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Establishing New Research Directions*

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ABSTRACT

Increasingly, in the United Kingdom and other countries, government policy towards the academic sector is being guided by the principle that scientific and engineering research should be linked to the requirements of industry. Governments are selectively supporting areas of research based on their perceived industrial relevance. However, given that academic research is essentially long-term in perspective, this policy implies an ability to choose those areas of science and technology that will be important some time in the future. This paper considers the institutional structure within which this choice is currently made in the U.K. and considers some possible alternatives.

I. Purpose of the Study

For many years the United Kingdom's Science and Engineering Research Council (SERC) has had a developing policy of selectively providing extra support for a limited number of areas of research deemed by some criteria to be important. Such a policy obviously requires that a choice be made between alternative areas of research. This in turn requires, first, a mechanism for generating alternatives and, second, a mechanism for evaluating and choosing between the alternatives.

This paper briefly outlines these mechanisms as they currently exist and considers some other possible arrangements. The history of selectivity is sketched out to provide a background to the choice process. Then the current approach is described and its strengths and weaknesses examined. Alternative approaches are identified, and a few are expanded upon. Finally, the key issues to be tackled in any attempt to modify the current system are discussed.

II. Background to Selectivity

The need for selectivity in support of research was set out by the then Science Research Council in 1970 [1]. There it was argued that in some areas of science there was a threshold funding level below which it was not possible to do good, useful research.

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A too thin spread of available funds across all research areas was, therefore, undesirable on the grounds that funding below the threshold would be ineffective. As there were not sufficient funds to support all science at at least the threshold level, a choice would have to be made to select those areas that were to be adequately funded and those areas from which Britain would withdraw. That is, a choice had to be made in the light of funding constraints, but that choice was having to be made because of the cost characteristics of science.

Since that first paper on selectivity S(E)RC, in response to financial pressure and the changing economic and political climate, has extended the practice of selectivity. It is now an integral part of the Council's approach to supporting research and, indeed, an increasingly important feature in the research support provided by the other research councils. The basis for adopting a policy of selectivity has, however, changed. For the SERC's Engineering Board, which has adopted the concept most wholeheartedly, the reason for selectivity is not so much the existence of funding thresholds but the perceived need to make academic research more industrially relevant [2].

The principal mechanisms by which the Engineering Board has implemented this policy of selectivity have been the specially promoted programs and, in particular, the directorates. Figure 1 shows that the directorates now account for about 50% of the Board's research support.

Directorates (including, for example, biotechnology, information technology, and manufacturing technology) are charged with designing, promoting, and coordinating academic research programs in key areas of industrial relevance and promise.

Inasmuch as science is commercializable in the long run and engineering in the

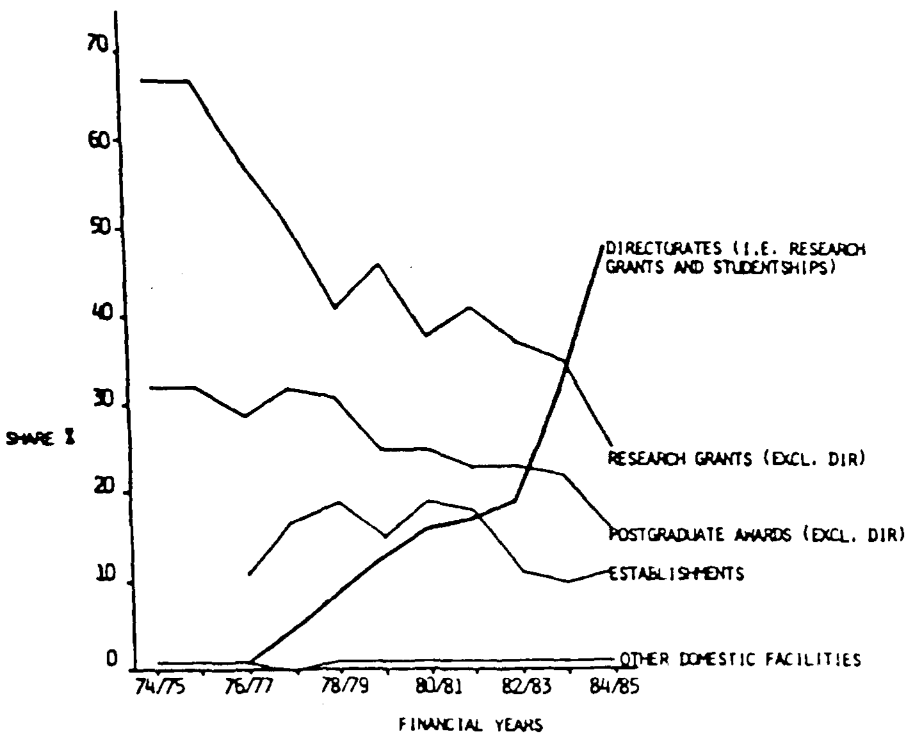

Fig. 1. Basic research support by the SERC from 1974–1985. ***Source:*** **[3]**

(relatively) short run, the concept of industrial relevance may be less obviously appropriate to science than to engineering. Although research on fundamental questions may eventually have an industrial impact, often it will not do so within the planning horizons of industry. Furthermore, on the assumption that market uncertainty is directly related to time, industry itself will be less certain about what is relevant long-term research than it is about what is relevant medium-term research. Nevertheless, in *A Strategy for the Support of Core Science* [4] the SERC's Science Board identified the need for selectivity based on the need scientifically to underpin certain sectors of the economy. This need for science to be selectively supported on the basis of its potential economic impact is reinforced by the report *Exploitable Areas of Science* [5]:

> The role of strategic science policy over the next twenty years of UK economic growth then becomes clear. On the one hand, it should contribute to existing activities keeping close to world best practice. On the other hand, it should respond to the important export and growth opportunities which lie in newly emerging technologies where the knowledge base is changing rapidly enough to prevent low-wage countries establishing a technological presence . . . . [T]he allocation of resources to strategic science should be seen as an integral part of this market-driven process.

III. The Choice Mechanism

The policy is, of course, based on the assumption that it is possible to choose appropriate fields of research on which to concentrate resources so as to achieve the set objectives. At present such choices generally emerge from the committee system that orders British science. Within the SERC, these committees include representatives from academic institutions, industry, and government research laboratories, though at the board level, as can be seen from Table 1, only the Engineering Board has a balance of industrial and other members.

The committee composition is similar to that of the boards shown in Table 1 except that engineering committees and subcommittees tend to have a higher proportion of industry members than does the Engineering Board, but the science committees and subcommittees have a still lower level of industrial representation than the Science Board. It is from this structure that the choice of selected areas emerges by a process of consensus.

The extension of the consensus-forming approach as a mechanism for selectivity is advocated in the *Exploitable Areas of Science* report [5]. However, this consensus approach has weaknesses as well as strengths.

The strengths of the approach are the breadth and depth of knowledge and experience that can be employed in selecting areas for research, the unification of actors with differing motivations (such as industrial scientists and academics) by involving them together in the consensus-forming process and the commitment that comes from involvement.

TABLE 1
Composition of SERC Boards

	Members from		
Board	Academia	Industry	Government Laboratories
Astonomy, space, and radio	13	1	1
Engineering	8	8	—
Nuclear physics	10	1	2 (incl. CERN)
Science	11	3	—

Source: SERC Annual Report 1985/86.

The SERC committees, by including eminent scientists and engineers, are designed to apply in-depth knowledge of the state-of-the-art to the task they are set, be it evaluating projects as part of the peer review system or advising on aspects of policy such as the selection of priority areas. This is the first, and very important, strength of the committees for arriving at consensuses. They should be capable of good scientific judgement.

The members of the committees tend to be senior scientists and engineers who should be able to apply mature judgement to the tasks set. This is reinforced, especially in engineering, by the inclusion of industrial representatives who can provide an alternative to the academic perspective.

The inclusion of members from different backgrounds and with different objectives and motivations, together with the necessity for such members to come to an agreed position, should help ensure that decisions are not taken which exclude the interests of some groups. Furthermore, by including members from different sectors, advocates are established within each sector who, having been involved with the decision-making process, have an interest in the successful implementation of the decision. This is consistent with modern thinking on planning which stresses decentralized planning with the involvement of staff who will have responsibility for implementation rather than centralized, top-down imposed planning.

Within the SERC system, however, there are three reasons why this consensus system might not work as well as it might. First, the members of the committee, especially the industrial members, might not, in fact, be among the most eminent. The view has been expressed that the best people in industry are too busy for their companies to allow them the time to serve on SERC committees. Among academics, and among some more enlightened companies, this is less of a problem. Second, in general the committees are small relative to the size of the community involved so that the majority of the community is not involved in the decision-making process. Third, related to the second reason, the members of the committee are appointed—they are not elected democratically by the community—so that it may be stretching the language to say they represent that community.

More fundamental weaknesses of the approach are that it is inherently conservative and that it leads to a situation where major industrial countries using similar selection mechanisms will tend to choose the same, limited number of areas for selective support programs.

The conservativeness of the consensus approach follows naturally both from the task that is set a committee that chooses an area to be selectively supported (that is, "arrive at a consensus") and from the membership of such committees. The practice is for such committees to comprise established scientists with excellent track records. As such, the committees provide excellent examples of the scientific enterprise as described by Kuhn [6].

Kuhn proposed the seminal idea of scientific paradigms for accounting, in sociological terms, for the development of science. He stressed the innate conservatism of a system where decisions on the allocation of resources are controlled, or at least strongly influenced, by scientists with long-lived commitments to current paradigms. In the choice of areas for selective support this means that the areas chosen will be those where the leading scientists are active and the results will be a further concentration of funds in support of these scientists. Evidence of this comes from recent studies in Canada [7] and Britain [2]. The Canadian study showed that:

> the concept of strategic support has been operationalized as additional support for researchers who have been with [the Natural Science and Engineering Research Council] for considerable lengths of time and who are, and who have been in the past, highly funded by the traditional discipline committees [7].

The British study of the SERC's specially promoted programs showed that the effect of the programs is

> largely to increase the research effort within departments with well established research projects [2]

but that this occurs

> not by eliciting [from established programs] a larger proportion of applications in the desired areas but by increasing the success rate of [their] applications [2].

The industrial membership of the committees may also impose a second form of conservatism. The industrial membership is predominantly drawn from large, established companies. In both engineering and science boards and committees, over 75% of the industrial members are drawn from the top companies in Britain (based on the Times 1000) or U.K. subsidiaries of large foreign companies. It has been argued, however, that large companies are less innovative (relative to their size) than are smaller companies and that the industries of the next century (that is, the industries that current fundamental research should be underpinning) will emerge not from the large companies of today but from smaller, entrepreneurial ventures [8]. Large companies will enter these industries not by innovation but by the acquisition of small companies that survive the initial bout of creative competition.

If this is the case, then the decisions on selectivity may be influenced by an inappropriate stratum of industry. The areas chosen will, presumably, be those expected generally to reinforce existing (and, in the long term, declining) industries rather than emerging industries.

The "superconsensus" problem leads to several countries concentrating on broadly the same priority areas. Biotechnology, information technology, manufacturing systems, etc., rank highly in the priority listings of almost all industrialized countries. This is a particular problem for Britain which tries to maintain a capability across a wide range of science whilst operating at a lower level of resources (Table 2) than the scientific enterprises in several competing countries.

Added to this is the apparent tardiness in Britain in selecting and committing resources to priority areas. (Consider, for example, the lag between the announcements of the Japanese Fifth Generation Computer Program and the Alvey Program).

It is suggested that one aim of policy is to identify priority areas within which Britain might develop a competitive advantage and to do so in advance of such areas being identified by potential competitors. With these aims in mind the purpose of this paper is briefly to consider possible ways of overcoming the weaknesses of the consensus approach as currently practiced while retaining the benefits accruing from its strengths. To this

TABLE 2
Comparative Expenditure on Civil R&D

	Civil R&D	
	Percentage of GDP	$US billion (ppp)
USA	1.9	62.2
Japan	2.5	32.9
West Germany	2.5	19.1
France	1.7	11.3
UK	1.6	9.4

Source: 1986 Annual Review of Government Funded R&D.

end, the alternatives considered in this paper should be seen as complements to, rather than substitutes for, the consensus approach.

Before proceeding, it is worth emphasizing the two conceptually distinct tasks that need to be performed in establishing a set of priority areas. First, there is the task of *generating* a set of alternatives. Second, there is the task of *evaluating* the various alternatives and choosing between them. These two tasks are quite different in kind. The former is an exercise in creativity; the latter is an exercise in judgment. There is no necessary reason why the two tasks should be performed by the same individuals or be performed within the same institutional framework. Indeed, in many organizational settings the two tasks would be clearly distinguished as belonging to staff and management, respectively.

In what follows, for the purposes of conceptual and expositional clarity, the generation and evaluation of alternatives will be considered separately.

IV. Generating Alternatives

A. TECHNOLOGICAL FORECASTING

Technological forecasting (TF) might by now be regarded as the traditional approach to the problem of anticipating the future. TF is, in fact, not one forecasting technique but a collection of techniques and approaches. It attempts to make the best use of our limited knowledge of the development of technologies into businesses or socially useful implements in order to guide R&D planning, capital investment, etc.

There is an extensive literature on TF, and there is little to be gained by repeating here a description of the multifarious techniques and their pros and cons. Outlines of the techniques together with evaluations of their strengths and weaknesses are given elsewhere [9, 10]. Three of the techniques—morphological analysis, scenario writing, and trend extrapolation—are described to illustrate how TF techniques could be potentially fruitful for the SERC in the selection of priority areas.

1. Morphological Analysis

Morphology is a systematic approach to identifying alternative technological arrangements for tackling problems. It involves the construction of a matrix where each row comprises the set of possible technological solutions to a key parameter in the problem. By combining the elements of each row, the set of potential technological solutions to the overall problem can be constructed. The research needed to realize each feasible solution and the likely value of each solution can then be assessed.

Table 3 shows an example of a morphological matrix. It can be seen that there are combinatorial problems associated with morphology. In the example given there are 30,240 combinations of the elements in the six rows. This crude calculation may overstate the case as there can be some combinations that are infeasible (say, light rays as an energy source and weights as the energy store). However, the size of the matrix can present real problems if all feasible alternatives are to be given serious consideration.

Further problems with morphology are systems definition and completeness. The table shows the matrix for clocks, but if the system was conceived more widely as timepieces, then the matrix would need to be expanded to accommodate, for example, the Advent candle and the egg-timer. Completeness is a problem as one wants to include in each row not just the existing options but the potential options. This seems particularly important when using morphology for arriving at research programs. Thus, in the example,

TABLE 3
Morphological Matrix for Clocks

Parameter	Alternative Solutions							
Energy source	Manual winding	Vibration	Expansion winding	Pressure fluctuation	Temperature fluctuation	Hydraulic energy	Galvanic cells	Light rays	Power supply system
Energy store	Weight store	Spring store	Bimetallic coil	Pressure container	Electric accumulator	No store			
Motor	Spring motor	Electric motor	Pneumatic motor	Hydraulic motor					
Regulator	Balance wheel	Torsion pendulum armature	Centrifugal governor	Inching pendulum	Tuning fork contact	Constant mains frequency	Electric impulses		
Gearing	Pinion drive	Chain drive	Worm drive	Magnetic drive					
Indicator device	Hands dial plate	Plates and marks	Rollers and window	Slide and marks	Turning leaves				

Source: [9]

it might be fruitful to consider holograms as indicator devices. We are drawn back, therefore, to the need for creativity and the realization that there is no purely mechanistic way of generating alternatives.

Morphological analysis may be seen as complementary to the approach of disinvention (see below). Having chosen the technology to disinvent, one can generate its morphological matrix, which can be examined to decide the most fruitful alternative.

2. Scenario Writing

Scenario writing involves constructing an image (or, more commonly, images) of the future and deducing the research required to move from the known present to these anticipated futures. Implicit in scenario writing is a market-led model of research and innovation. This contrasts with the technology driven model assumed by morphological analysis.

Scenario writing can itself involve many of the individual techniques of technological and futures forecasting such as brainstorming, Delphi, time series, or systems dynamics, and the uncertainties associated with each technique are also associated with the scenario. Indeed, when using scenarios for research planning there are two major areas of uncertainty. First, the uncertainty of the image and, second, the uncertainty of interpretation, that is, deducing the research implications of the scenario.

The use of scenarios for planning purposes has been increasingly common in recent years, not least because of the unpopularity of more traditional forecasting techniques. Shell, in particular, has stressed the use of scenarios in its planning process. Typically, the uncertainty of the scenario is recognized by constructing not one scenario but a range of scenarios ("best case" through to "worst case"). The company's plan should then be sufficiently robust to allow a response to whatever scenario materializes. This is a particularly important point for SERC to consider since, given the time lags involved between research and eventual commercialisation, the range of scenarios to be considered will be wide.[1] This suggests it will be important for the SERC to fund a range of options and not place too many of its eggs in one (or a few) basket(s).

In much technological forecasting there is some confusion between projections and prescriptions, the former being "objective" extrapolations of current and recent history, the latter being expressions of preferred futures. Scenarios of both kinds can be constructed with the difference being an illustration of the gap that needs to be met by research.

Consider, as an example, the *The Limits to Growth* report [11]. This has been widely criticized, but the most useful criticism was that it failed to take account of the potential of technical progress, especially in information technology, to produce economic growth that involved less use of natural resources than had historically been the case. This suggests that the proper response of the scientific community to *The Limits of Growth* was not to denigrate it but to treat it as setting a research agenda: How can growth be maintained while using less natural resources?

This view has parallels at the micro level of a factory where overall production

[1]On the assumption that uncertainty increases with time, the range of the scenarios which will have to be considered will do likewise. So, if the scope of research that has to be maintained is directly related to the range of the scenarios, the greater the relevant time frame, the broader the scope of research. Does there come a point where the exercise becomes pointless, i.e., does not allow one to prioritise research? If so, is this point within realistic timescales.

efficiency is enhanced by eliminating bottlenecks. At the factory level there is the alternative of totally redesigning the system. Since only megalomaniacs and revolutionaries aspire to totally redesigning the social order, SERC should, perhaps, attempt to identify those "bottlenecks" preventing the achievement of a desired future state that appear amenable to solution through research.

3. Trend Extrapolation

The technique of trend extrapolation is one of the best established tools of technological forecasting. It can be used as one approach to scenario writing but it can also be used as a separate approach in its own right. The basic idea is to identify the trend through time in some physical or performance parameter and extrapolate it into the future. One is then in the position to consider the research required if these values of the parameters are to be attained.

B. BIBIOLOGRAPHIC ANALYSIS

The use of publication, citation, and other bibliometric techniques as an aid to science policy evaluation has been the subject of extensive research over the past decade or so.

The assumption underpinning bibliometric analysis is that the output from research is represented by publications and that suitable measures of the literature will in turn represent measures of the output from research. Furthermore, if the appropriate measures can be derived and implemented, then the association between various fields may also be measured. It is this measure of association that is suggested as a technique of identifying at an early stage the emergence of new subfields of science that might represent candidates for priority support.

Healey et al. report on a series of studies commissioned by the Advisory Board for the Research Councils and the Economic and Social Research Council to evaluate the usefulness of alternative bibliographic analysis techniques as aids to science policy makers [12]. Three techniques were evaluated: co-citation modeling, co-word modeling, and citation analysis.

Co-citation modeling, the most promising of the three techniques, involves identifying pairs of documents in the literature that are cited together in a high number of subsequent publications. Cluster analysis of these co-cited pairs is then performed to identify groups of publications that have a shared intellectual basis and together represent a "research speciality." It is suggested that the use of co-citation modeling techniques may allow the identification of emergent specialities and so provide policymakers with alternatives to consider when choosing areas for priority support. Healey et al. suggest that the results of such techniques are consistent with the judgement of experts in the fields:

> . . . some experts were sufficiently confident about the general validity of the co-citation approach to question their [own] assumptions when specific findings . . . went against their expectations. Most were sufficiently confident about the work to recommend its use to historians of science, or those planning new work in the field and looking for a general orientation to the main currents of activity and the literature supporting them [12, pp., 244, 245].

Given the focus of this paper, the main advantage of co-citation analysis is that, implicitly in the technique, the generation of possible priority areas is being performed not by the small number of scientists found on a typical committee but by the members of the scientific community at large inasmuch as they contribute referenced papers to academic journals. Foreign scientists, who generally would not take a part in the decision-making process, are included, and the weighting given to each scientist is determined by his or her peers' reaction to the literature rather than his or her status.

The approach, however, is not without its drawbacks, in terms of both resources and technical features. First, the computing costs of co-citation analysis are high relative to the budget that would typically be available to, say, an SERC committee. Second, the time lags that exist in the publication system (between submission and publication, publication and citation, and citation and entry into the database) mean that co-citation modeling can only given an indication of scientific specialities that emerged a number of years ago. Third, in deciding whether a speciality is emerging, a threshold number of citations needs to be achieved. But differing areas of science exhibit differing publishing and citation behaviors, and an arbitrary choice of threshold might easily allow a proliferation of "noise" in one field while "silence" prevails in another. To an extent, this can be controlled by normalizing each field of science separately, but this may still lead to problems in the areas of interdisciplinary research, the very areas that might be expected to give rise to the most fruitful new priority areas. Finally, to the extent that the citation process fails accurately and completely to represent the output of science, so also the co-citation technique will fail to give a correct picture of emerging science [13].

C. DISINVENTION

What would happen if, say, the wheel were no longer to exist? Undoubtedly, some extensive R&D programs would ensue as firms and nations adjusted to a world of skis, legged machines, pogo sticks, etc. How competitive would these new technologies be if the wheel were then reintroduced? The approach of systematically (notionally) disinventing existing technologies to force consideration of alternatives has been suggested [14].

This approach might be regarded as one way of accelerating a technological paradigm shift. It forces the research community to follow lines of thought that would otherwise not be entertained on the grounds that current technology was not in a state of crisis. Alternatively, the approach might be regarded as the converse of substitution processes in technology. While with substitution analysis one starts with the new technology and sees how it progresses in the market, with disinvention one starts with the technology to be substituted for and investigates how it might be displaced.

An advantage of disinvention would be that it could throw up unexpected developments, leading to priority options that did not suffer from the superconsensus problem. As the areas identified as fruitful by this approach do not emerge "naturally" from the current state of science and technology, there is no reason to expect different groups carrying out disinvention exercises to disinvent the same technologies. There is, therefore, the potential to establish a competitive advantage through being first.

A problem with disinvention is that there are no obvious guidelines for choosing the technologies to disinvent. Out of the multitude of technologies that could be disinvented, which would lead to the most fruitful research programs? One possible approach might be to disinvent technologies that are exposed for economic, political, or strategic reasons, where there is a risk that they might become expensive or regulated. One could, thus, disinvent nuclear power, oil, tungsten, or food additives. A limitation of this specific approach would be that it might reintroduce some aspects of the superconsensus. If, we assume that different countries face broadly similar problems in the world economy, environmental pressures, etc. then, they would be led to disinvent similar technologies.

It is claimed that the disinvented technology should run across a wide range of uses in different ways [14]. Taking the example of mild steel, the impact of its disinvention is assessed by looking at currently available alternatives to the technology. The impact is classified as small (e.g., packaging), intermediate (e.g., construction), large (e.g., marine structures), and areas where the new technology provides dearer but better alternatives (e.g., heat engines). This form of analysis allows one to identify which areas of research resulting from disinvention would be most fruitful.

One question which has not been addressed is who in the context of the SERC would do the disinvention. The choice of technology to be disinvented and the interpretation of the potential research programs that might follow are obviously highly subjective. It seems quite likely that if the process was allowed to reside within the present institutional framework of the SERC, then the results would be little different from those emerging from the present system of priority area selection.

V. Evaluating Options

A. A DIFFERENT CONSENSUS

One of the criticisms of the present consensus forming process is its conservativeness arising from the makeup of the committees involved. As a generalization, the members of these committees will be older, established scientists and engineers who, if drawn from industry, will represent large, well-established firms. Can this be changed? Would it be possible or wise to change the membership of committees to include younger scientists or representatives of smaller, newer companies? Such changes might lead to the selection of different priority programs from those currently being chosen but would not be without difficulties, both in principle and in practice.

The first difficulty envisaged is that the reformed committees would not be credible to the wider community of scientists and engineers. Whatever their limitations, SERC committees are supposed to represent and reflect the views of the community from which they are drawn. To do this credibly, the committee members need to be known and respected throughout the community, and it is unlikely that younger scientists and engineers would meet this criterion. This is a practical difficulty, an objection in principle to the inclusion of younger members on the committees would be that their judgment between areas of science was untested or, at least, undemonstrated. Younger scientists will tend to be specialized and have limited experience of work outside their own specialism. But the selection of priority areas is a choice between areas so the committee members should be experienced enough and broad enough to be able to judge areas they themselves are not currently involved in.

When it comes to smaller firms, a different kind of difficulty emerges. The question here is not just whether one *should* involve representatives of such firms but whether one *could* involve the right people. Are today's dynamic entrepreneurs—whose currently small businesses are going to grow into large firms and be the basis of tomorrow's industries—essentially noninstitutional people? Would they have the time or inclination to get involved in the necessary committee work?

Furthermore, one might ask whether representatives of such companies should be involved in the consensus forming process. The relationship between science and technology remains an uncertain one, but it is clear that the naive linear model that starts with science and progresses through to technology, though it may fit some cases, is far from being universally applicable. Many of today's dynamic, young companies will be based on technologies, the science of which is imperfectly understood and does not need to be understood within the time horizon employed by the entrepreneur. It might be questioned whether representatives of such companies should be on the SERC's committees.

B. A BROADER CONSENSUS

The changes described in the previous section merely tinker with the membership of the committee but do not alter in any fundamental way the structures and processes involved in arriving at a consensus. But there is an alternative consensus forming process that is quite different both in technique and the size of the community that can be involved. This is the well-known Delphi technique.

In Japan, Delphi studies have been used to help establish the country's technological priorities [15]. Three massive Delphis have been conducted since 1969 by the Science and Technology Agency. These Delphis involved experts drawn from academia, government, and industry and "efforts were also made to ensure that younger researchers were represented." The Delphis' objective was to identify major developments in all fields likely within 10 and 30 years. The first Delphi involved 4000 experts, though this was reduced to 2000 in the second and third exercises.

There has been much debate as to the validity of Delphi as a forecasting tool. Furthermore, when a Delphi is conducted as openly and is as broadly based as the Japanese exercise, it can be expected to affect in a significant way the behavior of the individuals and institutions involved. Thus such a Delphi might be seen as providing the research community with agreed targets (rather than forecasts) so ensuring a coherent national research effort leading to "accurate forecasts."

Whether target-setting or forecasting, the STA Delphis have been viewed as successful on four counts [15]: They cause the research community, including policy makers, periodically to address the long term on a systematic basis; they provide a general summary of what is happening, or likely to happen, across the entire range of R&D activities; they provide a mechanism for synthesizing research trends across sectors; and they assist government in establishing national priorities in allocating resources.

This last point is obviously relevant here. Since the Delphis are taken seriously by the research community, including industry, they can be used by government agencies to ensure that priority programmes are consistent with, *and supportive of*, the research programs of industry.

The Delphi approach is not without its disadvantages. It is conservative (like any consensus forming process) and may be less relevant to basic science than to technology. Also, because it is a consensus approach, it can lead to all companies focusing on the same technology, thus leading to excessive competition. A third problem with the approach is that it is resource-intensive, both within the administering organization and within the research community, though enhancement in information technology should obviously lessen this.

As described here, the Delphi approach is an evaluative step—it helps establish priorities between alternative fields. But such a Delphi requires, in the first place, a set of options to be evaluated. These may be arrived at through a pre-Delphi round of questionnaires to experts asking for qualitative data on expected developments. Alternatively, they may be presented to the Delphi participants, having been decided in some way by the administering organization. Both these approaches have been tried in Japan, but, if the second approach is used, it is necessary to consider in more detail the techniques and processes for generating options since the quality and validity of the output from Delphi, as in any futures technique, is governed by the quality and validity of the inputs.

VI. Discussion

Anticipating the future to predict tomorrow's technologies is not a new activity. Leonardo da Vinci, Jules Verne, and Isaac Asimov, among so many others, have applied their imaginations to the task. And it is always important to remember that the future really is essentially unknowable and that looking to thc future, no matter how systematically approached, remains an exercise in imagination. For those with an interest in being right about the future—be they gamblers at the races or SERC committee members—the problem is whether there are techniques for guiding the imagination and reducing the disparity between the imagined and actual futures (only luck will eliminate the disparity entirely).

The history of long-range technological forecasting is not very auspicious [16]. What would have been foreseen as the leading technologies of the 1980s back in, say, 1945? Surely not microprocessors, though thermionic valves would have looked promising. What technological visionary would have thought we would still be burning coal?

The history of computers is littered with what now seem laughably erroneous forecasts [17] including, "[In Britain] we have a computer here at Cambridge, there is one in Manchester and one at the [National Physical Laboratory]. I suppose there ought to be one in Scotland, but that's about all." This was an expert's expectation in 1951. If scientific priority areas were being chosen then, would computing have been selected?

But technological forecasts do not always err on the side of caution. In the 1940s and '50s, there were expectations of nuclear-powered space ships, planes and cars and energy "too cheap to meter" produced by nuclear power plants that might even be "the size of a typewriter" [18]. Even in 1958, Ford was proposing futuristic designs of cars powered by a "replaceable, rechargeable nuclear reactor."

Broadly speaking there appear to be four kinds of error that are made in anticipating the future and which need to be guarded against in the selection of priority areas for research. The first two of these errors lead to *conservatism,* a tendency to underestimate the nature or extent of technical change. The third and fourth errors lead to *radicalism,* a tendency to overestimate. The errors are:

1. The inability to conceive of technologies qualitatively different from those of which there is current experience. How in, say, 1945 could anyone have forecast the important of, for example, microprocessors or monoclonal antibodies? Where is the expert who can predict the currently nonexisting technologies that will be important in 2025? The extent to which this is a serious problem depends on the time horizon within which SERC operates. The shorter the time horizon the greater the probability that the relevant science or technology will have already emerged.
2. The inability to conceive of new functions for emerging technologies. The tendency is to see a new technology as a substitute for an existing technology rather than see it as creating a new set of functions that could not have been performed by the current technology. That is:

    > Whenever a new technology is born, few see its ultimate place in society. The inventors of radio did not foresee its use for broadcasting entertainment, sports, and news; they saw it as a telegraph without wires. The early builders of automobiles did not see an age of "automobility"; they saw a "horseless carriage." . . . The inventors of the first digital computers saw their machines as direct replacements for [a] system of humans, calculators, tables, pencils and paper, and instructions [17, p. 194].

    The speed with which a technology can move into new areas can be great and may well be a result of entrepreneurship rather than technical development. However, the development of the technology may well be a contributory factor as recent advances in the theory of technological diffusion have highlighted [19].

3. Technological visionariness which tends to extrapolate a feature of a new technology, often a minor feature, to an unrealistic extent and without recognition of the constraining factors acting upon the technology. Ignoring problems of scaling is possibly the most common form of this error. Technological visions are usually utopian but may also be dystopian.
4. Similar to the third type of error is the tendency to underestimate the time taken for a technology to develop to the stage where it may have a significant impact. This, in turn, is due to three factors. First, a failure to recognize the technical developments needed for a technology to be acceptable to the customer (the history of Sinclair's product announcements demonstrates this); second, a failure to recognize that technical feasibility is not the same as market acceptance; and third, a failure to recognize the institutional resistance to a new technology, especially if it requires heavy capital investment or specialized infrastructure.

These errors are not exclusive and may be compounded. Thus early workers in robotics often combined errors of types 2 and 3 to forecast the development of intelligent, dynamically sophisticated, but anthropomorphic robots [20].

These comments are included as a caution against treating the future as knowable and forecasts as ways of getting to know it, thus enabling relevant research to be planned. But the techniques and approaches discussed in this paper go further than simply forecasting and may be seen as performing two other important functions. First, they force a consideration of options for research that would not arise from the internal dynamic of the current technology [21]. That is, they provide the potential of establishing a competitive advantage through being first. Second, they may provide unifying targets for the research community, leading to a concentration of effort in certain areas, again leading to competitive advantage.

There is no real theoretical or empirical support for the approaches presented here, but, as long as the SERC is to continue its policy of selectivity in support of research the use of one or more of these approaches would at least provide a common framework for discussion and choice.

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