details.

212. The Ministry of Education and Science is also extremely interested in the use of research foresight. As was noted in Section 3.1, in 1987 it commissioned a two-year study by SPRU of experiences with RFA in eight countries (see Martin and Irvine, 1989). It has since set up a foresight committee but the work appears to be less advanced than that by the Ministry of Economic Affairs.

213. van Dijk (1991).

214. *Ibid.*, pp.228-30.

215. Multidisciplinary technologies were seen as more likely to yield major innovations than single-discipline technologies (*ibid.*, p.230). This is similar to the view of Kodama (1992) and others that the 'fusion' of previously separate technologies tends to yield the greatest benefits.

216. *Ibid.*, p.230.

217. The three case-studies were carried out by an international consultancy, a large Dutch company and a national bureau, the costs ranging from $140k to $220k.

218. *Ibid.*, p.231.

219. *Ibid.*, pp.231-32.

220. A questionnaire survey of the 250 participants in the three strategy conferences suggested that they had proved successful in disseminating information and generating contacts. 95 per cent of respondents agreed that the Ministry should continue such foresight exercises.

221. *Ibid.*, pp.232-34.

222. See the SPRU study by Martin and Irvine (1989, Chapter 7) and the report by ASTEC (1990, Chapter 2). The latter also reviews experiences with foresight in Japan, Sweden, Canada, Norway and France and draws similar conclusions to the SPRU study. It employs a broadly similar classification of the different levels of priority-setting (eg national direction-setting, strategic priority-setting). It also points out that successful mechanisms for priority-setting have several structural similarities. One is that they need to be overseen by a high-level committee with the authority and responsibility to implement the results. Second, the working group with executive responsibility must have sufficient expertise to be accepted as legitimate; they must also operate at arm's length from the budget-setting process but at the same time not be too isolated from it. Third, the detailed analysis has to be carried out by those with the necessary technical credibility. Fourth, broad consultation ensures commitment to the results. Finally, any process for foresight and priority-setting must be compatible with the local research environment (cf. Martin and Irvine, 1989, Chapter 11, especially the discussion of authority, legitimacy and credibility on pp.335-36). The ASTEC report recommended that the Prime Minister establish a means by which a White Paper would set broad national directions for Australian R&D every four years, situating them in the context of a long-term perspective covering the next 8-12 years (ASTEC, 1990, Chapter 4).

223. CSIRO (1990, p.115).

224. Among the more detailed questions considered in relation to this criterion are: should the research be done in Australia? Is there a 'critical mass' effect? Does Australia have the ability to deliver the scientific resources needed?

225. To help in ranking all the research areas, a check-list of questions to be considered under each heading was drawn up. For 'attractiveness', those included: Who are the users? Is the existing quantity and quality of service

delivery satisfactory? And what is the scope for providing additional benefits? Each of these was in turn subdivided into more specific questions (see CSIRO, 1990, p.119 for details). Likewise, 'feasibility' was broken down into categories such as 'cost of research support activity' and 'capacity to deliver', each of which was then further subdivided into more specific questions (see *ibid.*, p.120).

226. *Ibid.*, p.116-17.

227. *Ibid.*, p.117.

228. See CRC (1992, p.2).

229. R. Johnston, seminar at the Science Policy Support Group, London (March 1991).

230. This is apparently the New Zealand equivalent of the Science Vote in the UK. It represents 60 per cent of government research funding, the remainder going to government departments, universities, the Health Research Council and private sector agencies (Ministry of Research, Science and Technology, 1992, p.i).

231. As in the Australian exercise, each criterion has been further subdivided. Thus, the first is broken down into the likely magnitude of the output, growth potential, urgency and the extent to which research can contribute. The second is split into ability to retain knowledge in New Zealand, communication networks between researchers and users, competitiveness and the technological culture of users. Criteria (iii) and (iv) are divided into research opportunity, time-frame, probability of scientific progress, quality and efficiency of New Zealand research, the level of skills and the available research facilities (*ibid.*, p.ii).

232. *Ibid.*, p.iii (original emphasis).

233. *Ibid.*, p.iii. The document on which the above description is based was prepared half a year ago and it is not known how far the exercise has since progressed.

234. *Ibid.*, p.iii.

235. Department of Commerce (1990, p.5).

236. Depending on the foresight exercise, other definitions may need to be established as well, for example for 'critical' technologies. However, it is not the intention here to attempt to put forward an all-embracing set of definitions, but merely to indicate some of the concepts that have to be clarified.

237. Martin and Irvine (1989, p.34).

238. The Corporate Plans developed by AFRC and NERC are perhaps an exception here.

239. See Martin and Irvine (1989, pp.20-22) for further details of this four-fold classification.

240. Although no German firms were contacted in this study, the results from the previous SPRU survey three years ago suggest that foresight is regarded as a useful tool in industry (see Martin and Irvine, 1989, pp.92-96).

241. For a brief description, see Martin and Irvine (1989, p.58 and p.65).

APPENDIX 2

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APPENDIX 3

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Dr Roy Farmer (Information and Manufacturing Technology Division)
Lorna Windmill (Information and Manufacturing Technology Division)
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Energy Technology Support Unit (ETSU)
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Ministry of Agriculture, Fisheries and Food (MAFF)
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Dr Mandy Bailey (Research Policy and Co-ordination Division)
Aidan Power (Economics - Resource Use)
David Smith (Chief Scientists' Group)

Ministry of Defence
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Ian Mackintosh (Director of Research, Technology)
Dr Christopher Pell (Assistant Director of Research, Technology)

Policy Research in Science and Medicine (PRISM)
Dr Joe Anderson (Director)
Dr Lesley Rogers
Dr Nigel Williams

Science and Engineering Research Council (SERC)
Dr A.E. Hughes (Director Programmes)

B. France

Observatoire des Sciences et des Techniques (OST)
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D. Netherlands

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Coates and Jarratt, Inc.
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Department of Commerce
Dr Kenneth Gordon (Deputy Director for International Affairs, National Institute of Standards and Technology)

National Institutes of Health (NIH)
Dr Helen Gee (former Head of Research Evaluation)

National Research Council (NRC)
Dr Donald Shapero (Director, Commission on Physical Sciences, Mathematics and Applications)

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