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STUART BLUME

Science Policy Research

Its Current State and Future Priorities

INSTITUTE FOR STUDIES IN RESEARCH
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P R E F A C E

The roots of science policy studies can be sought in the tremendous growth of science and technology during the past decades. A heterogeneous field of study has emerged, academic or directly policy-orientated in scope, and drawing on several scientific and scholarly disciplines. In this paper Dr. Stuart Blume gives a survey of some major lines of research and some of the results attained in this area - as well as recommendations for future research.

During the last years our institute has built up a Division for studies of R&D resources and science policy, headed by Hans Skoie. The task is to provide material for policy-making within the Norwegian Research Council for Science and the Humanities, as well as a Norwegian contribution to science policy studies in general.

We have benefitted much from seminars that Dr. Blume has given at our institute. They form a valuable framework for our activities, and we believe them to be of interest to a wider audience, particularly in the other Scandinavian countries. Dr. Blume has revised an expanded version of his seminar papers, originally written as a report to the British Social Science Research Council, for publication in our Report series. We owe him gratitude for inspiration to our work and his kind permission to publish it here.

Oslo, May 1978

Sigmund Vangsnes

P R E F A C E

B Y T H E A U T H O R

This report was originally written for the British Social Science Research Council in 1975, when I was working at the Science Policy Research Unit of the University of Sussex. I am delighted to have the opportunity of presenting it to a new audience.

Few changes have been made. I have limited myself to bringing in certain studies, published since 1975, which seem to me important. None of these studies has led me to modify the conclusions to the report, with which I still agree and which are unchanged.

Two other points must be made. The review was intended to focus upon that in science policy research which is relevant to the concerns and responsibilities of the British Research Councils and their Advisory Board. There is thus a substantial emphasis upon the British situation. Moreover, many areas which have become part of science policy research (e.g. technology assessment, energy problems, scientific ethics) are not treated. Nor, really, is industrial R&D.

And finally, I should like to repeat the Acknowledgements I made in 1975. My thanks are no less due now than they were then to Christopher Freeman, Marie Jahoda, and other Science Policy Research Unit colleagues for continuing and invaluable advice.

London, April 1978

Stuart Blume

C O N T E N T S

INTRODUCTION: SCIENCE POLICY AND SCIENCE POLICY RESEARCH

In spite of a number of isolated efforts in the nineteenth century, and of the impetus of the First World War, science policy became a systematic concern of government only in the course of the Second World War. Writing in 1938, J.D. Bernal observed the "appalling inefficiency" of science "both as to its internal organisation and as to the means of application to problems of production or of welfare. If science is to be of full use to society it must first put its own house in order". (Bernal, 1938, p. xiii.) His study was bedevilled by a lack of the kind of statistical and other information upon which analysis, not to say policy, had to be based. As Greenberg (1967), Rose and Rose (1969) and others have discussed, recognition of the role which science could play in the war effort led to the establishment of structures and mechanisms whose success put them beyond subsequent demobilisation. The public resources which science could claim in post war years were staggering by comparison with earlier times, as science and government acknowledged their mutual need, each for the other. It was now accepted that fruitful science demanded that the scientific community be permitted substantial autonomy in the utilisation of these funds. Yet this in itself, rather different from the way in which government sought to control expenditure and make policy in other fields, posed problems for policy-makers (Price, 1954).

It became increasingly clear that the exercise of their responsibilities in regard to science presented governments with a variety of difficulties, which required specific consideration. Machinery was established, in an increasing number of countries thanks partly to the propagandising efforts of the OECD, for the consideration of fundamental and long-term issues in science policy.

Academic interest in these self-same issues followed a rather similar path, though lagging rather behind. The concerns of Bernal and his scientist-friends in the 1930's are today the concerns of large numbers not only of scientists, but of economists, political scientists, sociologists and others. Moreover, in addition to those who share Bernal's practical concern with improving the social benefits of science, there are today numerous social scientists whose interest in science is of a more theoretically-inspired kind. History and philosophy of science developed quite independently of these practical considerations. The economics of research may be traced back to Marx, and even to Adam Smith who recognised that science could have economic effects. It was stimulated in the 1950's by a theoretical interest in explaining the sources and rates of economic growth. Robert K. Merton brought growing numbers of sociologists to the study of science as a social activity, but in the 1950's and 1960's their increasingly specialised work had little conscious relation to practical questions of policy.

So the fact is that the academic study of science draws upon a number of quite unrelated traditions. Differing emphasis has led to the imprecise use of a variety of terms to describe this kind of study, of which "science policy research" is but one. It is true that as a result of advancing knowledge, of increasing interaction between these traditions, and of the inter- and multi-disciplinary questions posed by policy-makers, boundaries are blurred. But they have not disappeared: for example, many sociologists of science would see their work as in no important way connected with science policy, but (hopefully) contributing to mainstream sociology. It is therefore far from easy to establish any guidelines for an overview of 'the' field.

Most of this report consists of a review of the research literature. Since this lack of guidelines necessarily renders the process of selection and review problematic and subjective, it seems to me both honest and necessary to make explicit certain assumptions which underlie my presentation.

Most fundamentally, I have tried to stress the <u>integration</u> of the various disciplinary approaches which are not, therefore, discussed separately. Thus, it has seemed to me that to utilise the terms and concepts of each discipline in presenting findings was inappropriate. In trying to use a relatively homogenous set of concepts, and a single framework, it is inevitable that some injustice will have been done to each discipline represented.[1] The emphasis upon multi-disciplinary integration has necessarily meant that distinctions have been made within individual disciplinary approaches which cannot but appear arbitrary to their adherents. Some readers may feel that I have tried in Procrustean fashion to fit findings into an overly sociological framework, and it is of course perfectly possible that other preferable alternatives exist.

A further difficulty derives from the importance which ought, or ought not, to be attached to the term 'policy'. Ought the selection, and evaluation, of research findings to be made with a strict criterion of policy-relevance in mind? By contrast, since we are concerned with an area which lays some claim to academic respectability, ought the criteria of pure science (such as methodological sophistication, theoretical implications) to receive greater weight? It seemed to me that on this point some compromise was essential, and it is necessary to explain this compromise.

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1) For a treatment of the study of science and science policy within each of the relevant disciplines, the reader should consult Spiegel-Rösing & Price, (1977).

The report is oriented around two questions: what factors have been shown to affect the quality of scientific research[2] and the effectiveness of its utilisation? The structure of the report is an attempt to group these factors: individual, 'microsociological' (or organisational), 'environmental' and so on. This orientation has necessarily implied a selection criterion from amongst the literature, for not all the work which in one way or another may be said to lie within the field is concerned with such relationships. For example, work in the sociology of science focusing upon the reward system in science - the exchange of 'professional recognition' for contributions to knowledge - has been largely omitted, though it has been central to the sociology of science in the 1960's. Moreover, this central orientation (and the structure which follows) also implies a concern with questions of policy, since improvement of research performance and utilisation are the central interests of science policy. But in trying to keep the possible policy relevance of the studies in mind I have not gone much beyond this. I have not thought it appropriate to evaluate individual pieces of work with the single yard-stick either of theoretical sophistication or utility on the one hand, <u>or</u> policy relevance on the other. Nor, in writing the report, did I make any judgement of the relative importance of government policies concerned with the promotion of science on the one hand, or with the contribution of science to economic or other objectives on the other (the twin facets of science policy). In some places it was expedient to focus upon research related to one kind of policy; in other places to the other. Nevertheless, this particular issue is taken up again in the concluding section.

The structure of the report is as follows. The first section in a way 'sets the scene', for its focus is upon the various interpretations which are placed upon 'quality of research' and 'effectiveness of utilisation': the dependent variables in what follows. Thereafter the apparent determinants of these variables are grouped as follows. Section 2 deals with individual (psychological and sociological) correlates of research ability (or its surrogates). Section 3 is concerned with microsociological aspects (the internal organisation of research groups); Section 4 with the relations between researchers (or groups) and the scientific community; Section 5 with the relations between performers of research and its (potential) users; and Section 6 with researchers' relations with research-funding bodies. In Section 7 I turn from the determination of research effectiveness at the level of the research group to the problem of comparing and evaluating national research systems. Finally, some conclusions and views on the current structure and future development of science policy research are presented.

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2) Effectively, <u>non-industrial</u> research. I was supposed to restrict myself to the kinds of scientific research within the purlieu of the Advisory Board for the Research Councils - social sciences thereby being included.

1. THE EVALUATION OF RESEARCH

I want to begin by discussing the various criteria for evaluating scientific research at four levels of analysis:

1. The contribution of an individual scientist
2. The importance of a specific piece of work
3. The output or quality of a national research system
4. The value of a particular line of research or scientific discipline

It seems to me evident that we cannot assess the meaningfulness of a particular attempt at correlating quality with its organisational, economic or other determinants, without first clarifying our notion of quality. Moreover, a discussion of the evaluation of research provides a useful integrating framework for introducing a wide range of research studies in science policy. Orthogonal to the four levels of analysis listed above, and to be distinguished at each level, is a second dimension of the problem. We must attempt to separate out three kinds of criteria:

1. Those used in practice by scientists or policy-makers
2. Those used in academic analysis (by sociologists, economists, etc.)
3. The problem of meta-criteria (for evaluating the criteria of scientists or academic analysts)

1.1. The Contribution of a Given Scientist

Scientists are frequently called upon to evaluate one another's overall quality or ability. They do this in selecting faculty members, in electing to membership of the Royal Society or NAS, in the award of medals, and in the operation of the referee system, and so on. A number of (largely sociological) studies have explored the criteria which they seem to use. In particular, these studies have investigated the way in which these evaluations depart from the norms of science which require the consideration only of scientific merit. Caplow and McGee (1970) have shown how the 'old boy network' functions in appointments to the faculties of prestigious American universities. Diana Crane (1970) has demonstrated the barriers faced by the scientist of working class origins in securing such appointments. Hargens and Hagstrom (1967) have shown how the scientist who obtained his Ph.D. at a minor American university is unlikely ever to obtain a post at a major one. A number of recent studies (reflecting recent interest in gender as a dimension of social stratification), e.g. by Folger, Astin and Bayer (1970) have illuminated the bias against women usual in the academic marketplace. We know rather less about other sorts of evaluation processes, although there is anecdotal evidence to suggest that both personality and institutional affiliation are very relevant to his chances of being elected to the NAS. Not wholly irrelevant, we know that in totalitarian societies religion or political affiliation (at least the semblance of political conformity) can greatly affect a scientist's standing among his colleagues. In my view the findings of these various studies may be synthesised in the general statement that scientists' day-to-day evaluations of each other are very substantially influenced by the prejudices common in the societies in which they live.

Let me turn now to the indices of ability, creativity, etc. used by academic students of science and scientists. The simplest indicator used by sociologists has been counts of papers published by each scientist, sometimes stratified for age (so that one only compared the total production of a scientist with colleagues who have been active for a similar length of time). It was soon realised that if the attempt to assess a scientist's real contribution had to be made, then some correction for the varying quality of papers was called for. In this, the Science Citation Index proved an invaluable tool, the utility of which was demonstrated by Bayer and Folger (1966). The implication is that the more quoted by succeeding scientists is a given paper, the greater its impact. Thus, each paper published by a given scientist could be 'weighted' by its number of subsequent citations, and his total contribution thus assessed. Procedures based upon this notion are particularly developed in the work of J. and S. Cole. In their 1967 paper, for example, they attempted to correct for the fact that a large number of mediocre papers may attract as many citations as a smaller number of important ones. In place of total number of citations, therefore, they substituted the number of citations to a scientist's three most heavily-cited works. They attempted in addition to correct for the 'contemporaneity' of science by giving extra weight to papers which had remained significant over a long period (i.e. work published some while ago which was still being cited). Other indicators used have sought to approach more exactly to the evaluations made in practice by the scientific community. Probably regarded as best of all, though rarely used in practice because of its difficulty has been direct peer group assessment (see Blume and Sinclair, 1973, and Clark 1957). In a study of the productivity of university chemists, Blume and Sinclair used a composite index based upon such indicators of status as membership of the Royal Society and of Research Council committees, office in scientific societies, and so on (although these indicators are generally used in a rather different way in most sociological studies). Finally, I should point out that there is evidence to suggest that the various criteria to which I have referred are fairly closely correlated. For example, Clark, in his study of the research productivity of American psychologists, found a correlation of r = 0.67 between citation scores and peer group assessment.

Parallel indices have been used in the differently-motivated studies carried out (e.g. by Pelz and Andrews, 1966) of 'organisational', principally applied, scientists. They used both 'objective' measures - counts of published papers and technical reports - and 'subjective' measures - assessment of an individual by his colleagues and/or his superiors.

Psychological studies must be included here also. These are necessarily rather different, since they have been concerned not with socially-defined acknowledgements of research quality, but with the psychological characteristics of the creative individual. Notions of what is creativity are thus rather different, since for the sociologist of science creative research is defined socially or (in its more philosophical variants) in terms of the intellectual needs of the science. Psychologists have frequently chosen to define the creativity of an individual (who may or may not be a scientist) in terms of his responses to laboratory tests which are not derived from his normal intellectual activity. A second group of such studies have focused upon a pre-selected group of 'eminent' scientists, chosen either on the basis of an heroic theory of the history of science (Cattell) or by having a panel of scientists select them (Roe). Thus, concern with the psychological/personality correlates of creativity has resulted in an acceptance of the latter term as essentially non-problematic.

Reflecting on all this, do we find in it the sorts of criteria which seem wholly acceptable as measures of the quality of a scientist's work? It is important to recognise that most sociological studies have sought to approximate as closely as was feasible to the actual criteria of evaluation used in the social system of science. Productivity, citation, election, appointment and above all, péer-group assessment, have been seen as the sorts of indicators of achievement with which the scientific community operates. However, other studies within the same research tradition have demonstrated the extent to which these natural evaluation processes are biased by consideration of ascriptive factors. That is, they are influenced by the prejudices common in the environing society. How acceptable, then, is such a behavioural approach? A number of sociologists of science today consider that what scientists commonly do has to be treated as problematic, and is not necessarily 'correct'. The interests of the science at a given point-in-time may not be accurately reflected by the activities or judgements of the practitioners. The behavioural approach then becomes problematic, and we are forced to derive from a more theoretical formulation some underlying conception of value. In a later section, I shall have something more to say about this new cognitive sociology of science. For the moment, suffice it to say that because its focus is cognitive, because it treats questions of epistemology it is less directly concerned with people as units of analysis. In other words, it is more concerned to find means of evaluating the real worth of a scientific contribution than of a scientist (whose contributions may be to a specialism at varying stages in its development).

1.2. The Importance of a Specific Piece of Work

Rather different kinds of studies have sought to assess the value of a piece of scientific research

(a) for the development of the research field

(b) in the pursuit of some external goal

which we may term 'internal' and 'external' evaluations. The matter of which is appropriate, or what the proper balance between the two considerations should be in any particular situation, is not of concern for the moment.

1.2.1. Internal Evaluation

How do scientists evaluate the scientific significance of a piece of research? What determines their reaction to a scientific paper? Although, as I shall outline below, various behavioural indices have been used in studies of the research process, I do not think that this problem has been seriously tackled at the conceptual level. It seems reasonable to suggest that no single criterion is actually used, but that evaluation is based upon considerations such as utility in one's own research at one extreme, and general considerations of elegance, clarity, at the other. In other words, scientists will utilise a rather different mix of values depending upon the similarity between their own problems, hypotheses, experimental procedures, and so on - and those treated in another paper. When on occasion, they might read a paper rather divorced from their own work, they are thrown back upon very general conceptions of what science is about.

Now both sociologists and philosophers have indirectly concerned themselves with the valuation of new scientific developments, although their approaches have traditionally been rather different. Philosophers have tended to adopt a normative perspective, arguing about what science should be like, and basing their criteria very largely upon detailed considerations of physics alone. Their scorn for what actually goes on (see Lakatos' (1970) critique of Kuhn) renders the notion of utility of little relevance. In other words, it seems to me that their focus has been largely upon those more general values which scientists probably fall back on when examining a contribution somewhat outside their own specialist sphere of interest. By contrast, sociologists in the Mertonian tradition, have tended to go to the opposite extreme. They have been concerned to derive objective and preferably quantifiable indices of how scientists use the results of others, focusing largely upon the public manifestations of such use. Most notable here has been the use of the citation index. The number of citations to various pieces of work has been taken "to represent the relative scientific significance or "quality" of papers" (Coles, 1967). As described earlier, this procedure has been made more sophisticated by weighting for citations received after the paper in question has lost its immediate topicality. There are many weaknesses in the procedure. First, specialisms differ in their size and growth rates, so the number of authors who may cite a given paper is likely to differ from field to field. This effect is multiplied by the varying typical productivities of scientists working in different fields. Of course, citation outside the sphere of immediate relevance is possible, and is likely for a 'very good' paper.[1)] For example, papers describing new instrumental techniques, or new methodological or statistical techniques are likely to have wide currency. This of course reflects the kind of matter with which they are concerned as much as the quality of the paper relative to others of its kind. In other words, even were citation a wholly rational procedure (which it is not) what precisely it would reflect is a good deal more problematical than its users have appreciated. Second, we know that citation has a number of ritualistic functions in addition to its use as an indication of the utilisation of a specific piece of work. Although this has not been studied systematically, we know that citation of a man or his work may be a general mark of gratitude or indebtedness; it may reflect a wish to be seen to be familiar with certain classic or novel pieces of work; or it may be the prelude to criticism. Third, D. Price has shown that specialisms differ in their characteristic citation practices, e.g. in their relative citation of recent and archival papers (Price, 1970).

This behavioural approach cannot offer a truly valid criterion because it has been unconcerned with the cognitive development of sciences, just as philosophers of science have largely neglected their social structures and development. Sciences or specialisms may have needs at any one time (whether for data, methods, hypotheses, unifying theories, etc.) which may not be reflected in the work of the mass of practitioners whether for reasons of difficulty, availability of funds or whatever.

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1) Provided the findings are not too novel, and the field not too marginal.

Nor can they be deduced from any unitary model of how sciences should develop. It may be however, that the new approach to the sociology of science which focuses upon the interplay of cognitive and social processes will be of some value in clarifying these notions.

1.2.2. External Evaluation

How should we assess the importance of a piece of research in the pursuit of some extrinsic goal? This is of course a requirement of those concerned with the funding of most research, who may have to decide upon the relative value of research and other methods of attaining goals of economic growth, military preparedness, environmental control, etc. Other policy-makers may have the somewhat different concern of comparing the utility of two different research approaches or projects. In fact, the perspective of such policy-makers is generally future-oriented, and they are most anxious to assess the potential benefits of research in general or particular. By contrast (and especially until the last 2 to 3 years) the perspective of most academic analysts has been retrospective. For the purposes of this paper I shall not concern myself with the almost inevitably subjective assessments of practical men, but with the methods used in studies of the benefits of actual (not potential) research. It seems to me that only with the aid of techniques tested in that way can the more difficult prospective exercise be tackled.

There are two relevant kinds of evaluation:

- How important was (a given piece of) research in the realisation of an achieved aim? This requires the comparison of the contribution of research with the contributions of other inputs or factors.
- What is, or was, the relative importance of, or benefit accruing to, two different research projects or sets of findings? This requires a national scale of value of a rather different kind.

Both approaches have been used, and I shall give one or two examples of the use of each.

(i) Comparison of Research with Other Inputs to Innovation:

I want here to refer specifically to the TRACES and Hindsight studies on the one hand, and to the more recent work of Gibbons and Johnston in Manchester on the other.

Although as is well known TRACES and Hindsight reached somewhat different conclusions as to the importance of basic research in the innovation process, their methodologies were rather similar. In each case a number of significant innovations were chosen for study (civil in the first case, military in the second). Various categories of R and D were defined: e.g. TRACES distinguished non-mission research, mission-oriented research, and development and application. The procedure then was to have a group of experts reconstruct the history of the innovation under study, identifying each scientific/technical event necessary to the innovation process or to the scientific work upon which it seemed to rest.

> "The origins of a historical tracing were selected by the scientists as those research milestones which are recognised as marking the beginning of the various distinct lineages of scientific speciality that contributed to the innovation" (TRACES)

In each case every event so identified was clarified as mission, non-mission, or development, and the number of events in each category counted. A conclusion of TRACES is then that "Of the key events ... approximately 70 per cent were non-mission research, 20 per cent mission-oriented research, and 10 per cent development and application." Do we then have a method of quantitatively assessing the contribution of basic research to innovation? There are a number of points which must be made. First, since it includes scientific and technical events only, the method permits only the comparison of various categories of such events: market forces (for example) are excluded. Second, and this is acknowledged as a major source of the divergence between TRACES and Hindsight, the selection of an historical starting point for any innovation is both arbitrary and critical. Third, each event identified is quite arbitrarily accorded equal weight - the whole universe of scientific events is divided into the 'critical and necessary' and the 'wholly irrelevant'. Is it meaningful to assume that Maxwell's work on the electromagnetic wave theory of light (1864) and Gabor's construction of a magnetic lens (1927) were equally important to the development of the electron microscope? If it seems wrong to make this assumption, or at best simplistic, how do we attempt to assess their relative importance? This has not been tackled. Finally, the basis of Gibbons' and Johnston's critique - and the starting point for their own work - is in the tacit assumption that a series of scientific and technical events are both the necessary and sufficient conditions of an innovation. That is, there is an inherent unidirectional causality deriving technology from science. The apparently firm quantifications resulting from this approach must be seen in the light of the assumptions upon which they rest. In my opinion the questionability of these assumptions are a severe constraint upon the value of the findings.

Gibbons and Johnston (1973) focused upon a set of recent, or on-going industrial product innovations. Data collected directly from the individuals principally involved in each innovation permitted parallel reconstruction of the history of an innovation and the identification of all critical technical problems which had had to be overcome. Subsequently, all the inputs of information which relevant individuals had used in solving these critical problems were identified by interview, and classified. Among the conclusions that the study yielded were the following:

> "Slightly more than one third of the information-inputs from outside the company which led to the resolution of technical problems occuring during innovation can be classified as resulting from science; the remainder are principally technological."
>
> "One third of the total of information inputs obtained from outside the company are in the form of scientific literature reporting the results of original research".
>
> "In over half of all the innovations no scientific literature was used at all. However, [when] it was used, it was relied on a great deal".

This work gives us an indication of the kinds of innovation to which science is relevant, and of the kinds which may be understood solely in terms of technological progress. It shows that science may be 'tapped' in different ways - via education, personal contact, the research literature - to yield useful information. Elucidation of the complexity of the coupling between science and technology is a major achievement of the study. Even so, the approach does not permit the assessment of the importance of a particular piece of research to a particular innovation. Research becomes defined as 'important' (if referred to by the problem solvers) or 'not important', and even though we may know more about the means by which it contributed, we do not know 'how important', it was. Moreover, implicit in the methodology is a particular conception of technological innovation as a sequence of individual problem-solving exercises, in which the broader socio-economic environment is of secondary importance.[2)]

To summarise then, these approaches give highly assumption-dependent indications of whether or not a particular piece of research was relevant to a given innovation. They do not offer any answer to the quantitative question of 'how important'. I am not at all sure that any such answer exists.

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2) In fact, when, as in the SAPPHO approach, these organisational/ environmental factors are introduced we learn rather less about the particular inputs of research - although a good deal about coupling between the research system and the organisation.

These, and other related, studies are concerned with hardware or process innovations. What of the possibility of assessing the contribution of research (whether in the social or physical sciences) to innovation in social policy or practice (e.g. modes of health care, other than drugs; educational change, other than educational technologies)? It is perhaps over-optimistic to search for the quantitative conclusion which the hardware studies could not produce, but what of the more general question of whether or not a piece of research was relevant to a given policy outcome or change of practice? In what instances can we say that a particular piece of research was, or was not, important, or to suggest (as Gibbons and Johnston were able to do) how policy-makers became aware of the research or pace SAPPHO (SPRU, 1970) to categorise the organisational conditions for innovative policy or social practice? To my knowledge there are no such studies, even though increasing volumes of research funds are directed towards what may be called social innovation.

(ii) The Comparison of Research Results:

A second meaning which I suggested could be attached to the term 'assessment of the external value of research' required not the comparison of research with other inputs, but the comparison of sets of research findings on some scale of value. This seems to require that we go somewhat further than was necessitated by the earlier groups of studies. We must now assume either that a research result can have a direct social, economic, or political benefit, or that some technological change mediates between the two. Outside the realm of social science and public policy, at least, the latter seems to be the more reasonable assumption. We then have:

RESEARCH -----> DERIVED TECHNOLOGICAL CHANGE -----> EXTERNAL BENEFIT

The question then becomes: can we associate a (quantifiable) benefit accruing to a technological change with some antecedent research result? Clearly there is no theoretical necessity for expressing this benefit in economic terms. The benefits of a new drug (by implication associable with prior research and discovery) might best be expressed in terms of 'prolongation of life' or 'decrease in distress'; the benefits of an educational change (e.t.v. or the E.P.A.s) might best be expressed in terms of 'improved scores on educational tests', and so on. I do not propose to deal with the utility of, or problems in, constructing social indicators at this point. Expression of value in cash terms is the simplest interpretation of the general problem, since economic indicators are well-developed and (relatively) uncontentious.

Thus, does the work of economists on research seem to offer, or at least to promise, a scale for valuing the results of research? First, a whole range of economic studies (as reviewed by Mansfield, 1972) demonstrate that at the national, industry and firm level there is an association between economic growth or increase in productivity and R&D expenditure. However, even at the firm level, there has been no concern with the sorts of R&D involved: it is a highly aggregated concept. Thus, most of this work seems unlikely to have much to say about the value of any specific type of research, let alone of a specific research project.

However, in 1968-69 Byatt and Cohen of the Department of Education and Science addressed themselves to the problem with which we are concerned here. Their thesis was as follows. It is theoretically possible to estimate the net economic benefit to the nation (or the world) of an industry, and to discount this benefit back to any chosen year (Byatt and Cohen, 1969).

> "If parts of this residual (net benefit) can be assigned to the earlier basic discovery or discoveries associated with the industry and essential to it and discounted back to the dates of each discovery, then the sums so calculated can be described as cash benefits associated with those discoveries. ... The only way in which it seems possible to estimate the value of particular scientific discoveries is to ask what the effect on this net profit would have been, if the discovery in question had been delayed (or accelerated) as a consequence of some changes in research expenditure".

This marginal approach seems to focus directly upon the issue with which we are concerned here, allowing the comparative valuation of different discoveries necessary (though not sufficient) to the creation of a specific industry:

> "One might, for example, deduce the cash value ... of the transistor industry in 1950, and enquire the relative importance of, for example, the introduction of pn functions (1949), semiconductor/metal boundaries (1941) and the quantum theory of semiconductors (1931) by postulating (such notional delays ..."

The further away in time any critical discovery is from the establishment of the industry, the smaller its economic value is to the industry. This is partly because the value of the industry discounted further back is smaller, and partly because even large delays in 'classical' discoveries must be seen as giving rise to no more than tiny delays in the industry.

Thus, the authors write "a relevant, indeed essential, scientific discovery, has an economic value to an industry only if it comes 'nearly at the right time' - when other necessary scientific discoveries are made, when the appropriate technologies are available, and when society is in a position to invest appropriately." This is one among a number of implicit assumptions in the approach which to my mind seem inherently reasonable.

Subsequently, a number of studies were commissioned in order to assess the practicality of the method. These focused upon a variety of specific innovations, and whilst some probed back into their scientific antecedents, others probed forwards into the financial profitability of the innovations. The general implication of the studies was that the method did not in practice permit the quantification of the economic returns upon scientific discoveries. Indeed, it was not even possible to impute quantified benefits to existing product innovations (let alone to the antecedent research). There were a number of reasons for this. First, firms did not themselves calculate their own return upon specific innovations, nor did they have the necessary data for this to be done. The diffusion of an innovation through an industry (e.g. by imitation) rendered the calculation much more difficult. Thus, even if the relative importance of the various discoveries antecedent upon an innovation could be determined by the procedure suggested, no absolute economic value to be apportioned between them could be calculated. (The major results of these exploratory studies were that scientific progress and technological change were only intermittently coupled: there was no necessary causal link. Profitability is not in anyway inherent even in an innovation, but depends greatly upon economic conditions, marketing strategy, responses of competitors, etc.)

1.3. The Evaluation of National Research Systems

When we turn to the comparative assessment of national research systems we are again faced with the possibility of both 'internal' and 'external' criteria of evaluation. To be sure, in many 'practical', or policy-oriented assessments the two are deliberately or implicitly confused. The approach of the OECD, for example, in both its Country Reviews and its recent cross-national studies of The Research System is to use a panel of experts with interests in different aspects of science policy. The end result is a series of analyses (of varying degrees of comparability) in which judgement is passed (e.g.) on the academic research system, the utilisation of industrial R&D, etc. Similarly, in formulating national science policies one's own country vis-à-vis others may be a powerful political weapon. However, for purposes of conceptual clarification, which is our concern here, it is useful to focus upon the more objective indices of internally and externally defined performance which have been used in analysis. Finally, I shall look at possible relations between the two.

First, however, it is worth referring to the (US) National Science Board's current attempts at developing a series of 'science indicators' "for describing the state of the entire scientific endeavour". In the introduction to their first report on this work, the NSB outline the potential usefulness of their indicators (NSB, 1973):

> "Such indicators, updated annually, should provide an early warning of events and trends which might reduce the capacity of science - and subsequently technology - to meet the needs of the Nation. The indicators should assist also in setting priorities for the enterprise, in allocating resources for its functions, and in guiding it toward needed change and new opportunities."

The first report dealt almost wholly with inputs to R&D: expenditure (divided up and expressed in a variety of ways), manpower resources (by sector, qualification, function) production of scientific manpower (by academic level, geographic region, duration of training), unemployment rates, etc.

But subsequently an attempt has been made to develop performance measures, and we shall refer to these below.

1.3.1. Internal Evaluation

In devising and using internal criteria, sociologists, historians and others have sought to compare the contributions made to science, or to a science, by various countries. The intention may be to seek to understand the evolution of science as a social activity, as in the work of Joseph Ben-David. By examining indicators such as on the one hand the nationalities of the great scientific innovators, on the other indicators of activity levels (see below) Ben-David (1971) showed that the 'centre' of world science moved progressively from England (17th century) to France (18th century), to Germany (19th century) and to the U.S.A. (20th century). But perhaps the best-known approach to comparative internalist evaluation is that of Derek Price. In one fairly typical paper ("The Distribution of Scientific Papers by Country and Subject - A Science Policy Analysis") Price offers a statistical analysis of the content of 1961 Physics Abstracts, by subject field and by country of publication of the journal in which each paper appeared. He writes:

> "Although the average quality of papers may well vary from field to field and also from country to country, we may take as a first crude hypothesis that the number of papers abstracted in each category is a measure of the activity of that country in that field."

Publication rate, then, is the first indicator of effectiveness. A second criterion is offered:

> "physics, like other basic research, is such an international currency of free exchange that it is very difficult (if not directly unwise) for any country to deviate from the overall world distribution of interest in the various subject fields"[3)]

Admittedly, the number of papers published in a nation's journals may not always reflect accurately the number of papers published by that nation's scientists. Large countries often profit from the international prestige of their major journals, whilst a few smaller countries possess one or two journals of international repute (e.g. Italy's Nuovo Cimento). Let us accept this as a problem of methodology only, and focus on the broader conceptual issues involved. On the basis of his first criterion, Price is able to rank the major physics-producing countries: U.S.A. (31.1 per cent of all papers), U.S.S.R. (16.4 per cent), G.B. (13.5 per cent), Japan (7.7 per cent), and so on. It seems to me that the weaknesses of this approach are certainly no greater than the use of paper-counts for evaluating individual performance, and they may (as Price believes) be smaller. That is to say, it provides an acceptable starting point for internalist evaluation susceptible, in principle, to improvement.

Science Indicators (NSB, 1975), for example, does two things more. In the first place it examines trends in the relative share of the USA and certain other nations in the literature of various scientific fields. On this basis it is able to conclude that "The international position of the United States may be declining in the fields of chemistry, engineering and physics. The U.S. share of the literature in each of these fields declined slightly in both 1972 and 1973..." Secondly, it attempts to 'weight' each nation's literature output by its quality. Whilst recognising the limitations of citation index counts, this method of evaluating scientific output is of course legitimated by practice and by demonstrated correlation with other quality measures. Using all the Science Citation Index for 1973 an indicator was computed for each major nation and area of science. "The index was created by comparing the actual fraction of the world's total citations in a given field with the expected proportion based on that nation's share of the total publications in that field" (thus, for example in the biological and biomedical research areas, indices worked out at: USA 1.3, UK 1.2, Japan 0.8, West

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3) To which a caveat is added later: "The most cogent argument for a deviation from norm would be the existence in a nation of an unusually large technological sector related to the subject field in question, as with German optics and Japanese solid state physics."

Germany 0.8, France 0.6, USSR 0.3. That is, the USA and the UK produced literature which was 'overcited').

Alternative methods of evaluation could, of course, be devised: based for example upon assessments carried out by scientists for the purposes of the study. A panel of 100 physicists might be asked to each individually give the names and locations of the 20 'most eminent' physicists (a procedure which has been used in ranking academic departments in the USA). Or judgements actually made might be used, as for example in counts of Nobel Prize recipients. This, because of its simplicity, has often been attempted. Unsurprisingly it turns out that in the period 1901-1974 the USA received more science prizes than any other country, and that this is also true of each individual post-war decade. However, the picture looks somewhat different when these numbers are expressed by reference to population size. On that basis, among the major prize-winning nations the United Kingom leads the USA. But still more important, as the second report on Science Indicators points out (NSB, 1975, p. 13) is the fact that other countries, such as the Netherlands and Switzerland, have received a still greater number of Nobel Prizes per population than either the UK or the USA. Clearly, different kinds of indicators can be developed which reflect different aspects of scientific strength, quality, or level of activity. Though we now know quite a lot about the statistical interrelationships of these various indicators (as is discussed in various places below), the fact is that little effort has been made in elucidating their conceptual interrelationships.

Counts of papers are in my view best regarded as reflecting 'activity levels'. I use this term deliberately to stand for what is being measured, since I am not sure to what extent it is a more meaningful indicator than, say, numbers of scientists. If we are concerned to compare the 'climates' for research of different countries (as we might compare different forms of research organisation) then we need a more sophisticated index. The computation of the average number of citations per paper published in each country and field of science was a move towards just this. But from a slightly different perspective, input measures seem appropriate: for example, availability of resources, access to graduate students, scope for consultation with colleagues, etc. Factors such as these also seem in a sense properly to characterise the 'supportiveness' of a research environment.

Thus, comparisons of nations' contributions to the world scientific literature, though useful and quantifiable, have to be interpreted with care. It is true that if nations are ranked on this measure and then again on receipt of Nobel Prizes, broadly similar rankings are obtained, even though the considerations underlying award of these Prizes may be assumed to be different. Ben-David has taken the similarity of these measures and others referring to different fields of science[4] as strong evidence for the validity of the resulting ranking - in which we may thus have a certain confidence.

What finally of Derek Price's second dictum: that for any country the distribution of effort (i.e. of research papers) between fields of science should correspond to the world norm? Is this to be taken as a yardstick for evaluating research output or (as I think it was intended) as a directive to policy makers? Science Indicators is circumspect and produces profiles of this kind without much comment, e.g.

> "The 1973 profile of the United States was most similar to that of West Germany and the United Kingdom in the relative proportion of the total literature in each field... The profile of France's scientific research also resembles the United States except for a smaller proportion of engineering research on the part of France...
>
> The country with the profile which differs most from the United States in the literature studied appears to be the USSR. The life sciences... represent nearly 55 per cent of the US litterature compared with just over 20 per cent of the Soviet..." (NSB 1975, p. 12)

There seems to me no obvious reason why national traditions, reputations, the interests of the scientific community, and socio-economic priorities (whatever the relations between interests and priorities), should be sacrificed to some 'inherent logic' of the scientific enterprise thought to reside in 'average behaviour'. That is, I think the valuative or normative significance of such profiles is highly problematic.

1.3.2. External Evaluation

Implicit in the last section was the notion that the variety of indicators potentially available reflect different aspects of the scientific capacity of nations. The same is true here, in that to seek to compare national performance in terms of technological innovation, the 'impact' of science, etc., is by no means to specify what is to be measured. A range of indicators may be imagined, including, for example, counts of seemingly useful discoveries or inventions at one extreme, and the contribution of R and D to economic growth at the other. Thus, in the

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4) Original contributions in physiology, relative share of discoveries in the medical sciences, references in standard psychological texts.

one case we may wish to compare nations' capacities to support inventive activity (without specifying the relation of that activity to scientific research) - in the other a complex of factors involving also industrial organisation, availability of risk capital, international politics, etc.

Science Indicators develops indices reflecting many of these different dimensions, though the actual data given are designed principally to show changes in the performance of the USA compared to other countries (and not to rank these 'other countries' by reference to each other). One such indicator is the "patent balance", which for any country is the number of patents granted to its nationals abroad minus the number of patents granted by that country to foreigners. This index (which for the USA fell substantially between 1966 and 1973) is seen as a measure of "inventiveness". A second indicator is based upon a study of 500 innovations ("new products or processes embodying a significant technological change") introduced commercially between 1953 and 1973, and considered (by a panel of experts) to be of particular importance. The proportion of innovations produced by (each of 5) countries could be estimated for 3 year periods. The nations were also characterised by the mean number of years elapsing between their innovations and the inventions on which they were based (although this is at least recognised as problematic!), and by the 'radicalness' of their innovations. Finally some more strictly 'economic' indicators are given: payments and receipts for patents, manufacturing rights, licences etc. ('technological balance of payments'); and balance of trade in R&D-intensive products (strictly defined, to include only chemicals, electrical and non-electrical machinery, aircraft, and scientific instruments).

(Conclusions broadly demonstrate the importance of the USA in international technology, and the extent to which that country has profited from its technology. The 'technological balance of payments' was increasingly positive through the period 1960-73, and in 1974 the balance of trade in R&D-intensive products offset the negative balance in non-R&D products.)

The indicators taken as reflecting an external evaluation of the national research capability clearly focus on different stages of the innovation process: invention and patents at one end, trade balance at the other. But the picture one obtains of relative national success seems not to depend upon the precise choice of indicator: rankings are broadly similar. This is demonstrated quite clearly by Pavitt and Wald's study of Technological Innovation for the OECD (OECD, 1970). They employed six indicators (previously used in the OECD's Technological Gap exercise, OECD, 1970):

- location of 110 significant innovations, since World War II
- monetary receipts for patents, licences, know-how (1963-64)

- origin of technology imported by Japan (1960-64)
- patents taken out in foreign countries (1963)
- export performance in research-intensive industries (1963-65)
- export performance in research-intensive product groups (1963-65)

The authors find that "despite the limitations (in the interpretation of the data) when these six indicators are corrected for differences in country size ... there is statistically a high degree of concordance in each country's rankings." That is, it seems that the composite rank index derived may be confidently taken as indicating relative success in the utilisation of R&D. For all practical (policy) purposes this may be enough.

1.3.3. Concluding Remarks

First, what of the relationship between the two sets of indicators: the 'internal' and the 'external'? Is there any relationship between a country's performance judged in purely scientific terms and judged in terms of its capacity to utilise the results of research? Again we can usefully turn to Pavitt and Wald's report, since they sought to correlate their composite index of technological performance with a variety of indices reflecting scientific investment and performance. At least three of these are relevant here: number of scientific abstracts; QSE in R&D per head of manufacturing population; Nobel Prizes in physics, chemistry, medicine/physiology per 10,000 population. Rankings on these indices were correlated with rankings of the composite index of tehcnological performance, with varying results. Thus sample correlations were:

- technological performance and Nobel Prizes r=0.92 (sign. at 1 %)
- technological performance and abstracts r=0.67 (sign. at 5 %)
- technological performance and QSE in R&D r=0.29 (not sign. at 5 %)

The complexity of the situation begins to emerge from its cloak of statistical simplicity! It has appeared hitherto that the evaluation of national performance is a rather simpler (or at least less contentious) matter than the evaluation of the work of a scientist, or of the value of a piece of work. I believe that this has appeared so because the focus has throughout been on rankings, rather than on the search for measures, and the concordance between the various rank orders has resulted in a good deal of confidence in them. There are two things to be said about these useful, but unclear, results. The first is that the magnitude of the differences between countries involved are so great that the rank orders are highly insensitive (to small variations).[5)]

5) I would be happy to be corrected on my intuitive statistics!

The second is that interest in national research systems has been largely policy-oriented, and the rankings are sufficiently useful that there has been no incentive to seek greater conceptual clarification of such measures.

Of course, there are exceptions to any such broad generalization. There are countries which have profited far more from technologically based industries than their research effort would have implied: Japan always used to be cited as an example. On the other hand there are countries which appear to profit far too little. In a study of Israel, Wald found that in many respects its scientific potential more or less equalled those of a number of European countries. However, he points out, "the similarity of Israel, Sweden, and Switzerland in scientific strength is not paralleled by any similarity in economic wealth or industrial strength. There are many reasons for this gap... In the case of Israel, unique historical reasons made it inevitable that industrial and technological strength lagged more behind scientific strength than in other countries. Such a gap can be seen in two very different ways; as an unusual scientific proficiency of a poor country, or as a deplorable inability of a country to use its own scientific wealth." (Wald, 1972) Preferring the second explanation, Wald is led to consideration of structural aspects of the Israeli scientific-technical-industrial system, and of the values embodied in that system. We will take up questions of that sort in Section 7.

1.4. The Evaluation of Research Areas or Disciplines

I turn finally to what is perhaps the most difficult kind of evaluation. How can the value of research in physics be compared with chemistry or theoretical chemistry with analytical? The first thing to be said, is that evaluations of this kind are made, practically by science policy bodies such as the Research Councils and their committees. To some extent at least the distribution of Research Council support between the areas of science represents (or could be said in justification to represent) some kind of assessment of their relative worth or potential for development.[6] A less controversial statement may be culled from a Report of the SRC's Chemistry Committee (SRC, 1971):

> "...the Chemistry Committee have evolved a flexible grants policy which ... has permitted an experiment by which some funds have been set aside for enhanced support of certain selected areas ... designated by virtue of their promise and potential ... (e.g.) Organometallic Chemistry and Photochemistry."

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6) Of course, other factors are relevant: number of people working in the field, the costs of typical experiments, needs for trained manpower, and so on. Yet at some level in the funding process (if not at the Research Council level then at ABRC or Departmental level) allocations must in some way reflect an implicit evaluation of relative worth.

Such relative judgements are made, and it will be useful to try to ascertain their basis. Also characteristic of deliberations in practical science policy is the confusion of internal with external criteria. This is largely intentional, since on the whole such judgements are meant to reflect a (varying) balance between internal and external benefit.

But once more analytical clarity requires that we seek to distinguish these two. Our difficulty is that, to my knowledge, few social scientists have sought to deal with this problem in either sense. I can do little other than to step back to the level of what I earlier called 'metacriteria', and discuss how we might set about designing appropriate indices. There are various definitions which might be given of the internal and external dimensions of the problem:

Internal

> It is meaningful (or possible) to say that one discipline (or research area) is inherently more fruitful than another?
>
> Is it meaningful (or possible) to say that at a given point in time one discipline (or research area) is more fruitful than another?
>
> Is this the same as saying that field A is advancing more rapidly than field B? Or that the chance of a breakthrough in field A is greater than in field B?

External

> Is it meningful (or possible) to say that one discipline (or research area) generally contributes more to the solution of practical problems than another?
>
> Is it meaningful (or possible) to say that at one point in time one discipline (or research area) contributes more to the solution of practical problems?
>
> Can we say that one research area is inherently (or temporarily) less moral than another (i.e. that it creates more ill than good compared to the other)?

Let me deal first with the problem of internal criteria. I believe that most scientists would accept that at a given point in time one area of science may be said to be more fruitful than another, but not that it is inherently more fruitful. Scientists are constantly making judgements, choices, which reflect their evaluation of the various areas of science - both as individuals and as a community. Thus scientists choose their research areas; this is perhaps the major individual process of choice.

Among communal manifestations of such relative assessments we may note the following: relative numbers of papers accepted for publication; numbers working in each field; number of journals in each field; relative representation in the Royal Society/NAS, etc. We know, however, that all of these choices are strongly conditioned by social and personal considerations. Scientists' movement from one research area to another has been shown to depend in part on relative competitiveness, the possibility of "making a move" (Ben-David and Collins), and on career strategies (Nowakowska). The relative numbers of papers published, or the relative productivity of scientists, in various fields may reflect varying standards of journals, the relative ease with which results may be obtained, or the degree of consensus on problems and solutions (Blume and Sinclair, 1974). Numbers of journals may also reflect degree of consensus as well as commercial pressures. Representation in NAS, Royal Society, has been shown to depend in part on the disciplinary and institutional loyalty of those already members (Blume 1974a). Thus as in our discussion of criteria for assessing the relative value of specific research results we are faced with the problem of the appropriateness of such behavioural criteria. As I said at that point, the general tendency among sociologists of science (notably in W. Germany and the UK) is to regard the actions, accounts and judgements of scientists (shown to be socially conditioned) as problematical. This approach should permit greater theoretical clarification of the concept of "fruitfulness": under what circumstances is it best regarded as implying "rapid rate of advance", under what circumstances as "potentiality for breakthrough"? It is not easy to find a brief statement of the aims of this theoretical enterprise, which are rather at the formative stage,[7] but it ought to allow some comparison of the real needs of a scientific discipline or speciality with the general activities of its practitioners. I am frankly not certain whether it might also offer criteria for evaluating the actual or potential contributions of a speciality to science in general. But I do not see that any other analytical approach which we now have can do this.

In seeking to clarify the criteria of evaluation we do not have this choice! There have been a number of academic studies which have partially sought to assess the effect of specific research areas or traditions and to explore the process of this effect: e.g. the contribution of economic theory to economic policy (Winch), of social theory to social policy (Pinker), the return on agricultural R&D (Griliches), the relations between chemical research and the chemical industry (Haber, Langrish). But the conceptual frameworks and methods of assessment have varied so much that they do not add up to a method. And even within economics, I am not aware that there have been many studies which have focused upon the economic impact of a variety of research traditions.

7) Though see, for example, Mendelsohn, Weingart and Whitley (1977).

When we turn to external evaluation, we again find that such comparisons are constantly made in practice. Given a (politically salient) social problem, which scientific discipline, research tradition or method is most likely to 'deliver the goods'? Is a mighty problem-focused 'war' likely to be more effective than expansion in the basic research fields upon which solution may depend? Put in another way, as Nelkin puts it (Nelkin, 1977):

> "When is the state of knowledge in a field sufficient to warrant a massive development program? This is a difficult question contingent upon the 'ripeness' of a science, the funding available, the distribution of scientists and other factors. It poses particular problems in the biomedical area..."

Even to introduce the term 'ripeness' of a field is to hint that we cannot answer the question, since we cannot really say what we mean by that term, any more than we can by the term 'fruitfulness'. The other side of the same coin is that we often cannot tell where the idea(s) necessary for the solution of a social problem will come from. The intuition of scientists is the best guide we have, and when scientists profoundly disagree (as over the American cancer programme) then there is no guide at all. Science policy research needs far more sophisticated models of the development of science in order to contribute on fundamental issues such as this. Of course science policy-makers, especially when under political pressure, have to make some decisions, have to do something. And what they tend to do, quite properly under the circumstances, is to 'hedge their bets' by putting some money on each horse.

I want now, in the following sections of this report, to summarise some of the work which has sought to explain differences in quality of research, productivity, efficiency of utilisation, and so on. I begin with work using characteristics of individuals as explanatory variables.

2. PERSONAL FACTORS

This section therefore raises the first set of factors which have been related to scientific performance: characteristics of individuals. I want to summarize some of the relevant findings under two headings, distinguishing two different kinds of influence: personality and social statuses. For a number of reasons this work is discussed only briefly. In the first place its relationship to policy decisions (though perhaps not to research management) is extremely tenuous. Secondly, whilst all the pertinent literature does focus upon a common unit of analysis - it has little more than that in common. In particular what it is that has to be explained, 'performance' (or its surrogates) differs substantially from one kind of study to another, since much of the work deals with the ability or creativity of groups within which scientists may be only an element.[1)]

2.1. Personality Factors

A brief word should be given as to the psychoanalytic approach to, essentially, creativity. Much of this work has hinged upon the interpretation of the creativity of (frequently) artists in terms of such Freudian and Jungian concepts as "unconscious fantasies", and "sublimation", "self-actualisation" (Maslow). For many of these writers the notion of creativity refers less to the creation of something objectively new than to a mental process (which may produce something new only to the individual). The crucial question, for this group of writers, is how some people's wish to be creative can be explained. For example, Kris (in a classic work on artistic creativity) argues roughly in terms of a process: traumatic experience ---> mastery through daydream or fantasy ---> artistic creativity as the expression of this daydream ---> location of an audience who will welcome the expression of conquest.

In one of very few attempts at basing any kind of prescriptions upon this work, Kubie points to the various ways in which unconscious urges can affect choice of career, the nature of the problems undertaken, and the satisfaction derived:

> "These factors need to be understood by the scientist in order to prevent distortion and personal bias in his results, stifling of productivity by neurotic needs, or lack of satisfaction and contentment such as can lead to giving up in the face of success ... The

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1) Reviewing (lack of) progress in the psychology of science, Fisch points out that "Lacking integration, substantive research in the field has been spasmodic, discontinous, and fragmentary". See Fisch (1977).

> development of the intellectual life as an outlet for other frustrated needs leads to a supercharging of the research with many irrelevant and unfulfilled emotional needs. ... The emotional maturation of a scientist should, therefore, not be left to chance, but should be emphasised in some process of developing insight into his own motivations."

In addition to work within this psychoanalytic framework, there is a considerable volume of empirical work on more strictly psychological determinants of creativity. C.W. Taylor and F. Barron have attempted to summarise much of this work. They write (Taylor and Barron, 1969, pp. 385-86):

> "A highly consistent picture of the productive scientist has emerged from the researches of Roe, McClelland, Barron, Saunders, MacCurdy, Knapp, and Cattell, though the methods employed in these researches were highly varied, ranging from clinical interviews and projective techniques through empirically developed biographical inventories to factor-based tests. This or that investigator may use slightly different terms, depending upon his theoretical preferences or biases, but so consistent is the common core of observation that little is needed in the way of translation from one terminology to another. In what follows we shall try to abstract from these descriptions a single unified delineation of the productive scientist by listing the traits which are found in study after study.
>
> 1. A high degree of autonomy, self-sufficiency, self-direction.
> 2. A preference for mental manipulations involving things rather than people: a somewhat distant or detached attitude in inter-personal relations, and a preference for intellectually challenging situations rather than socially challenging ones.
> 3. High ego strength and emotional stability.
> 4. A liking for method, precision, exactness.
> 5. A preference for such defense mechanisms as repression and isolation in dealing with affect and instinctual energies.
> 6. A high degree of personal dominance but a dislike of personally toned controversy.

7. A high degree of control of impulse, amounting almost to over-control: relatively little talkativeness, gregariousness, impulsiveness.
8. A liking for abstract thinking, with considerable tolerance of cognitive ambiguity.
9. Marked independence of judgement, rejection of group pressures toward conformity in thinking.
10. Superior general intelligence.
11. An early, very broad interest in intellectual activities.
12. A drive toward comprehensiveness and elegance in explanation.
13. A special interest in the kind of "wagering" which involves pitting oneself against uncertain circumstances in which one's own effort can be the deciding factor.

Some of these traits are descriptive of productive scientists in general, while others are especially pertinent to the appearance of originality specifically in the scientist who is productive."

The work summarised is based upon a variety of studies, utilising the analysis of biographies, clinical interviews, tests such as Rorschach and TAT. The focus of study has been 'eminent scientsts' both dead (Cattell) and living (Cattell, Roe), graduate students rated as original (Barron), orginal/less original Air Force Captains (Barron), gifted adolescents (Getzels and Jackson), etc. The fact is that 'creativity' is a concept usually used by psychologists in an individualistic and sometimes quite technical sense. That is, the social or economic valuation of creation is not usually relevant. The scientific community, like the economic market, tends to be much concerned with the value of invention or discovery, and for the purposes of this review we are interested primarily in the personal correlates of socially (including economically) esteemed creativity.[2)] A high score in a psychological test in itself tells us little or nothing about an individual's creativity in this sense. This is a severe limitation upon the integration of this psychological work with other studies within the framework of this report.

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2) Though, as Hill and Jagtenberg point out, creativity in this sense (e.g. making unexpected associations) may correspond with scientific worth in "immature" scientific fields (Hill and Jagtenberg, 1977).

Some exception must be made for the work of Roe, who has tended to focus upon scientists deemed creative (or productive) by the professional communities in which they work. For example (Roe, 1952), she has suggested that eminent natural scientists differ from eminent social scientists in a number of respects (though admitting that the same differences may not hold between the less eminent). Thus, she observes differences between the two groups in psychosocial development as well as such current psychological traits as attitudes to parents, importance of social relationships, kinds of imagery employed in thinking, and concludes with the interesting speculation that "it is likely that the kind of person who has gone into social science may have had a biasing effect on the theories produced by social scientists."

A further exception must be made for Rossman's forty-year-old study of inventors (holders of patents).

When we turn from psychological to sociological studies we find a somewhat greater diversity. Much of the work has been conducted within a 'sociology of science' perspective, and has therefore defined its dependent variable in terms of the usual approximations to scientific creativity, e.g. number of papers published. Other authors have used sociological independent variables to explain technically and not socially based measures of ability (e.g. IQ). There is some evidence for very <u>slight</u> relations between these two kinds of creativity measure, (IQ and scientific production) casting some doubt on the possibility of a real synthesis between these two kinds of work (see Bayer and Folger, 1966, Cole and Cole, 1973, 1968-70).

2.2. Social Statuses

There is a good deal of evidence of the under-representation of lower socio-economic groups, women, and (in the USA) blacks in the scientific community. To a large extent this reflects the selection procedures leading to higher education (including, of course, self-selection). However, there is also some evidence that such factors continue to affect the course of an individual's career after qualification, though this evidence is not weighty. Thus, Crane (1969) found no correlation between the class origins (measured in terms of father's occupational level and education) and productivity of scientists. (A conclusion borne out by Gaston's study of British high energy physicists and by Blau's recent (1973) study.) However, she does find evidence for substantial bias against working class scientists in the course of their career. Faculty members of lower class origins are less likely to be found in <u>major</u> (American) academic institutions. This seems to derive from two independent mechanisms. On the one hand, lower class scientists are likely to have graduated from lower status institutions; and this in itself makes subsequent appointment to a high status institution unlikely

On the other hand, even when institution of (post graduate) training is held constant, Ph.Ds of lower class origins are likely to be found in poorer universities and colleges. Blau reaches a similar conclusion "Faculty recruitment in the best academic institutions reveals a bias in favour of candidates with middle class origins" (p. 96). West, whose conclusions are similar, suggests how this may operate. He suggests that because the values and attitudes of working class students are less similar to those of their teachers than are those of middle class students, they are less able to form effective (and ultimately rewarding) relationships with faculty members (West, 1961). Gaston, considering the community of high energy physicists in Britain reaches an opposite conclusion, finding no evidence that reward for scientific achievement (which is how American sociologists of science conceive appointment to a prestigious university) is affected by class background (Gaston, 1973).

Studies within this sociological tradition of the relevance of sex status has also tended to focus upon career implications (that is, bias in the communal perception of achievement) rather than directly upon acievement itself. However, we may refer to the study by Cole and Cole (1973) and other work reviewed by them. It is apparent that American women scientists are less likely than men to hold tenured posts in high status departments and that they tend to have lower salaries. Harmon had found that men and women science Ph.Ds had roughly similar IQs (Harmon, 1965), but Cole and Cole did find that women scientists were, on average, less productive than men (published less over their careers), and that these differences could not be attributed either to family commitments or to the kinds of institutions in which women tended to be employed. Cole and Cole's conclusion is therefore that the apparent bias against women scientists (expressed in terms of job status, salary, 'honorific rewards') is directly attributable to their lower productivity. The latter, however, remains to be explained.[3] For this, even according to the Coles, "it may be necessary to look outside the institutional structure of science; to examine carefully the prior experience and socialisation of women in the larger society..." (p. 138). Of course, it is now largely accepted that the direction of girls away from scientific careers is initiated by early culturally-based sexual stereotyping. Many would therefore argue that these crucial early pressures and predispositions derive more from considerations of the social acceptability of roles than of intellectual abilities. (See Groth, 1975; Joesting, 1975.)

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3) And is in fact disputed. Other studies have found no difference in productivity, e.g. between full-time employed men and women. See, inter alia Simon et al (1967-8).

There is some sociological interest (not within the framework of sociology of science) in the ways in which the family experiences of the child may influence subsequent academic abilities and achievement. Bernstein argues that working class modes of speech do not involve the use of abstract modes of thought and complex grammatical constructions, thus disadvantaging the working class child in his early contacts with teachers (Bernstein, 1969). Getzels and Jackson (1961) attempted to compare the family situations of two groups of gifted adolescents: one group of high IQ, one group of high 'creativity'. By interviewing the children' mothers, they discovered that the high IQ children tended to come more usually from families in which early financial insecurity had led parents to stress the need for security. These parents were in addition more 'vigilant' with respect to children's behaviour and friends, and more concerned with their academic performance. In addition, they found that the parents of high IQ children tended to read more than the parents of 'creative' children.

It is difficult to acknowledge any but the most tenuous connection between this work and most other work which we shall be discussing. The precise relevance of work such as Crane's account of class biases in the rewarding of achievement (rather than in achievement itself) depends upon how 'objective' a criterion of achievement is sought. (After all, even quantitative measures of publication depend upon prior perceptions and evaluations by others.) Explanations of academic achievement in terms of family environment seem most relevant as a source of hypotheses were this kind of inquiry to be taken further with special attention to scientists. To my knowledge it presently is not.

When we turn to consider the influence of 'education' on scientific achievement, the field of interest is potentially vast. After all, under 'education' can be subsumed a wide range of variables and relationships. For example: does educational performance correlate with subsequent scientific achievement? What are the effects of all the organisational variables (e.g. selection, streaming) with which educational reformers are concerned upon scientific careers? Do certain kinds of institutions 'foster' scientific talent more effectively than others? Is the situation the same in the applied sciences and engineering? Can an education system (and does ours) produce a 'maldistribution' in undergraduate interest in the various fields of science and engineering? How does educational background affect the process by which scientific achievement is perceived and rewarded by the scientific community?

In order to select from amongst this potentially vast field it is useful to bear in mind that in the UK few of these issues have been considered,[4] and also the criterion of policy-usefulness. The production of

4) In the USA more work has been done (notably by L. Harmon and colleagues making use of the National Research Council manpower register).

qualified (or talented) scientific manpower is only one function of an educational system, and perhaps even a relatively unimportant one (at least in political terms). In other words relationships between aspects of educational structure and the fostering of scientific talent could not often form a sufficient basis for restructuring the education system. I shall therefore limit myself to two kinds of relationship which have been examined and which might have rather important policy or administrative relevance.

First, I want to refer to some work done in the late 1950's by L. Hudson. His interest was in the extent to which class of first degree was a predictor of later scientific achievement. In two papers (1958, 1960) he examined the degree classes of Fellows of the Royal Society who obtained their first degrees at Oxford or Cambridge in the period 1920-1939 - i.e. using fellowship as a criterion of success in science - and compared them with a control group of non-members who graduated at the same time from these universities. A parallel study was made of D.Scs and non-D.Scs (a somewhat less exigent criterion of success). In the case of Cambridge FRSs, and all D.Scs, undergraduate achievements were not at all correlated. Only Oxford FRSs were significantly more likely to have had firsts than their control.[5] Since Cambridge FRSs were three times as numerous as Oxford ones, degree class (on average) is not a good predictor of subsequent research potential. This conclusion which is supported by Gaston's non-correlation between degree class and research productivity among British high energy nuclear physicists, would seem to have some implication for the allocation of postgraduate research awards etc.

Secondly, I want to consider the effects of different kinds of educational background. The classic study here is that of Knapp and Goodrich (1952). These authors attempted to examine the extents to which various universities/colleges with different characteristics produced students with bachelor degrees who went on to become successful scientists. "Productivity indices" were computed for the 490 institutions, based on the proportion of their male graduates between 1921 and 1944 who became starred or listed in _American Men of Science_. Relationships were sought between these indices and eighteen variables relating to size, endowment per student, entry requirements, qualifications of faculty, etc. One conclusion, which is now rather well-known, is that the (liberal arts) colleges tended to have higher indices than the (larger) universities. This finding they tried to explain in terms _(inter alia)_ of the following considerations:

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5) Which he interprets "the Oxford Final Honours School is, for whatever reason, a more valid index of potential research ability than the Cambridge Tripos".

(a) smaller schools have a 'community' atmosphere which facilitates creative personal relations between teachers and students;

(b) faculty devoted wholly to undergraduate teaching;

(c) limited resources lead to a concentration on basic, non-vocational courses, which stimulates intellectual interests.

It is doubtful if the same kinds of relationships could ever be demonstrated for the much smaller and less diverse British higher education system. However, conclusions of these kinds might have relevance for the organisation of work in any academic institution, although of course they would have to be taken together with rather different studies on the implications of academic organisation (see, for example, Orlans, 1962; Halsey and Trow, 1973; Blau, 1973).

There is a good deal of additional evidence on the way in which academic origins influence an individual's career once he has become a scientist, at least in the USA. For Britain, Gaston found no relationship between type of university attended at the undergraduate or postgraduate levels and subsequent productivity or 'recognition' of high energy physicists.

Let me turn finally to the relationship between age and creative achievement. The classic work here is Lehman's Age and Achievement (Lehman, 1956). In it, the author identified major discoveries made in various fields of science with the help of standard histories and advice of experts in each field. Slightly different procedures were employed in fields such as literature, philosophy, painting. The age of the scientist making each of these identified discoveries, at the time of the discovery, was noted. Broadly speaking, major contributions tended to be made in the relatively early years of professional work (chemistry 26-30, mathematics 30-34, philosophy 35-39). Lehman concludes that creativity generally rises rapidly to attain its maximum in the thirties and then declines slowly: the more brilliant the early work, the more rapid the decline. Differences between subjects exist because in those that require learning and unlearning, old people are greatly handicapped,[6)] whilst when an accumulation of past knowledge is valuable they are at an advantage. There are two points to be made about this work. First, it has been somewhat criticised, notably by Dennis (1956, 1958). Dennis disputes the interpretation Lehman places on the data. He also presents data showing that the productivity of long living eminent scientists (in terms of number of papers published annually) is approximately con-

6) Merton and Zuckerman (1972) argue that "the evidence on age differentials in receptivity to new ideas is thin and uncertain".

stant between 30 and 59. Clearly the two definitions of scientific contribution are quite different. The second point to be made about Lehman's findings is that, in practice, most scientists' work perspectives actually change with age, according the results a certain subjective reality. Personal interest and institutional pressures tend to drive scientists increasingly, with age, into complementary aspects of the scientist's role (e.g. research administration). This may have important implications for the relationship between age and productivity. Knorr et al (1976) found that if scientists were divided into those with and without substantial administrative responsibilities, then the correlation between age and productivity disappeared for the supervisors, but remained strong for the majority of the members of research groups. Zuckerman and Merton have shown (1972) that only the very greatest scientists retain a real commitment to research into ripe old age. They point out: "There is reason to believe that the general pattern of shifts from research to other roles holds more for journeyman scientists than for the more accomplished scientists. Sociological theory leads us to expect and scattered evidence leads us to believe that the more productive scientists, recognised as such by the reward system of science, tend to persist in their research roles". In other words, commitment to research falls off with age, but less rapidly for the most productive/ creative scientists.

There is no doubt that this work is academically most interesting: the question of how subject-differences in age/(productivity-creativity) can be explained leads on to a rather broader issue raised at the end of Section 3. But it also has important implications for research management and research-personnel management, in view of the well-demonstrated relationship (Glaser, 1964; Pelz and Andrews, 1966, etc.) between commitment to research and productivity. Other work discussed in this section may have implications (which investigators have not drawn out) for the selection of research scientists, and potential research scientists, at various stages of their careers. Finally, any relationship between age and achievement will have implications for the research performance of university staff, in a period of 'no growth' (Skoie, 1976).

But it is to the other issues facing research management that we now turn.

3. MICROSOCIOLOGICAL FACTORS

In this section I want to deal with research which focuses principally on the internal organisation and structure of research groups, and the influence of such factors upon research performance. The literature in question is essentially that of research management (minus those elements which deal with project selection, cost and duration estimates, rates of return, etc.) and much of it is aimed at improving the quality of such management.[1] Although much of it has its theoretical basis in social psychology, it is this practice orientation which gives the material its unity and coherence. It is perhaps worth noting, with Ben-David (1973), that this is an area 'abandoned' by the sociology of science which in recent years has shifted its focus from these work groups to consideration of networks of scientists bound rather by common research interests (see Section 4). The managerial orientation of those who have moved into this area of study is partly reflected in the independent variables selected for analysis: emphasis has been on variables subject to manipulation by management.

So far as measures of performance are concerned, much of this work (notably the work of Pelz and his co-workers) has used a range of measures both objective (for example, papers and reports written) and subjective (for example, evaluation by colleagues, superiors). On the whole these measures have been highly correlated: where they are not, I have tried to indicate this in the discussion.

Increasing the extent of freedom and autonomy in research is often recommended as an appropriate incentive to the research-minded (e.g. by Kornhauser, 1962, Chapter 5). To be sure it is to be expected that the scientist whose main interests lie in pure research will be exceptionally anxious for complete autonomy. Should scientists then be allowed to do pretty much what they want - left to their own devices, as it were?

They have been a number of studies of the influence of managerial style: the general conclusion seems to be that scientists should <u>not</u> be simply left alone - but nor should they be subject to strictly authoritarian control. The answer seems to be something in between (called by Baumgartel "participatory leadership").

1) In other words, I have not done justice to economic aspects of research management: one example, I fear, of Procrustean sociologising! But perhaps it is fair to say that here the findings of economists have been less positive than elsewhere, and less so than the findings of other social sciences. In Freeman's words: "the important conclusion emerges that the bureaucratisation of innovation, new management techniques, and the concentration of R&D activities in large firms have not necessarily reduced the uncertainty associated with innovation..." (Freeman, 1977).

Pelz and Andrews (1966) employ a rather similar framework of analysis to Baumgartel and come to similar conclusions. They find (Chapter 2) that scientists and engineers whose research goals are set either by themselves alone or by their superiors alone perform less well than those for whom goals are set as a result of discussions. The more general discussion of goals, the better; the more influence over involved superiors which scientists have, the better ... "the feeling that others are interested in your work, we feel, is an excellent way of sustaining your own interest". Thus, scientists have to be brought into a policy-making process, not simply left alone, in order to work most effectively. Smith (1969) studying the fifteen divisions of the laboratory of a US petroleum company, finds that different patterns of consultation favour different kinds of research effectiveness. Using indices of performance similar to those of Pelz and Andrews, he finds that:

> "The more practical contributions are required, the more consultation and decision making can benefit from formal organisation, provided a viable process of consultation is retained along the hierarchy ... When more science-oriented achievement is the goal, consultation and decision-making of this more traditional form is less strongly associated with high performance. The variations on either form of organisation point to the necessity that expertise be accompanied by actual decision-making power..."

Andrew and Farris (1967) conclude that a supervisor should not 'meddle' in subordinates' work, but should be available, interested and informed. Moreover, they suggest that where the supervisor cannot, because of his own inabilities, provide stimulating guidance, he should leave his team alone: "provision of freedom was a substitute for skillful leadership". Lickert, (1969: 169) describing the replacement of hierarchical by participative management comes to a compatible conclusion: "improvement appears first in the attitudes, communication and motivation, and later in productivity and costs". Related to the means by which research goals are formulated is the 'immediacy' of the goals - the pressure of deadlines. Andrews and Farris (1972) studied the relationships between the "time pressure" under which scientists in a NASA laboratory worked (on activities ranging from applied research through development to technical services) and their performance. High correlations are obtained between the time pressure scientists claimed to experience and their usefulness to the organisation as evaluated by superiors five years later. However, it is not the case that scientists and engineers who are judged more useful subsequently find themselves under marked stress by above average time pressure. (Other measures of performance - e.g. productivity - are less highly correlated with time pressure.) It has to be borne in mind, however, that those who experience high pressure also tend to welcome high pressure of work, and it may thus be the provision of a level of pressure appropriate to each man which produces optimum usefulness.

A parameter of research organisation which has received considerable attention, is communication. Pelz and Andrews (1966, Chapter 3) demonstrate an association between communication and the performance of individuals. Their major independent variable is extent of communication (the frequency with which a scientist communicates with his colleagues: whether by conversation, memos, in seminars, or by other means). The average frequency of communication of a man with his five self-chosen "most important" colleagues (excluding those in supervisory positions over him) is correlated with performance. A second aspect of the analysis focuses upon the total number of other scientists with whom each man exchanged information, distinguishing between those within and those outside his own work-group. Significant relationships between the performance of an individual and the number of colleagues in his group, and outside the group but within the organisation, with whom he exchanged information, are obtained. Pelz and Andrews acknowledge the possibility that the extent to which a man communicates with his colleagues is a consequence, rather than a cause, of effective performance, but this seems not to be so. Farris (1972) carried out a study in a NASA laboratory (in which work ranged from basic physics and chemistry to rocket and satellite based experiments) designed to examine the relationships between research group effectiveness and the nature of the communication. Groups are divided into more or less innovative ones on the basis of the mean scores of members, these scores being based upon the judgements of supervisors. The more innovative groups tend to function much more as teams, the group supervisor being very much a part of the team. At the stage of formulating initial ideas upon which to work, "the roles most associated with group innovation are the supervisor's receiving original ideas from more outside sources but having fewer original ideas himself, group members providing each other with technical information...". In subsequently working the idea up into a concrete proposal "the high innovation groups are characterised by greater exchange of help among themselves in thinking through technical problems and greater usefulness of their supervisors in critically evaluating their ideas".

A major contribution to the understanding of communication networks in research organisations, and of their significance, has been made by Allen and associates. Factors which have particularly occupied this group are the ways in which information enters R&D organisations and the channels of communication through which it then flows. Like Pelz and Andrews, and others, Allen finds that high performers in organisations communicate with colleagues more frequently and more widely, both within and without their own specialities (Allen, 1970). On the whole, however, effective performers communicate more with colleagues from within their own organisation: internal consultation seems to be more useful than external communication for the majority (Allen, 1970). Why should this be? One reason suggested (Allen and Cohen, 1969) is based upon Katz and Kahn's notion of a "shared coding scheme" (Katz and Kahn, 1966). Academic scientists are most significantly oriented towards their disciplinary community or invisible college: their perceptual schemes are widely shared with scientists outside their own institutions. The situation may be rather different for scientists in government or industrial laboratories: "mutual experience and schemes of ordering the world that are bureaucratically imposed are characteristic of the organisation and can be quite different from the schemes of members of their particular discipline in other organisations". So it may be difficult to impart information in assimilable form: there is a problem of translation. How then is information imported; for there is no doubt that any R&D laboratory must keep "in touch" with outside developments if it is to be effective (Carter and Williams, 1957)? Based upon theories developed in mass communications research (see for example, Katz and Lazarsfeld, 1955), Allen introduces the notion of the "technological gatekeeper". Sociometric studies, in which scientists are asked to name those colleagues with whom they preferred to discuss technical matters, show that a few key individuals play highly significant consultant roles in the laboratories (Allen and Cohen, 1969). These key individuals ("sociometric stars") prove to maintain many more continuing professional relationships outside the organisation and, to read much more widely in the technical and scientific literature. These few individuals ("gatekeepers") play a crucial role in keeping the establishment in touch with developments outside. They import information upon which their colleagues may and do draw, and thereby render a vital service to the laboratory. In organisations containing various specialist disciplinary divisions the gatekeepers in each division are likely to maintain close communications among themselves (Allen, 1970). Although gatekeepers cannot, apparently, be identified in any a priori way, they are noted as being on average more highly educated, more productive and more likely to be first in line supervisors than non-gatekeepers.

To return now to the other aspect of sociometric communication studies: the pattern of technical communication within the laboratory and the factors which affect it. Given Pelz and Andrews' general conclusion that effectiveness is improved by increasing the extent of internal consultation, these factors acquire some importance. Two kinds are distinguished: organisational structure and physical or geographic location. Organisational structure may be regarded as made up of two sub-structures: the formal structure (that shown on organisational charts) and the informal personal relationships between the people making up the organisation. There is some overlap between the informal social structure and the technical discussion network in Allen's American laboratories; that is, there is a slight tendency for scientists to discuss technical problems with those colleagues with whom they socialise. But formal structure is very much more important, technical discussion being largely determined by work group structure. Within work groups status becomes relevant. Allen finds that whereas the Ph.Ds in his laboratories communicate quite freely among themselves, they rarely either socialise or discuss technical problems with non-Ph.Ds. The latter discuss technical matters among themselves much less frequently, and tend to direct both their socialisation and their technical discussion towards the higher status group. Whitley and Frost's results in an English laboratory seem to be in accord with these findings. Physical location also appears to be an important determinant of patterns of communication within an organisation. Examining the effect of distance upon the interaction of pairs of technical and scientific people within organisations, Allen (1970) finds that the probability that a given pair of individuals will be in communication is inversely proportional to the square of the distance separating them. Any additional barriers (corners, stairs, etc.) considerably reduce this probability.

A further variable introduced by Pelz and Andrews (1966, Chapter 4) is termed by them "diversity of activities". Diversity covers both the number of areas in which a man had specialist knowledge, and the range of functions in which his work involves him. Their study indicates that advantages accrue to diversification of all kinds. The more specialisms claimed by an individual, the better his performance is likely to be (this is particularly true among younger scientists, among whom breadth of expertise is an especially valuable quality). Secondly, those who work about three-quarters of the time on R&D are the most effective: more so than full-time researchers. Some outside duties seem useful. Thirdly, a range of functions within R&D seem beneficial. Instead of spending all his time on basic research, or product improvement, or development, a scientist (whether in a primarily research, or primarily development laboratory) should be encouraged to work on others of these R&D functions. Measures such as these can inhibit staleness.

Should a scientist work with colleagues who are similar to himself (for example in previous experience, career-objectives and orientation, approaches to problems, motivation, etc.)? Pelz and Andrews (1966, Chapter 8) examine the performance of respondents as a function of their 'similarity' to the five colleagues selected by them as being of most importance to their work. Various indices of similarity are defined: past experience; perceived similarity in terms of technical strategies; similarity in style of approach (as assessed by the investigators); similarity in orientation (i.e. whether principally to science or principally to organisational advancement); similarity in sources of motivation (whether they draw inspiration principally from their supervisors, colleagues, clients with practical problems, themselves, the scientific literature, etc.). Similarity in terms of past experience seems irrelevant to performance. Results suggest that a balance between similarity and dissimilarity is best of all: similarity in motivation, combined with dissimilarity in orientation and technical strategies adopted in problem solving seeming especially advantageous. But the individual's optimum environment - the balance between similarity and dissimilarity - seems to depend upon the kind of function he fulfills. Scientists and engineers who are used principally as sources of innovative ideas, rather than in more routine ways, seem to profit particularly from some sort of environmental dissonance. But complete dissimilarity on all parameters seems difficult to bear, and even for the innovators similarity in motivations provides a necessary source of what Pelz and Andrews call "emotional support". A somewhat related problem is that of "functional" or "project" organisation. Should research groups be organised on the basis of the specialisms of personnel (physics, electronic engineering, etc.) - "functional organisation" - or on the basis of the project on which each is working?[2)] Shepard (1957) argues that status considerations act against a project orientation: since project groups are likely to require frequent re-arrangement, hierarchies or chains of command become unclear. Individuals find their uncertain status unpleasant. Although he does not imply any direct relationship between "uncertainty" and "creativity" Shepard agrees that project groups will be more creative. Allen, approaching this same problem from a communications point of view, points out that whilst effective internal coordination may favour project organisation, this may cause the individual team members to lose that contact with developments in their field which a functional arrangement may allow (Allen, 1970). Allen suggests that project-organisation is to be preferred for projects of short duration, specialist (or functional) organisation for long-term projects. Shepard's status considerations suggest otherwise.

Pelz and Andrews invoke the notion that work environments must offer a proper balance between "uncertainty" and "stability" in discussing the relevance of group age (Pelz and Andrews, 1966, Chapter 13). They hypothesise that in situations of high <u>emotional</u> stability, a good deal of <u>intellectual</u> uncertainty is valuable, but that when emotional stability is lacking, toleration of intellectual uncertainty is reduced. A new research group presents a situation of tension, of uncertainty, to its members. As the group ages (age being defined as the mean length of time spent in the group by members), relationships between individuals change, and the performance of the group as a whole is affected. The subjectively-evaluated measures of scientific/technical performance and overall usefulness to the organisation were obtained for 83 groups in industry and government, and expressed as a function of group age. Results indicate that scientific contribution is at its highest at the time the group is first constituted, usefulness after it has been in existence for four to five years. An attempt is made to explain their

2) For a survey of the extent to which each method of organisation was used in a range of US industries in 1964, see Seiler (1965, Chapter 3).

results in terms of the aging process: groups become less competitive, less secretive, more relaxed - and at the same time over-specialised - with the passage of time. They profit from "collective wisdom" rather than from "intellectual tension". Thus, whereas young groups need a calming atmosphere of security and co-operation, to offset their natural tensions, in older groups tension (for example through diversity among members) has to be fostered in order to prolong their productive lives. It is noteworthy that those older groups which do continue to perform at a high level are those in which vigorous interaction among members is maintained, and which exist in an atmosphere of inter-group competitiveness. Though these results are highly plausible on a priori grounds, subsequent work by Smith (1970) seems to cast doubt on their general validity. He examines the performance of 52 groups in a single large petroleum engineering laboratory. Performance is defined both in terms of the two subjective measures used by Pelz and Andrews, as well as objectively in terms of papers published and patents. Smith's plots of (the four) performance measures against group age show that in this instance (a) both subjective measures reach maxima at three to four years of group age; (b) both objective measures show slow and continuing improvements with age. Whilst, therefore, it seems likely that certain aging processes do take place in research groups it is not possible unambiguously to describe their effects upon performance; nor indeed is it yet possible to describe the processes themselves in any detail.

A final parameter which should be included here is research group size. Blume and Sinclair studied the relationship between the productivity (defined in various ways) of chemists in British universities and the size of the groups (defined to include both academic colleagues and research students) in which they worked (Blume and Sinclair, 1973a). Briefly stated, the principle conclusion of that study is that a significant, though weak, relationship obtains and that no threshold (or minimum effective size of research group) exists. Subsequently, the effects of three potentially intervening variables are discussed. Two of these are academic rank and the composition of the research group. It transpires that Readers (who typically devote more time to research than any other category of established academic staff)[3] profit more from large research groups than do their colleagues. The authors conclude, on this point, "A large group can be a great aid to the committed scientist, who is himself deeply involved in his research; it cannot substitute for that involvement." In considering the composition of research groups, the attempt is made to break down the notion of 'size' in some degree. Research students predominate in (academic) chemistry of research groups, being about twice as numerous as academic staff and

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3) Or, as Halsey and Trow have it, Readers are more committed to research than are others.

four times as numerous as full-time research fellows. In looking at 'composition' the question posed is this: "to what extent can students (being trained in research) compensate for more experienced scientists in ensuring the benefits of size?" The answer is 'only partly': at a given level of group size, the presence of one or more paid fellows or faculty colleagues is a distinct advantage. Thus it is implied that the various categories of research personnel (excluding technicians, who are not considered) are not strictly substitutable: however, there is (and has been) no analysis of the roles which each category typically acts out (i.e. of the division of labour). The third intervening variable considered by Blume and Sinclair is defined as the 'sub-discipline' of research. It appears that the 'sub-disciplines' of chemistry (physical, organic, theoretical, etc.) differ significantly in the strengths of the relationship between research group size and the measures of performance. In other words, a large research group is a much greater advantage to the inorganic chemist than to the physical chemist.

Not incompatible results were obtained by Knorr and her co-workers in Austria (Knorr et al, 1976). Having found, unsurprisingly, that the total output from research groups correlated with their size, they went on to examine average productivity per capita (i.e. divided by the number of research group members). When various corrections for other determinant variables were made to output scores, these authors found negative relationships for both academic natural science and technological research groups (though apparently stronger in the former case). The authors comment: "scientific field and the associated technology requirements could play a key role in determining optimal group size as suggested by the far less pronounced negative effect of size in technological science research units".

Knorr's work (done within the context of a multinational study co-ordinated by Unesco) does provide confirmation of Blume and Sinclair's finding that the significance for research productivity of group size differs from one field of research to another. Blume and Sinclair tried to explain their results as follows:

> "These differences can perhaps be rationalised in terms of the variations in typical modus operandi between the fields of chemistry. For example, it is possible that physical-inorganic (and physical organic) chemists tend to be people who use available apparatus to make measurements on available compounds: given adequate assistance they will normally be able to make the measurements they require and publish the results. In contrast the physical chemist may spend some years attempting to develop a better technique for a specific problem: only if he succeeds will publication follow ..."

Thus what is then called 'modus operandi', and which might be taken as encompassing typical research objectives and tasks, as well as typical ways of working, is clearly an important intervening variable in the relationship between 'productivity' and many social/organisational variables. Unfortunately, there have been no attempts to characterise and contrast scientific disciplines in these terms, and nor have they been much used as a parameter in empirical studies. The distinction between 'applied' and 'basic' research (which when made has been pointed out in earlier parts of this section of the review) is but an over-crude approximation. Certainly, some such distinction is necessary if Blume and Sinclair's finding that there is no "threshold" in academic research is to be reconciled with Freeman's (1974) demonstration that in some areas of (applied) R&D a firm needs to expend above some threshold of expenditure to keep abreast and be able to utilise discoveries which may be available.

The discussion of the relationship between 'size' or 'scale of activity' and performance usefully demonstrates that there must be some variable intervening between social and organisational variables on the one hand, and performance on the other. It is also apparent that the simple distinction between 'basic' and 'applied' research will not do. This follows from, on the one hand, the problem raised by Blume and Sinclair's study of academic research, and on the other from work in the economics of innovation. As Freeman points out, whereas some types of R&D can only be carried out with very large resources (e.g. on reactor development), in other areas advances may be (more frequently?) made with limited resources or even by individual inventors. This has been particularly true of the scientific instrument industry, where small firms, often created by a single 'mobile' scientist, have been exceptionally successful (Shimshoni, 1970). What then is this apparently crucial variable? It seems clear to me that none of the commonly used distinctions (for example, between disciplines, applied/pure research) will do. Nor do we know if the distiction within academic research is similar to or different from that shown to obtain between technologies.

It also seems clear that a similar situation must obtain with respect to the organisational variables discussed earlier in this section. In some instances authors showed that conclusions did depend upon some notion of this kind (e.g. Smith's (1969) distinction between "practical contributions" and "science oriented achievement"), but this intervening factor requires much more refinement. Though this research management literature is both comprehensive and coherent (in a way that much other material considered in this review is not) it is hard to escape the conclusion that it <u>must</u> theoretically suffer from lack of explication of the variable(s) intervening between organisation and performance. The attempt to explicate these variables, which must depend both upon theoretical and empirical analysis, and upon the contributions of a number of disciplines, seems to me urgent on grounds both of theory and usefulness. To reiterate, the factor(s) which I have in mind must be made up in some way of (for example) goals (theory, data, hardware ...): 'microtasks' (the kinds of things individuals do, hour by hour, day by day); the technology of what goes on (instrumentation being one aspect of this); the time scale of the activity; and so on.

Recently Whitley (1977) has attempted a theoretical categorization of sciences more or less on those lines. By developing a distinction between 'restricted' and 'unrestricted' sciences, he tries "to show how particular sciences are associated with particular ways of organising scientific work, training recruits" etc. The distinction is fundamentally based in comparison of the kinds of 'objects' (or phenomena) with which sciences work.

> "In some sciences, events and phenomena are embedded in a highly esoteric theoretical structure which requires elaborate technical facilities for their production... the understanding of these objects is impossible without the use of particular techniques..."

By contrast, in 'unrestricted' sciences the objects of investigation are less narrowly conceived and less theory-specific. Whitley then tries to show that implications for work organization follow.

> "High specificity of objects, techniques, and purposes implies a considerable degree of clarity about task formulation and interrelationships. Separation of tasks and the differentiation of research topics and approaches are also facilitated... a developed division of labour between tasks... could occur."

Further elucidation of a theoretical schema such as that proposed by Whitley, and its testing, must be the only way by which we will come to understand the differential impact of organizational factors in different fields of science.

4. RELATIONS WITH THE SCIENTIFIC COMMUNITY

In discussing the relationships between a given individual or research group and the scientific community there are two rather different kinds of issues which must be raised. The first is concerned with the general structure of the research area, speciality, or discipline with which the individual identifies, and towards which he largely directs his communications. Its cohesion, size, organisation etc. will to a considerable degree determine the kinds of relationships which he <u>can</u> have with his professional peer group, as well as his perception of the core membership of that group. The kinds of inter-personal relationships characteristic of a research area or specialism can be shown to vary with such parameters. The second kind of issue relates to what might be called the 'sociometric centrality' of the individual researcher or group: the extent to which he, she, or it is 'plugged in' to whatever communication/collaboration pattern exists within the field in question. The first of these is actually central to the sociology of science, which in its 'interactional' variant has abandoned an earlier concern with communication patterns within institutional work groups in favour of those obtained in research areas or disciplines. To deal thoroughly with the question posed requires a review of a major part of the recent literature in the sociology of science. This literature is much more theoretically coherent (in spite of something approaching a paradigm shift in recent years) than any other body of material relevant to this report: which in a sense facilitates our task. On the other hand, some recent work is not concerned with the correlates of research productivity, since that concept has itself become problematic. The second of our two issues is less abstract and less complex, and has not been the subject of much academic inquiry. As I understand it, it has two dimensions, which have generally been discussed in terms of the effects of impaired communication. How does working in an institution which restricts the extramural communication of its scientists (e.g. for security reasons) influence contact with the international scientific community? The two issues are linked by studies within the first tradition showing a correlation between productivity and the extent of an individual's communication interactions with peers.

4.1. <u>Structure and Interaction in Science</u>

My introduction of the material discussed below is necessitated by the sociological finding that interactions between scientists are patterned by and characteristic of the structure and stage of development of the field of research in question. In other words the kinds of relationships which a research group <u>could</u> have with other groups (at best) are dependent upon the parameters characterising the area in which it works. As I mentioned earlier, such dependencies are the subject matter of a very large proportion of the sociology of science. I propose to discuss this quite substantial literature only by reference to the work of one or two sociologists. It may well appear that this sociological analysis has little import for questions of science policy. Whilst the question of its wider relevance has indeed been of little interest to the authors concerned (a matter which is taken up at the end of the paper), my own view is that it does have implications (which need to be elucidated) and that it may ultimately provide a theoretical basis for crucial policy issues.

One of the most comprehensive sociological accounts of the structure of scientific interactions is Diana Crane's, in her Invisible Colleges. Before summarising the most salient of her conclusions, it is necessary to make explicit two inherent assumptions in the work. First, she is concerned with interactions characteristic essentially of developed, and not emerging, fields of research: that is, not with the process of scientific change. Second, and related, she adopts an essentially incremental, non-cognitive, model of scientific growth. What must ultimately be explained, for her, is a series of exponential growth curves (authors, papers published, etc.) rather than an evolution in concepts, problem-definitions, etc.

The first critical point made is a demonstration of the importance of informal contact (interaction) between scientists for scientific growth. She compares the growth rates in fields empirically examined in her study (rural sociology and the mathematics of finite groups) in which pronounced interaction had been found to occur, and other fields (mathematical invariant theory and reading research) in which other authors had found "the level of interpersonal communication and influence was low" (pp. 24-25). The comparison supports the hypothesis that "science grows as a result of the diffusion of ideas that are transmitted in part by means of personal influence". Scientists themselves acknowledged the importance of these personal links to their work.

Subsequently the structure of interactions are considered in detail. Whilst there are a number of kinds of interaction ("informal discussions of research, published collaborations, relationship with teachers, and the influence of colleagues upon the selection of research problems and techniques") she finds that comparable pictures of the strengths and directions of structural links result (p. 42). By asking each scientist working in her two chosen areas to specify his/her contacts, a sociometric picture is built up. When all the four kinds of interaction were considered together, some three-quarters of all researchers in each area were linked in such a network.[1] Within this, it appears that there are small numbers of more tightly knit groups of scientists who

1) It was also found that people isolated from these networks were perceived as doing low quality work.

collaborate with one another (i.e. internally), but which are related to one another by other communication flows. The most productive scientists in each group (who intended to have the widest contacts) are generally in touch - hence maintaining the coherence of the network. Introducing comparable results from other studies, Crane concludes:

> "These findings from various studies indicate clearly the presence of an invisible college or network of productive scientists linking separate groups of collaborators within a research area. There is some tentative evidence that the absence of an effective invisible college ... can inhibit the development of a field." (p. 54)

Of course, communication is not limited by the boundaries of a research area: most scientists have extensive contacts outside the field in which they primarily work. However, these ties are weaker and more diffuse. This study does not really deal with the way in which research areas are aggregated into disciplines, or with the variations in kind or importance between extra-disciplinary, disciplinary, and intra-research area contacts.[2)]

Finally, Crane uses her information on growth of fields, and on the nature and role of informal communication, to support the view that growth is explicable in terms of a diffusion model (as developed in other areas of sociology to explain the spread of innovations). But this is of less relevance for our present purposes.

It is probably true that on the whole sociologists of science are today more interested in explaining the emergence of new fields of science than their subsequent (and quite possibly exponential) growth.[3)] Mullins'

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2) Whitley has discussed this, though not with reference to specific empirical findings. He argues "In well established and developed fields where research is highly differentiated disciplines are not likely to be an important focus of identity ... The main foci of commitment and competition between scientists and components of scientific activity in organised, established science, are likely to be specialities..." (R.D. Whitley, 1974b).

3) In his (1965) volume The Scientific Community Hagstrom discussed this in terms of the responses of scientific institutions to what is essentially the differentiation of science. This perspective is of use when we consider relationships between new fields and, for example, research funding bodies (Section 6). The approach is rather different here.

(1972) study of the origins of the 'informational' approach to molecular biology is a good case in point (and one which has certainly been influential) though there are others. Mullins was particularly concerned to show the way in which different patterns of interaction characterise different stages of institutionalisation of the research fields. Taking account of current concern with changing cognitive structure, he tries also to specify certain of the intellectual features of the field at each stage. In the intellectual sense, the field is said to progress from "paradigm development" in its early stages, to "puzzle solving" in its ultimate and established ones: an evolution characterised by some loss of excitement and "a sense of sadness and nostalgia" on the part of the earliest participants. In structural terms, the evolution is characterised as follows. First, a "paradigm group ... the minimal form of a scientific group. Its members have no necessary social connections ... [it] is thus a set of individuals, all of whom have moved into a similar cognitive situation with respect to the same, or similar problems" (p. 55). In the phage (informational) field, this was characterised (1935-45) by initial interest on the part of a few scientists, mostly unaware of one another's work, and scarcely in contact. There follows the "communication network" ... a set of pairs and triads of scientists engaged in regular communication, or colleagueship, over a period of time. The patterns of a network are in constant flux and they can change without much perceptible effect on the science itself. ... We should note that the communication network structure shows two changes from the paradigm group structure: (i) increased connection among scientists who are working in the area, and (ii) a corresponding decrease in disconnected or independent persons" (pp. 58-59). Meetings are now held (1945-53), and a number of the phage scientists are now in a position to recruit postgraduate students to the field. ("Recruitment was crucial to the growth of the network" and most recruits were graduate or postdoctoral students.) 1954-62 was the "cluster" period.

> "A cluster forms when scientists become selfconcious about their patterns of communication and begin to set boundaries around those who are working on their common problem. It develops from recombinations of pairs and triads in response to certain favourable conditions, e.g. luck, leadership ... These clusters ... are more stable than the pairs and triads which constitute them, have a distinct culture and are able to draw support and students." (p. 69).

The whole thing is still informal, and growth is a net effect made up of recruitment and emigration. Finally, after 1962, phage work reached the "speciality" stage, at which there exist "regular processes for training and recruitment into roles which are institutionally defined as belonging to that speciality" (p. 74). We have now reached the "puzzle solving" phase, at which a consensus exists over acceptable solutions to problems, ways of obtaining solutions, and so on. Socially, thanks to effective leadership, institutional support, money and so on, molecular biology has become established: "new departments of molecular biology and transformed departments of biology increasingly replaced the earlier "research institute" pattern. The processes of apprenticeship are more formal because rules and programmes exist" (p. 77). Growth is now slower (unless those now graduating in the field are included).

This careful study shows clearly that (1) at different stages of development, new sciences are characterised by different sorts of structures, different patterns of communication, collaboration, apprenticeship, etc. It suggests also (2) that these "different sorts of structures" are associated with different sorts of intellectual activity. I suspect - and if true this says something about sociology of science today - that (1) would be taken as a generalisable hypothesis, but (2) as no more than a case study. In other words, the relationship between social structures and intellectual structures is generally taken (with reason) as one of the most complex issues for the sociology of science. (See, for example, R.D. Whitley, 1974a).

Sociologists have long recognised that contact between scientists, awareness of one another's work, does not necessarily imply exchange of results or collaboration. Competition is also endemic in science. Initially this was conceived of primarily as competition between individual scientists for priority in obtaining and publishing results. Thanks notably to a study by Warren Hagstrom, we know that the incidence and results of such competition are complex. He concludes as follows (Hagstrom, 1967, pp. 18-19):

> "The desire for recognition induces scientists to work on problems their colleagues find important - it induces them to compete - but the fact of competition also leads some scientists to search for new problems their colleagues will find important and new ways of approaching old problems.
>
> Competition for priority leads to the experience of being anticipated [in publication, and concern about future anticipation]. This concern motivates them to work hard and fast, but it also leads some of them to withhold information from their colleagues until they are ready to publish...
>
> There are good reasons to believe that allowing scientists freedom to select their own research problems, influenced as they are by a desire to make discoveries their colleagues will find impor-

> tant, is an effective way to allocate human effort in basic research. Giving scientists freedom to compete has consequences that limit the effectiveness of science, but its major consequences are to facilitate discovery and the dissemination of discovery."

Hagstrom also found that the incidence of competition varies between fields of science, when quantified in terms of previous anticipation, current concern about anticipation, the inhibition of free communication, etc.[4] Two kinds of factors are adduced in explanation of these differences: agreement among scientists on the relative importance of scientific problems (in a field), and the numbers who "possess the skills and equipment to solve the problems". More recently, competition has been discussed in a rather different sense: as competition between ways of conceiving a problem, over techniques and approaches (Whitley, 1974c).

I should like to turn to the relevance of this work for science policy: that is, for the allocation of resources to science and the formalised structuring of scientific organisation.[5] There are three obvious points of contact. The first is Hagstrom's contention that scientific competition implies that 'freedom of choice' guarantees an optimum allocation effort between research problems. The relevance of this view for scientific policy is apparent, though its correctness (which cannot be taken for granted) has not been examined. The second relates to the attempts made (notably in the USA) to modify the formal communication systems of science, and the third to the development of policies towards areas of research at different stages of development.

Broadly speaking, the formal communication system in science, which complements the transfer of information by informal (interpersonal) interaction, may be said to have a number of functions (on this, see Ziman, 1968). It represents the dissemination of evaluated and legitimated knowledge: by virtue of the refereeing procedures of journals (see for example, Zuckerman and Merton, 1971) published reports are formally acknowledged as additions to the body of knowledge in a way which informally transmitted material is not. Secondly, since it gives formal credence to priority claims on the part of authors, it has a sociological

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4) Blume and Sinclair found that the prevalence of previous anticipation among British chemists was identical with Hagstrom's figure for chemists. However, British chemists were slightly more likely to (react by) restricting exchange of information on their work.

5) As noted earlier, the responsiveness of funding systems to scientific development (differentiation) is taken up in Section 6.

as well as purely informational aspect. Thirdly, scientists use the published literature to 'browse' around their subject. It may be surmised that potentially useful information is often gathered in a purely accidental way as a result of this browsing: hence many scientists devote a good deal of time to skimming a wide range of periodicals. The broader the field over which a scientist wishes to keep informed, the greater must be his reliance on this formal system.

It is in the light of this complex of functions that attempts to modify this communication system must be judged. One kind of modification which has been suggested may be mentioned: the very rapid circulation of unrefereed material, proposed in order to avoid the delays inherent in journal publication. Such an experiment was introduced by the National Institutes of Health in 1961, with information exchange schemes operating in a number of fields. Anyone working in a field could participate, and all material submitted by members was duplicated and rapidly circulated. The scheme was discontinued in 1966 at the request of the scientists. A rather similar proposal by the American Psychological Association was very contentious. The point is, as Crane notes, that speed is a less important aspect of publication than is legitimation, or quality control (Crane, 1973, p. 122). The information specialists may have failed to recognise this latent function. Any policy towards scientific information must clearly be based upon a real understanding of the range of functions which the formal and informal communication systems must each fill.

Thirdly, I want to raise the question of policies appropriate to science (or research areas) at different stages of development. On the whole sociology of science has denied or neglected any external influences upon scientific development: government policy being one such external factor. Certainly Mullin's work gives an indication of the organisational and resource requirements of a field in the earliest stages of its development, which cannot but be useful to those seeking to propagate such a field of research. But it does not deal with the issue fundamental to science policy, most especially in these days of cries for 'relevance'. Like most research in the sociology of science (whether functionalist or more recently 'cognitive') it does not consider the possible influence of external factors (including by implication policy) upon the development of science. Such concern has of course informed radical critiques of the perversion of science by dominant interest groups, but has not - until very recently - influenced the systematic sociology of science. Kuhn in his Structure of Scientific Revolutions may have implied that external notions can be crucial to science in crisis - at the breakdown of a paradigm - but given the rareness and transience of such revolutionary episodes as he conceives them, this is of little comfort to the policy-maker!

Recent theoretical work in Germany (notably at the Max Planck Institute in Starnberg) suggests that under certain (specifiable) circumstances research can be made to conform to external needs and objectives (Böhme, van den Daele and Krohn, 1973). That is, instead of concerning himself with the attempt to apply the prior results of science, the policy-maker may hope to influence the theoretical development of the science itself. The concept crucial to this work is that of "Finalisation" ("Finalisierung"). According to the hypothesis (which is far from proven) when a research area develops a "mature" theory (a complete theory which seems not in need of further development), it becomes open to theoretical direction from outside. This late stage has to be distinguished from the very early (pre-paradigmatic) phase of a science, in which the focus may indeed be upon practical problems, but where the theoretical foundations for their solution are lacking. It seems to be that this work - when the conditions of finalisation can be wholly specified - could have a fundamental contribution to make to science policy. Of course, the validity of the concept, which is rather controversial, requires further research.

4.2. Isolation, Integration and Productivity

I hope that the preceding section has demonstrated a symbiotic relationship between informal communication patterns and the development of the science in question (whether 'development' is conceived quantitatively or intellectually). These variables 'set the scene' for the individual participant, whose involvement may range from complete isolation to centrality in the communication network.

There is evidence that at the individual level extent of communication and research effectiveness are related. Allen's work on the relations between effectiveness and communication patterns within research laboratories has already been discussed (Section 3). At the level of the disciplinary community there is evidence from both Crane and Gaston's work that highly productive scientists communicate more (and above all, with one another) than do the less productive. Whilst this does not necessarily imply any causality in itself, there is evidence that the work of scientists isolated from the communication network will be somewhat undervalued, as Crane points out.

One barrier to communication and an important one for science policy is national location, and language. There are two kinds of evidence for the importance of these factors. First, there is evidence that scientists in major countries tend to be less aware of work published in foreign languages unless because of the acknowledged scale and importance of this work it is regularly translated (e.g. Russian work translated into English). Indeed, in a pilot study of the Canadian scientific community, I found some evidence of inhibited communication at the internal

Canadian 'linguistic interface'. Secondly, we have the evidence of scientists in countries generally cut off from international communication, and in which scale of research is inadequate for the development of a satisfying internal communication network. An observer of the scene has written of the predicament of the scientists in developing countries:

> "Scientists are relatively few in number, and they are often, as far as any particular field of research is concerned, dispersed over long distances. They suffer from isolation from each other ... They are in danger, a danger to which they too often succumb, of losing contacts with their colleagues in the international scientific community. They feel peripheral and out of touch with the important developments in science unless they can visit and be visited by important scientists from the more developed countries: they feel inferior and neglected because their own journals and publications, when they exist at all, are seldom read by foreign scientists, seldom quoted in the literature and are indeed often neglected by their own colleagues at home...
>
> They are in brief not fully fledged members of the scientific community and their work suffers accordingly. Neither its scientific nor its practical value is what it could be ... (S. Dedijer, 1963).

These reasons, among others, have led to the development of certain international scientific centres: the theoretical physics centre at Trieste being directed specifically at scientists from developing countries. There is little doubt that the summer schools held in such centres, and the prolonged but temporary visits abroad which they make possible, are of very great value to scientists generally somewhat isolated. They may also have other kinds of advantages. There are a number of studies demonstrating the importance of summer schools for the development of specific research areas (Mullins' work on the phage group being a case in point).

Given the demonstrable importance of personal contact for the development of research, and the fact that many scientists (even in advanced countries) are rather isolated, the extent to which such contacts need actively to be fostered seems worth considering. Although costly and glamorous international programmes and facilities provide a much greater focus of interest for science policy research (for example, Gibbon's work on CERN), the promotion of individual contact via exchange programmes is likely to be an increasingly important element of international scientific relations (CSP, 1970). Moreover, this approach to the internationalisation of science (not limited to very costly areas of research) is probably less complicated by political and economic considerations and expectations. Some assessment of its costs and its benefits (perhaps making comparisons with other approaches) would seem both worthwhile and feasible.

5. RELATIONS BETWEEN RESEARCH PERFORMERS AND USERS

In 1968 Joseph Ben-David wrote that:

> "there is no direct relationship between specific kinds of fundamental research and the eventual application of the findings in practice ... success in exploiting science for practical purposes does not, therefore, result from the guidance of fundamental research by practical considerations but from constant entrepreneurial activity aimed at bringing to the attention of potential users whatever may be relevant for them in science and vice versa."
> (Ben-David, 1968, p. 56)

In other words, it is the nature of the relationships between 'doers' and 'users' which determined the likelihood of research being utilised: there is nothing inherently 'utilisable' in a specific result. Whilst there may be disagreements as to whether or not some areas of research are more potentially useful than others, few would now disagree that these social relationships are of importance. In discussing conditions favouring the 'take up' of research then, the focus of attention has thus been upon the characteristics of optimum relations. Some, such as Lord Rothschild, have argued that one essential characteristic is that the user (or customer) pays for the research to be done. Others have argued that the customer may not always be competent to foresee (in advance) his needs, or to determine the kind of research which may enable them to be met. In other instances such a financial relationship may not be feasible. Moreover, there are clearly other conditions to be met.

In this section of the report, we shall be concerned with the kinds of relationships between performer and (potential) user which seem to stimulate the effective utilisation of research. Three issues will be raised:

- How can we categorise the customers for research?
- What do we know about their relations with research performers?
- How do we evaluate these relations?

One kind of 'customer' is excluded from the discussion. We shall not here be concerned with the scientific community itself as a user of research (in the furtherance of consensual knowledge), since that was the subject of the previous section.

Most of the discussion will focus upon two kinds of research performers: universities and government (including Research Council) laboratories, although I shall have something to say about the nonprofit, or independent sector. In the UK this is relatively small.

5.1. Who Are The (Potential) Users of Research?

Here we are concerned with a number of problems of definition and specification. So far as university research is concerned, we may say that there are three classes of (potential) users. First, there is the university itself. By this I mean both the direct embodiment of the results of research in curriculum change, and the more diffuse (but probable) effects of research activity on the ethos and general innovativeness of the institution. This issue is not discussed in the 'structural' terms which are appropriate, for example, to the analysis of university-industry relations (se below). That research and teaching are generally closely integrated is apparent: though fissiparous tendencies are demonstrated by the growth of 'institute' rather than 'departmental' research and the relative expansion of higher education in non-university institutions. But the important question can be put in the form: does close proximity to research actually and necessarily benefit teaching? (It can also be put in the form: how can the educational pay-off from research be maximised? I tried to deal with this form of the problem in Blume, 1974b.) One aspect of this is: are good researchers also good teachers? The research evidence on this is extremely uncertain. (See for example, Harry and Goldner, 1972; Astin, 1968; Hayes, 1971.) It seems possible that the question has been phrased in far too simplistic a fashion: it is simply more complicated than researchers have recognised. A second aspect of the problem is the effect of research performance or non-performance of research upon the 'ethos' of an academic institutions and indeed of the implications of 'ethos'. Here too we know very little, although Blau (1973) and Halsey and Trow (1973) have discussed the relations between research performance and stratification in American and British systems respectively.

Second comes the great range of institutions (firms, public bodies, etc.) having the capacity potentially to make use of at least something of the results of academic (which is not identical with 'fundamental') research in the short term. In some cases a financial relationship may bind academic researchers and users, in other cases not. Finally there is the almost unspecifiable category of those who might ultimately benefit from the long-term results of fundamental research: nothing and no one is clearly excluded from this category. Because most university research is fundamental (or at least is defined to be so) because most of it is freely published, and because its application is not generally predictable, this category of 'potential user' is of little analytical use. We shall therefore limit ourselves to considering the second as the 'user' of academic research. The situation with regard to government research is rather different. In the first place the major part of it can be classified as applied (i.e. there exists one or more potential user for much of this work.). The major conceptual problem in specifying who exactly are the customers - or at least the most important customers - is essentially a problem of political philosophy. The issue was brought very much to the surface - though not in precisely these terms - by the post-Rothschild debate in the UK. It boils down to this: to what extent do, can and should government departments act on behalf of their 'client groups' in the commissioning of, and access to, research? This becomes more than a philosophic debate when it is necessary to decide <u>which</u> relationships need to be strengthened, perhaps at the expense of the other, in promoting the utilisation of research - i.e. A1-A2 or B. (See diagram below.) In the debate over the implementation of Lord Rothschild's Report both cases were argued.

Research Institutes $\xrightarrow{\text{A1}}$ Government Departments $\xrightarrow{\text{A2}}$ Clients:

(Government, Research Councils)

professions (farmers, doctors, teachers) etc.

industries, social services, (local authorities) etc.

B

It is of course perfectly possible that Departments may interpret the needs of 'clients' differently from the latter's own interpretation. Moreover, some government scientists have argued that a correlate of recent attempts to strengthen their links with Departments has been a specific requirement that they reduce their contacts with the private sector. Although there are undoubtedly considerations of political values here, there is also a question of sheer efficiency.[1)] Upon this it seems to me, empirical enquiry could shed some light, particularly with regard to certain industrial sectors. Outside the industrial sectors of interest to government laboratories (e.g. shipbuilding, machine tools, aircraft design) value questions become more central (e.g. in health, education, etc.).

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1) This is one of a number of policy issues in the science and technology field where political desiderata and efficiency may be in conflict. For example, Pavitt (1972) has shown how this was so in many attempts at European co-operation in high technology projects. See also Williams (1973).

When we come finally to the independent (or non-profit) sector the situation is confused by the heterogeneity of the research-performing institutes to be included, and the paucity of information. In terms of scale of resources, it is almost certainly grant-aided institutes in the agriculture, fisheries, and health fields which carry out most of the work in this sector in the UK (unfortunately we do not know what percentage this sector represent of all research work in these areas[2]). But it is probably in the fields of behavioural, social and economic research that this sector is proportionately the most important. (For example, in the field of education, it is probably of the order of 25 per cent of all educational R&D - see Ward, 1973, pp. 61-63. In the social sciences they are said to spend some £2.4 m. p.a. - OECD op.cit. p. 108). Examples of the well-known institutes which operate in these fields are the NIESR, the Tavistock Institute, CES, NFER, the Centre for Studies in Social Policy, etc. Of the work carried out in such institutions in the educational field, Vernon Ward writes:

> "It is usually more applied or action oriented than research at universities. Partly this derives from deliberate policy: there is a determination to apply research methods to practical or 'real' problems and partly it derives from the fact that much (perhaps half) of all research work is externally financed." (p. 68)

The same is probably true of most of the social research in this sector. As with socially oriented R&D in the government sector it is necessary to note that both, professions on the one hand and policy-makers/administrators on the other, are potential customers (whether willing or reluctant!) for such research.[3]

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2) Health R&D expenditure broken down by sector of performance is now being collected by the OECD. (See OECD: DAS/SPR/74.46).

3) Superficially it may seem that they are customers for different kinds of research outputs: hardware (drugs, educational technologies, etc.) in the first case,'softer' policy oriented findings in the other. This is not so clear a dichotomy: hardware innovations and policy innovations in these areas are frequently interchangeable as the notion of the 'technological fix' implies. (See, for example, Etzioni and Remp, 1973.)

5.2. The Links That Exist

In this section I want to outline what we know about the kinds of institutional links which exist between research performers and potential customers, leaving until later the question of their evaluation. Because it di fers, in terms of the kinds of problems raised and of the conceptual framework of the discussion, the utilisation of the social sciences is discussed separately. We thus discuss successively the 'customer' relationships of (a) university and (b) government institutions in the hard sciences, and then (c) the situation in the social sciences (focussing principally on the government and non-profit sectors).

(a) University-'customer' relations in the 'hard' sciences

Because close university-industry relations have been seen as an important stimulus to technological innovation in Britain, their promotion has much occupied policy-makers as well as many academics and some industrialists. As a result there is a good deal of information on relationships between universities and industry in Britain. A part of this literature is concerned with the utilisation of skilled manpower in industry: in what functions are graduate scientists and technologists employed? How does their training equate with industry's interpretation of its own needs? In what ways would industry like to see their education/training altered? and so on. These questions have been the subject of various official reports as well as of economic studies on returns to education, and of sociological studies into the determinants of graduate career-choice and subsequent job satisfaction. A further part of the literature is concerned with relationships in research, and for present purposes I shall focus upon this. Generally the issues treated have been: what sorts of relationships exist between firms and universities? What are the determinants (in terms of firm-size, sector, etc.) of such links? What can government do to promote such relationships? It is important to point out that almost all this discussion has been limited to manufacturing industry: that is, to that element of the economic sector which has generally been thought capable of profiting from a research input. A particularly compendious volume of the data which I have in mind is contained in the so-called Docksey Report (CBI Committee of Vice Chancellors, 1970). This was based upon questionnaires widely distributed among universities and firms. Typical of its findings are the following. Size of firm has a pronounced influence on contact with university researchers: whereas 90 per cent of large firms (5000 + employees) have such contacts, only 25 per cent of smallest ones (less than 200 employees) do so. Sector of production is also an important correlate: the more science intensive industries (e.g. electronics, instruments, pharmaceuticals) typically have closer links than the more 'traditional' industries (e.g. textiles,building materials, machine tools). The nature of these contacts is then discussed. Most of them, it transpires, are concerned with the instruction of staff (staff attending lectures, or courses, or working for higher degrees): contact strictly over matters of research is much less common. Again, sector of activity is an important determinant, there being much greater sponsorship of academic research and utilisation of university consultants in industries such as pharmaceuticals, petroleum, scientific instruments - and by the nationalised industries (above all). There is a good deal more information on the ways in which the most involved companies support university research (e.g. timescale, specificity, etc.), on their utilisation of consultants, and so on. There is a summary of industrial perceptions of obstacles to closer collaboration (e.g. "differences in research aims and outlook", "problems of commercial security", etc.), and all in all this Report offers useful data upon the nature, extent, and determinants of such links. The Report is, however, weak on evaluation: this is demonstrated by the brief account of government attempts to promote contacts between industry and universities (e.g by SRC, NRDC, the then Mintech, etc.) which are no more than described. The fact is that for any real understanding of the importance of such links we have to turn to the rather different studies of the origins of specific innovations: global surveys are of little help. This point is taken up again below.

(b) Government Laboratories and their 'Customers':

In principal these public sector laboratories have two sorts of customers: government departments, and industries or other external organisations. The relative importance of these two will vary greatly from one sort of establishment to another. At one extreme are Ministry of Defence laboratories whose sole 'customer' may be one of the Services. Then there are many multifunctional establishments with contacts with a variety of departments or in some cases with a variety of departments <u>and</u> firms (Harwell being a good example).

To my knowledge these relationships have never been the subject of academic inquiry. It is important to point out that over the years a number of policy initiatives have been taken which are likely greatly to have affected these relationships. Some of these - such as the emphasis on non-nuclear/commercial work at Harwell - have been specific to a single laboratory, whilst others have presumably had wider implications. A case in point is the reorganisation which took place in many departments following the 1972 White Paper <u>Framework for Government Research and Development,</u> which cannot but have affected the relations between laboratories and potential customers. Consider for example the industrial research establishment (IREs) of the then Department of Trade and Industry (e.g. National Physical Laboratory, National Engineering Laboratory, Warren Springs, etc.). In evidence before the Parliamentary Select Committee on Science and Technology, the Department's Chief Scientist (Dr. I. Maddock) gave some ideas of the potential effect of establishing the various subject based Requirements Boards[4] in DTI:

> "Each IRE will end up by having a portfolio of requirements which will have arrived through different requirement boards. Rather than the present situation where the programme is more or less invented within the IREs and then endorsed by a process of advisory committees and ultimately by myself ..."

In the past, industry had influenced the programmes "through the advisory committees to each IRE. Each IRE has its own committee ... In the future we see industry doing this through the requirements boards". There is considerable information on the extent to which these laboratories do work specifically required by industry, and so on. On occasion disagreement surfaced over the extent to which Ministries should act as "proxy customers" - a disagreement apparently less acute in the trade and industry field than in some others. The point that vesting this "proxy customer" role in the Ministry would reduce contact between research establishments and the 'real' customer was made most strongly by Agricultural Research Council Directors in evidence before the same committee. For example, Dr. H.C. Pereia (then Director of East Malling Research Station, which is concerned with horticultural research) said:

> "We are an institute set up by growers and run by growers. We have an absolute majority of growers on our governing body ... We identify our problems by continuous debate with the farming community in our experimental orchards ... we have 2000 visitors a year whom we take to the orchards ... However, we regard the ministry headquarters as somewhat remote and we are really quite alarmed at the possibility of a desk in Whitehall being interposed between us and our customers." (p. 154)

In sum, my points is that such evidence indicates the complexity of these inter-relationships as well as the substantial effects which government policy changes can have. It seems to me that a purely descriptive account or categorisation of these relationships (which would not be an impossible undertaking) would not only lead to some understanding of this crucial

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4) Made up of representatives of the Department(s) and of appropriate industries, with perhaps a sprinkling of academics - he referred to them as 'proxy customers' standing in for "thousands or even millions of customers". (See Select Committee on Science and Technology, 1972, pp. 457-77).

"proxy customer" role, but is a prerequisite of any evaluation of very many government policy initiatives in the research field. One cannot but wonder at the markedly greater attention which university-industry relations have received.

(c) Social and behavioural sciences:

Turning now from the 'hard' sciences to the social/behavioural sciences, it is worth recalling that we are concerned with two kinds of customers (which are themselves related in other ways): professional groups on the one hand (i.e. those who provide educational, medical and other social services) and policy makers on the other (i.e. those who determine or regulate the provision of such services).5) Before turning to the fairly substantial literature on both of these topics, there is one general point which I feel should be made. This literature is rather different from most of what we tend to think of as "science policy research". The authors of studies on the utilisation of educational research by educators tend to be students of education,rather than of research policy and similarly for other areas. In other words - though this is an issue which will be discussed at the end of the report - the science policy field has tended to limit itself to "policy for science" on the one hand, and research directed towards economic growth on the other. The utilisation of research in other areas of innovation has been left as the prerogative of other groups. When we turn to the utilisation of social research in public policy we again find a literature very different from that of science policy - conceptually often very sophisticated, but rather lacking in empirical data. But for the moment, the point I wish to make is that this has largely been left outside what we usually call science policy research.

Researchers and professionals:

The organisation of the health, educational, social and other such services is clearly different from the organisation of industry. They are offered by individual practitioners grouped in various ways, and controlled to very varying extents by local or regional authorities. Research related to their activities is organised very differently from the provision of services. In the medical field - which is not strictly our concern at this point - some research is of course carried out within the hospitals. But social research related to health service delivery, care, and organisation is more usually conducted in universities and specialist institutes and in relatively few of these. Educational research in

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5) There are of course other kinds of research using similar methods with very different clients groups: e.g. market research, research on management and organisation. It is simply necessary to restrict ourselves somewhat.

Britain is mainly carried out in university departments of education and in a few independent centres (e.g. NFER). It is clearly far from easy for the bulk of teachers to have close relationships with the educational researchers, and there are not the same economic incentives to such relationships as exist for a high-technology company. To take this as an example, there is indirect evidence of very slight contacts between the two groups and, indeed, of a certain antipathy. More directly, we know that educators are little aware of the findings or directions of educational research. In 1964 Lazarsfeld and Sieber estimated that only one per cent of American teachers read journals in which the results of educational research were reported in any detail (p. 56). They claim that teachers are little interested in research findings, and sceptical about the value of research other than of a purely "service" kind: that is, which is of direct practical use in their work, such as methods of assessment. Questions of organisation are also seen as relevant, in that there is no mechanism for bringing researchers and teachers together. Similarly Ward in his (1973) study of Educational R&D in Britain, writes:

> "It is a well-known phenomenon that practitioners and administrators within the educational system are in many ways the least active supporters of the R&D market ... teachers seem to have little knowledge and use for research results..." (p. 103)
>
> "There is a lack of communication between research staff, teachers, and administrators which prevents the free flow of information" (p. 104)

It is perhaps therefore not surprising that most commentators believe that educational R&D has made little impact on practice.

Researchers and policy-makers:

In considering the applicability of social science, as with the natural sciences, it is expedient to set aside the most fundamental work which is likely to have application only in the long term. The question then is: who among social scientists remain? How may we identify those whose work could have shorter-term implications for national policy? In my view the answer is best given in rather abstract terms. The social scientists who interest us (let us call them policy-oriented researchers) in the first place focus upon certain kinds of problems in some way akin to the kinds of problems which administrators face: questions of resource allocation, organisation, access to services, etc. Part of their work may consist in the evaluation of public policies (e.g. positive income tax, Family Income Supplement, the Rent Act, etc.). Part may consist in the more fundamental attempt at understanding the workings of some social system or process: the health service, the housing market, etc. But in my view the important criterion when they do this, is that they employ as explanatory parameters variables subject to administrative manipulation, or 'policy variables'. That is, in trying to explain differences in educational performance they will focus not on socio-economic background statuses, but (say) on differences in resources available in schools. With criteria such as these a 'policy research' group can be delineated.[6] They are to be found in the universities, in independent research institutes, and in government itself. Having made some distinction from more fundamental sociological or other research, there remains a further problem of delineation. This is the separation of the policy researchers from the government's advisers: whether whole classes (say the economics service) or influential individuals whose opinion may be sought. I do not propose to discuss this, as it is not perhaps an important problem for our present purpose.

What, then, do we know about the relations between these researchers and the policy-markers?[7] So far as the UK is concerned - rather little. But it is significant to suggest why we know so little, since I do not believe all can be blamed on the inherent secrecy of British administration.

If we look to the United States we find that a good deal of this policy research is conducted in a number of independent (profit and non-profit) research centres: the organisations commonly known as "Think Tanks". These have in themselves become a subject of some interest to social scientists. Among other inquiries one may point to Smith's study of The Rand Corporation which explores that body's relations with the military departments in some detail. Why has there been this greater interest among US political scientists and others? Perhaps there are three reasons: firstly, the Think Tanks are far more visible, bigger, better established, than are the independent research centres in Britain (or Europe); second, American social scientists (especially since "Camelot") are rather more sensitive to the uses and abuses of their work; and thirdly, perhaps policy research in Britain has appeared too peripheral to policy-making to be of interest to the student of government. When it is, apparently, close interest is created: witness the fascination of the Central Policy Review Staff for outsiders (see Heclo and Wildavsky, 1974). The fact is that in Britain the policy research community (or at least that element which appears to have any links with government) not only appears rather peripheral to the student of government, but it is so small. It would be difficult to avoid simply talking about a few well-known individuals - as Donnison does. And when one seeks to look at those doing similar kinds of work within government, then indeed secrecy may become a limitation. (Yet the fact is that the proper organisation

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6) Donnison suggests certain other criteria governing membership of this administrators' 'academic reference group' (Donnison, 1973).

7) Assuming for the moment that this is the crucial issue.

of this group of civil servants is currently a matter of some practical concern.)

So far as Britain is concerned, all we can really do is speculate about these relationships: about the systematic differences which certainly exist between areas of policy; about the sensitisation to research created by calls for reform; and so on. That is, we can speculate about the conditions under which policy-makers may turn to the researcher.[8] But this is not to say that his research will exercise a determining effect upon policy, or indeed that it will necessarily lead to change!

5.3. Institutional Links and the Utilisation of Research

We are concerned here with two related issues: the importance of institutional links for the embodiment of research in innovation, and the prescribed characteristics of effective such links. In attempting to summarise the relevant literature, one could ideally have introduced a matrix showing research performers ranged by sector down one side, and the kinds of customers we have discussed along the other. Each cell of the matrix, expressing one relationship, could have been discussed seriatim. Clearly, this is not possible: we know too little, for example, about government research establishments. So we shall adopt a more limited perspective, focusing upon the following issues in turn: first, research and industrial innovation - restricting ourselves once more to the university-industry interface; second, the embodiment of research in changing professional practice - again taking the teaching profession as a case-study; thirdly, the use of research in policy-change (developing the remarks of the previous paragraph).

(a) Research and Industrial Innovation: University-Industry Relations:

The importance of university-industry links for innovations has been demonstrated most convincingly by the study of specific innovation. To some extent this discussion depends upon the identification of the relations between fundamental research and applied research/development with those between universities and industry. As Pavitt and Wald write, "Science-technology links imply university-industry links". Indeed, some of the almost "classical" literature on innovation (e.g. Projects Hindsight and TRACES) are principally concerned with the utilisation of fundamental research in innovation. However, to quote Pavitt and Wald again, "Discussions on science and technology cannot remain general for very long. They have to focus on the institutions which produce and use science and technology, that is to say, on university and industry". And much of the recent work has indeed focused upon these institutions and their relationships. Yet the fundamental proof of the importance

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8) See, for example, R.A. Chapman, 1973.

of the relationship rests upon studies (such as Hindsight, TRACES, and their successors) which demonstrate the extent to which industrial innovations depend upon recent or ongoing academic research. To the extent that they depend only upon prior technology, or on the utilisation of well established scientific principles (whether embodied in the standard literature or in the education of the innovators), or on scientific work done "in house", these relationships are not important. Clearly this might be expected to vary between one industrial sector and another, and indeed, Gibbons and Johnston have shown that this is the case. Overall, the dependence of innovation upon ongoing academic research, or upon the 'frontier' knowledge of academic scientists, proves to be rather low (suggesting that university-industry links do not matter that much). But in certain other important areas the situation is far from this average.

The literature on economics of R&D demonstrates the varying usefulness of university relations and university research for industry. One determinant is clearly the level of R&D performed by the enterprise itself. Knowledge received from outside, whether from university, from a research association, or from another industry (as proves often to be the case in 'traditional' industries, such as pottery), must generally be modified so as to meet the specific needs of the individual firm. This requires some 'in house' R&D capacity, and is the basis of Freeman's finding that some threshold level of such capacity is a sine qua non of innovation in the electronics industry. A second determinant, which is reflected in typical inter-sectorial differences in university relations, is the nature of the characteristic innovation in a specific industry. That is, whereas in the science-based industries (for example, electronics, scientific instruments, pharmaceuticals) technological innovation (in products or processes) characteristically depends upon scientific advance, in other more traditional industries innovation seems to be stimulated in other ways.[9] Of course, a strong relationship between science and technological change does not necessarily require strong institutional coupling between industry and university. There have been cases when specific companies or industries have been well ahead of the academic community in the fundamental and the applied research relevant to an area of potential innovation. The classic example of this is Bell Labs in the field of semi-conductor research (Nelson, 1962).

9) The question of why this should be is a fascinating one. Is it simply that there has not been much research appropriate to the needs of these industries; or that because of their organisation and the values prevalent in them they have not been able to capitalise on what might have been useful; or is there something innately different about their products which makes them less compatible with the findings of science? The process of innovation in traditional industries (e.g. textiles, printing, building) is becoming an important area of study.

The question of how important university-industry links are for industrial innovation is distinct from that of the optimum characteristics of such links and the supplementary question of how such links may be promoted.

The same detailed case-study approach to specific innovations (at least in its more recent 'behavioural' manifestations) also sheds some light upon these matters. So too do the more subjective and evaluative methods used e.g. by Wald and his OECD colleagues. The findings from these two approaches are complementary.

Johnston and Gibbons (whose work we take as exemplary of the case study approach) focus upon the sources of information inputs essential to individual innovations - determined by interview with 'problem-solvers' involved in each case. They found that "slightly more than one-third of the information inputs from sources outside the company which contributed to the resolution of technical problems arising during the development of an innovation could be classed as scientific" (p. 124).[10] Then comparing the importance of printed material (e.g. the scientific literature) with personal or institutional contact, they conclude "[scientific] information from outside the company was acquired equally by inspection of literature and by person-to-person contact" (p. 125). The rather limited utilisation of university (person-to-person) contacts having been established, the authors give some idea of the way in which these contacts are used:

> "Information was obtained from universities through a number of modes of interaction: by employing academic scientists as consultants, by supporting research in the university relevant to the company interests, by requests for advice and assistance and by use of the specialist facilities ... available at a university ... [T]hese modes of interaction did not occur in single isolated incidents but occurred either a great deal or not at all. The establishment of coupling between a company and university followed a characteristically cascading path, originating perhaps from the company hiring a graduate from the university or sponsoring research there...
>
> Scientists at universities were used largely in a supportive role, attesting to the feasibility of a proposed solution ... [T]he dominant impact of information from this source was that of definition or resolution of the base parameters of the problem" (p. 130-1).

10) I.e. the rest are 'technological', or technical, such as may be found in trade literature.

This 'micro' approach thus gives an idea of the nature of the university's contribution, and suggests that the various modes of coupling tend to occur together. There is no more detailed discussion of the relative importance of these modes, nor of the ways in which contacts can be made, and the 'cascade' initiated.

Pavitt and Wald, reviewing a number of the rather earlier innovation case studies, point to the importance of person-to-person contacts demonstrated by many of these. In later work[11)] Wald has apparently focused upon examples of seemingly effective coupling between companies and universities (e.g. the Basle chemical industry), rather than upon specific innovations. This approach gives an indication of the conditions (societal and specific) under which such coupling develops.

> "Most industry-university links do not start as links between two institutions, but as links between one man in industry and one man in university ... The first, the most important sociological basis of industry-university links is a network of informal contacts ... allowing for a continuous and easy flow of ideas and information ... this ... cannot be enforced by law..."

> "The extent of person-to-person contacts between industry and universities varies greatly in OECD countries ... Some German scientists say that in their country no university chemist and few good scientists in other faculties were not or are not in ... contact with industry, and no important university research programme is said to remain unknown to the relevant industry..." (pp. 226-27).

"Personal mobility" as a mode of coupling is discussed (in respect of which European countries often compare themselves unfavourably with the USA), but it is suggested that such mobility is very much culture-determined. Industrial consulting, teaching by industrial scientists, industrial support of postgraduate students, contract research (with the arguments for and against), and industrial influences upon curricula, are all discussed. Whilst the conclusion is not expressed in this way, it would appear that at the macro level (as at the micro level) "good relations" tend to involve most or all of these modes of coupling in concert.

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11) In the context of the OECD studies of The Research System in various countries (Wald, 1971, 1972).

Subsequently the role which government can play in stimulating such coupling is raised. The military sector (especially in the USA) is indicative of what government can do, although in France, Germany and the UK "governments have a greater tendency to pursue their action through other channels, not related to defence". A variety of methods have been used (e.g. institutions such as NRDC, ANVAR, "liaison officers", etc.); others are suggested: "governments can design income tax laws in such a way as to make industrial consulting by university professors financially more attractive" (p. 45) but the general conclusion is that government programmes "cannot replace the initiatives taken by industries and universities. Therefore indirect approaches may be cheaper, easier, and more effective than direct government intervention". Pavitt et al., more recently (1974) have confessed that we really have little understanding of the efficacy of various modes of government intervention designed to improve university-industry relations.

(b) Research and Changes in Professional Practice:

Here again, I will take educational research and practice as an example. The suggestion, made earlier, that research has had little impact on practice is probably not atypical of the social field. In part, it may admittedly be that the principal contribution of research is so diffuse and long a process that it is simply unobservable or indeterminable. This may be made a little more explicit with a quotation from a study commissioned by the US Office of Education. In a questionnaire interview directed at 'school superintendents' (i.e. administrators of local districts) they were asked:

> "To state which innovations in their districts were derived directly from educational research. The responses indicated that many respondents found the question a confusing one. On the one hand, superintendents were uncertain about what was meant by "educational research" and how they were to interpret or substantiate the derivation of practice from previous research. On the other hand, comments like "obviously, someone must have done some research on it" ... suggest that school administrators are not consciously aware of any connection between the operations of their school system and educational research activities." (Quoted in OECD, 1971; pp. 353-54).

Clearly this is a quite different situation from that obtaining in the industry/technology area. It is perhaps because of these participant-perceptions, and the possibly rather different route by which research may typically impact on educational practice, that we lack the kind of studies indicating desirable characteristics of researcher-practitioner links. There are, of course, a variety of well-informed subjective prescriptions, for example pointing to the need for improved information and dissemination services (for example, by W. Taylor - then DES's Adviser on Educational Research 1972). Yet the research literature on the diffusion of innovations, which is perhaps the most relevant research tradition since we are concerned with the diffusion of innovations as much as their initial adoption, demonstrates that the availability of information on a possible new practice is not in itself a general incentive to innovate (see for example, Katz and Lazarsfeld, 1955; Rogers and Shoemaker, 1971). Personal relations play a critical role: relations between the initial source of the change and those in some way predisposed to innovate, and between these innovative practitioners and their peers. The fact is that whereas in the industrial field a substantial social (as well as private) return may derive from innovation by a single company, in education or health a process of diffusion is an essential prerequisite of social benefit.[12)]

(c) Research and Policy Change:

By analogy with earlier sections, the questions we should pose here are: how important is research for policy change, and what kind of researcher-policy-maker relations best facilitate the utilisation of research? But does the analogy hold? Can we talk about 'utilisation' of social science in analogous fashion; and if we can, does the key lie in the relationships between 'doers' and 'users' of research?

This question is very difficult to answer. Those who have thought about the utilisation of social science in policy, and about the factors which inhibit effective utilisation, have come up with very different analyses of the 'roots' of the problem. Analysis in terms of organisational factors - of relationships between doers and users - is but one of these. Other analyses have focused upon conflicting value systems, and upon the fundamental nature of social science or of the policy-making system.

Official inquiries and reports, at least in the UK, have tended to proceed on the assumption that improved utilisation is possible, and that it can be brought about through manipulation of organisational factors. The Heyworth Committee was to some extent of this opinion, though it was subtle enough to recognise that any useful internal social science research unit would need authoritative support: "the officer in general

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12) Perhaps the difference is partly due to the fact that a single company makes available an improved product or reduced price to a substantial and (if the market works) growing section of the community. One school, or hospital, or doctor cannot do this. For a study of innovation processes in the service sector, see OECD, 1977.

charge of this research activity must be accepted and supported by the Permanent Head of the department, and his rank and qualifications must be such that he can effectively identify problems amenable to research and exert influence to ensure that the necessary research is carried out" (Heyworth, 1964). Similarly Smith, in his study of the (American) Rand Corporation - surely one of the most successful of all policy research institutions - emphasises relationships with client (especially the US Air Force) as a major source of effectiveness:

> "Rand's prominence in the defense-analysis field is certainly related to the surprisingly high degree of independence that it has enjoyed over the years ... it has been able to avoid the role of merely providing ceremonial and ritualistic support for a client's current interest." (Smith, 1966)

This kind of analysis is inherently optimistic. It is based upon the assumption that social research will become more useful - and indeed more used - if relationships of this kind, communication patterns, and so on, can be optimised. Other forms of analysis are inherently more pessimistic.

Some, for example, argue that there are important differences in values, predispositions, attitudes to policy-making, on the part of administrators on the one hand and social science researchers on the other. This, if true, suggests inherent and inevitable conflict and misunderstanding. Sharpe, for example, has suggested that policy-makers dislike having too much information (since anything more than that produced by normal administration makes decision-taking more difficult); that they dislike specifying the objectives of a policy too precisely (to ensure support, but confound evaluation!), and so on (Sharpe, 1976). And again, social scientists have been seen as reluctant to offer final unequivocal conclusions or recommendations. Unlike administrators, who want to 'deal with' problems, to 'remove them from the agenda', 'close them off', social scientists tend to want to open them up, to explore their wider ramifications.

As I have already mentioned, still other diagnoses exist. Some are in terms of the inherent nature of the political process. It can be argued that any view of the usefulness of social science in policy-making is necessarily based on a view that policy-making is, or can be made, 'rational' (in Herbert Simon's sense of that term). Many would be driven to deny any proper or significant place for the researcher because they do not believe in the rationality or rationalisation of policy-making. And finally, a fourth set of diagnoses are in terms of the 'inherent nature' of the social sciences: a view which has led Dror, for example, to his idea that what is needed is a new kind of activity, standing in relation to the existing disciplines of the social sciences more or less as medicine does to the biological sciences (Dror, 1973).

Though there is a great deal which could be said here, it should be apparent from these few remarks that the problem of the relations between social research and policy-making is not really analogous to the main theme of this review. The fact is that policy-making is not like technological innovation, and the social sciences are not like the natural sciences. To go into the nature of these differences - were there need - would take us too far from our topic. But it is scarcely surprising that the literature on this theme is so very different from that of science policy. And yet, even on this point, one cannot be unequivocal. There are indeed many ways in which innovations in the service sector can be regarded as, or depend upon, both policy change and technological change. There is also a sense in which technologically-based 'technological fixes' stand in a very important relationship to the changes in social policy for which they may substitute. Relationships of this kind - between technological and social scientific approaches to dealing/coping with social problems - are, however, an important issue for the science policy-maker, and hence for science policy research.

6. RELATIONSHIPS WITH THE FUNDING SYSTEM

The basic issue here is this: Do the availability of funds for research, the policies in accordance with which funds are allocated and the modes of funding used, the structure of the funding system, have an effect upon the research performance of dependent institutions? In discussing these issues I propose to limit myself to one class of "dependent institutions": the academic ones. We know relatively little about the equivalent kinds of relationships and policies effective in the government sector and, moreover, since funding and customer systems are largely identical, there would be little to add to the discussion of the previous section.

There is another general point to be made. Underlying this discussion is a practical problem (that of structuring and operating a research-funding system - for most intents and purposes the Research Council system). In some ways this practical problem is analogous to the research management problem discussed in section 3 although it developes upon government rather than on organisational management. Moreover, the operation of these bodies is necessarily situated in a political context, and this context cannot be ignored. The initiatives, and the scope, of the research funding agencies depend to a large extent upon the resources available to them. The scale of these resources is the result of a bargaining process in which the scientific community (including its representatives in the government committees) seeks to exert what influence it can. At the same time, the occasional restructuring of the advisory and funding system may also be the result of political initiatives.

6.1. Structure of Funding

The structure of the funding of university research has been broadly described in statistical terms in a number of places. Most university research is funded from universities' own operating funds (i.e. UGC funds) - at least if the contribution of faculty salary is included. Various estimates of this contribution have been made on the basis of academics' division of their time between research and other duties. We have statistics showing how the resources devoted to university research by the Research Council have changed and, from their Annual Reports, comprehensive data on the receipt of these funds by sciences and by institutions. The changing balance between these two kinds of sources (usually referred to as the 'dual finance' system) have been charted (Blume, 1969), and discussed by detailed reference to a few specific fields (Bevan, 1971). The results of these analyses is a demonstration of the increasing dependence of academic research upon 'project funding' - that is, upon the support of research councils. This is true not only of the UK, but of a number of other countries (e.g. Canada). The fact is that these specifically research-supporting agencies were able to obtain extremely rapid increases in funds throughout the 1950's and 1960's - more rapid growths than were available to universities through their normal operating funds. This was principally because of the powerful arguments which the 'scientific lobby' was able to deploy in arguing for a substantial national commitment to research. (I shall return to this point later.) However, (as I shall also discuss later) these funds were often not equally available to all academic institutions. In the British context, we are in fact rather ignorant of the extent and funding of research in the non-university post-secondary education system (polytechnics, colleges of education, technical colleges), although a survey of polytechnic research has recently been carried out by the Council for National Academic Awards.[1)]

There have also been a number of analytical discussions of the changing policies, priorities, and modes of support of the Research Councils, most notably by the Councils themselves in their Annual Reports. The work of the Councils was also discussed in a number of reports of the then Council for Scientific Policy (notably the Dainton Report, 1971a; and the Report of a Study on the Support of Scientific Research in the Universities, 1971b) and is of course the central concern of that Council and its successor the ABRC. For a more disinterested point of view we can turn to the comments pages of Nature or the New Scientist but, however informative, this is far from being well-grounded analysis. Partly, as will become clear, we lack the methodology for such analysis. International comparison, which can function as a methodological last resort in this area, has been used with a degree of success by the OECD. Notably in G. Caty's contributions to The Research System studies one finds a comparison of the organisation of what might generically be called research councils, of their committee structures, and of the modes of support adopted. (Caty in OECD, 1971, 1972.) Among the most striking differences we may briefly note:

- The British and Swedish (for example) research councils are subject differentiated, whereas their American, Canadian, French and German counterparts are not.
- Whereas in the UK all research councils are administratively and financially dependent on a single department (DES), in France and Sweden medical research councils (for example) depend in the same semi-autonomous way upon Ministries of Health.

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1) See also Ruth Michaels' Report on social science research in this sector (Michaels, 1972).

- There are substantial differences in the proportional balance between the support of academic research and the performance of 'in house' research (as there are in Britain between the Agricultural Research Council and the Science Research Council).

6.2. Procedures and Policy Initiatives

Traditionally the British councils, and their counterparts abroad, have tended to act largely by responding to requests for funds emanating from the scientific community, and evaluating these proposals on the basis of their intrinsic scientific merit. This is still characteristic of council activities in many smaller European countries, but very much less so of the larger (OECD) countries.[2)]

I have referred previously to the emergence and often rapid expansion of government agencies with a principal function of financing academic research on a project basis. The appropriateness of devoting substantial volumes of government funds specifically to such research, to be allocated in accordance with purely scientific criteria, and on a basis of 'passive response' to applications is being questioned in a number of countries: particularly in countries where such agencies have grown to a substantial size. A number of themes in departure from this traditional policy can be distinguished. In France, CNRS policy has emphasised cooperation between research teams within different sectors or institutions; the orientation of academic research towards scientific areas felt to be particularly in need of development; the creation of centres of excellence outside Paris; and the harmonisation of scientific and regional development policies. In Germany DFG policy has recently been directed at increased coordination of research in given scientific and geographical areas, and the development of selected fields of research not readily accommodated in traditional university structures and requiring substantial resources. The development of post-graduate education (Graduiertenförderung) has also been a theme of federal policy. In the UK policy has been aimed at the reduction of the share of post-graduate education in educational and research resources, and the increasing orientation of research council support towards research areas of apparent economic or social importance. (This is marked on the one hand in the policies which the research councils have themselves evolved - for example, the SRC's recent emphasis upon fields selected <u>by them</u> as being of importance. It has also been given further impetus, on the other hand, by changes imposed upon them, notably in the post-Rothschild White Paper, which partially subordinates them (certainly in financial

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2) The issues raised in the following paragraph are discussed in very much greater detail in S.S. Blume (1974b).

terms) to the needs of government departments.) In all three countries policies have long had as a principal objective the stimulation of the full institutionalisation of new areas of research, ultimately as new departments or institutes. Finally in the United States there was mounting criticism of a federal project-support system which concentrated resources in so limited a number of universities and in a relatively limited number of fields of science. In the 1960's both NSF and NIH initiated policies designed on the one hand to solve the problems faced by institutions frequently unbalanced by a high volume of project research, and on the other to catalyse the emergence of centres of excellence outside the magic circle (in different institutions, hopefully in different regions). A recent theme of NSF policy (whether one adopted willingly, or by virtue of political pressure) has been a growing emphasis upon the utilisation of scientific research in the search for solutions to national problems. Finally, it goes without saying that the recent cut-back by such mission oriented agencies as NASA poses problems for academic institutions. Not all countries have been forced, in part by the increasing political visibility of their growing research budgets to depart from the traditional laissez-faire policy of 'passive response'. But it is reasonable to assume that sooner or later they, in their turn, may be forced into reacting in similar ways to those described above.

Our problem then is that of assessing the impact of these changing policies on the development of science on the one hand, upon the academic system on the other. It is interesting that these councils have, in their various countries, been trying to achieve rather similar aims - but with quite different procedures - in the last few years. Have they chosen the right areas of research, the right institutions, the right mechanisms? We know a little - but not much.

It is fairly clear that new areas of research which have become institutionalised in the academic system have depended heavily upon research council or foundation support in their early phases. Participant accounts (e.g. Lovell, 1968) and socio-historical reconstructions (i.e. case studies of the emergence and institutionalisation of new research areas: see for example, Mulkay and Edge, 1974) offer some precise indications of the ways, and extent, to which the researchers' needs have been met. To complement this, and to enable us to assess the adequacy of procedures for selecting emergent fields for support, one might suggest (for example) a study of the fate of the initiatives which have actually been taken (e.g. by the MRC in establishing Research Groups). Other studies might be designed to assess the outcome of procedures for selecting projects within developed research areas: for example, how were the research projects of a few years ago now regarded as (scientifically?) most crucial funded ... and so on.

Another issue which seems important may be the balance between the _kinds_ of activities funded. By this I mean the balance between (say) hardware (equipment, computer time), research assistance, investigator's own time, and so on. A number of experienced scientists have reflected unfavourably on the greater facility with which equipment can be obtained than can, say, time to think, irrespective of the needs of the discipline. It is quite widely believed (and some officials have given credence to the belief, especially in the USA) that some funding organisations actually _prefer_ to deal with (a few) large proposals: which cannot but affect the design of proposals for study. All of this seems to be a particular problem for the social sciences - which have largely inherited a project-approach to funding from the natural sciences. Robert Nisbet, among others (in _The Degradation of the Academic Dogma_), has emphasised the distorting effect which this system of finance has had. According to him, there is an inevitable over-emphasis upon data collection at the expense of real thought and even, within empirical study, an underconcern with the analysis of data compared to their collection.

What of the current tendency to concentrate resources in a given field of research in a limited number of institutions - an aspect of current SRC policy (SRC, 1970)? How appropriate a policy is this? First, of course, there is the question of economy. In some areas, requiring very expensive apparatus (e.g. radioastronomy, high energy nuclear physics), it may scarcely be feasible to duplicate such facilities. When such facilities are made available, the locations chosen are likely to reflect the past performance (and prestige) of recipients, as much as (or more than) future promise. The inevitable tendency in deploying very large resources is to 'play safe'. It is not clear what the implications of this inevitable conservatism might be. On the other hand, it seems likely that the _efficient_ use of substantial resources also requires the creation of large teams around such facilities, so that they may be used intensively. This too militates against any dispersal. But there are other arguments. For example, the discussion of pp. 41-42 suggested th the validity of policies of concentration, of creating large teams, depended also upon the nature of the research being done. It was suggested that these questions (and the appropriateness of other organisational arrangements) could only be resolved in the light of an understanding of 'unknown parameters' which intervene between variables of these kinds and research effectiveness. These unknown parameters seemed in some way to reflect the inherent nature of the problem-solving task being undertaken. How can considerations of this kind be reconciled with economic considerations? Clearly, any answer to the question of 'in what field is a concentration of resources desirable' must depend on an integration of these two sorts of issues: the organisational determinants of effective research in different fields of science, and the savings generated by concentration in these various fields.

But we must also take account of the effects of such policies upon the research capacity of individual institutions and individuals. One result of the British "concentration and selectivity" policy which has given rise to considerable discussion is the relative starvation of the polytechnics of research funds. It seems clear that Research Councils receive few applications from the non-university higher education sector, and that the success rate is very low. This has led a number of prominent spokesmen for the polytechnics to suggest that both in their decisions and in the composition of their committees, the councils are biased against them. These claims led to the establishment of committees of inquiry by both the SRC and the SSRC. Interestingly, a similar phenomenon may appear when an academic community is differentiated on other bases than structure/function - e.g. language. In Canada the francophone universities argue similarly. A fundamental problem then for research council-type bodies is the appropriateness of an emphasis upon policies of institutional development (or 'positive discrimination'). This can only be resolved as a result of a political process, since it depends on the one hand upon the relative importance of two distinct goals (scientific development and institutional development) and on the other upon the (political) delineation of responsibilities between research supporting agencies and education supporting agencies. Whilst in a centralised state such as Britain the 'politics' of such an issue may be concealed, in a federal state (e.g. Canada, West Germany) they are open and frequently vexed, since research is generally a federal responsibility, education a provincial one. Larger, and politically central, issues are thus raised.

6.3. The Structure and Functioning of Committees

These criticisms also raise the question of the 'proper structure' of scientific advisory and grant-allocating committees. The appropriateness of representing elements of the scientific community other than disciplines (linguistic or geographic groups, types of institution) is one aspect of this question. Another is the question of committee structure, and its responsiveness to the needs of emergent (and frequently inter- or multi-disciplinary) research areas. There has been little analytical discussion of the first of these. Although I have myself raised it as an issue (Blume, 1974a), its further elucidation really depends upon an understanding of the saliency of such loyalties and commitments in the judgement of scientists in committees. All we possess is indirect evidence from analysis of the results of peer-review which does seem to demonstrate the intrusion of these external factors, in spite of efforts made to exclude them (e.g. anonymity in applications). Yet it must be borne in mind that the answer to this question of 'representation' depends upon a fundamental political verity. Why do governments have relatively 'public' scientific advisory machinery (which extends down to the grant-awarding committees in a pyramidal structure)? In part, it depends upon the wish of governments to ensure the acceptability of their decisions. In the case of policy towards research this is achieved by the implicit acceptability of decisions to a representative sample of elite scientists. For this to imply legitimacy in the eyes of the entire scientific community requires that 'representation' is across those divisions within the community regarded as most salient by its members. These may be disciplinary - but they may also be linguistic, geographic, generational, political, etc.

On the second question we have on the one hand the subjective accounts of scientists who feel disadvantaged by virtue of the fact that committee structures seem unable properly to deal with applications which are not in the central areas of traditional disciplines. On the other hand we have Hagstrom's theoretical account of disciplinary resistance to the legitimacy of emergent (and essentially deviant) fields. He suggested that fields whose goals or methodologies depart radically from those of the parent discipline (e.g. molecular compared to classical biology) may be undervalued or penalised by the parent. This is an aspect of disciplinary control (accommodation attempts may follow) designed to maintain allegiance and integrity. The formation of such a new committee is then an aspect of the ultimate success of the field in establishing itself. How, though, is this innate regulatory tendency to be balanced against the need for flexibility? These theoretical questions (in what may be called the 'political sociology of science') require a good deal of further thought. We also lack empirical data or information on committee systems. Mullins' current work on recruitment to the American advisory machine is the only relevant study of which I am aware. The central questions which concern Mullins are "the empirical structure of the advising group over the past twenty years ... its true size; the degree to which it is in fact closed; and the degree to which its members circulate from position to position within the advisory structure ... What are the characteristics of the scientists who serve within this system? How do these scientists contrast with those who do not serve?... how do [the apparent] selection criteria fit the missions of the parent involved?" (See Mullins, 1972b; 1974; Groeneveld, Koller and Mullins, 1975).

6.4. The Political Context

The structure and operations of these research-funding agencies, and of the general scientific advisory machinery of which they are a part, has a certain centrality in any integrated discussion of science policy research. In many ways they link 'its constituent' disciplinary parts. On the one hand election to such a body is an important and valued accolade - a reward for scientific achievement which is recognised as such by scientists. Sociologists of science have recognised this, and service on such committees is usually used as an aspect of 'recognition' in empirical studies of the operation of the social system of science.[3] At the same time this machinery plays an important role in the working of the policy-making systems (a conjunction which led me to my view of the need for a Political Sociology of Science). Similarly, Mulkay sees the scientific elite as "mediating" between the scientific community on the one hand, and government and society on the other (Mulkay, 1976). Moreover, as I indicated above, the composition of the committees (who and what needs to be 'represented'), and their relations with the constituency which they 'represent', is also an important issue best regarded from a political sociology perspective. But the disciplinary integration may go wider than that, as I now want to show.

In this section I want to discuss briefly first an aspect of the political role of this advisory machinery complementary to the legitimating function mentioned earlier. This is its function as a lobby on behalf of science: arguing the case for increases in resources. Second, and again briefly, the kind of arguments which can be (and have been) used - amongst which economic arguments figure prominently - will be discussed. These economic arguments, and their political context, can be viewed as part of a political economy of basic research. (Thus, the complementarity of the functions of science policy machinery provides a bridge between the political economy of science on the one hand, and its political sociology on the other.)

In its second (1967) report on science policy, the then Council for Scientific Policy itself acknowledged its lobby function:

> "We shall continue to press for the most favourable treatment for science that can be obtained ... our position obliges us to face the facts of the situation: Government must be convinced that investment in science should have the right measure of priority among other objectives, before resources will be made available ... It is perhaps a measure of the case so far established that the accepted rates of growth of expenditure for science remain considerably in excess of the general rate of growth of Government expenditure." (Paragraph 8).

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3) Studies of this kind are a substantial proportion of recent sociology of science literature. I have not discussed them as I do not see them as having great relevance for our rather wider concern here, since they depend on an 'autonomist' perspective upon science. But see, for example, Cole and Cole, 1967; Crane, 1965; Gaston, 1970; Blume and Sinclair, 1973b.

And indeed, statistical data show that through the 1950's and early 1960's budgets for scientific research, in many countries, did grow at a remarkable rate. It is also apparent that this situation can largely be attributed to the success of scientific advisors in persuading (admittedly receptive) policy-makers of the potential short and medium term benefits of research. This success itself, the literature shows,[4] derived from a combination of political and economic factors and arguments. An unbroken chain linked the scientists of those years with the successes of war-time science. As political scientists and historians have shown, out of the relationships between scientists and decision-makers established (notably in the US and the UK) during the war, came a political climate intensively favourable to science and appreciative of its contribution to national objectives. At the same time, those arguing the case of science in the 1950's were the same individuals who had so successfully organised the wartime research effort, and had established an important rapport with politicians. It was for these kinds of reasons, that, for example, Vannevar Bush in the USA was able to make so strong a case (in Science, The Endless Frontier) for the support of basic research in a condition of substantial freedom and autonomy. From this report (though admittedly after the lapse of five years) came the establishment of the National Science Foundation.

This situation persisted well into the 1960's when (as is well-known) the climate changed. The arguments lost their cogency; in the USA (partly as a result of the Vietnam War) the Executive became increasingly irritated with the academic community; a 'romantic reaction' against science set in which seemed to give rise to, or at least reflect, a groundswell of changing general political feeling, and so on. But it is still worth reviewing some of the arguments used, in earlier days, with such success. We may start with those presented by the Council for Scientific Policy in its 1966 and 1967 reports, in pressing the case for a substantial commitment to (basic) research.

Three arguments predominate. The first is the economic argument: "examples can be quoted where the United Kingdom has obtained very significant commercial advantages from its own basic science". And some are quoted: polythene, selective weed killers, control of tuberculosis, increases in crop yield per acre sown, and so on.

> "The examples we have outlined above illustrate that research of high quality in applied fields can be shown to pay high dividends, and that there are many more advantages to be won. But it would be the most costly mistake to assume that applied research ...

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4) See, notably, Greenberg, 1967; Vig, 1968; Gilpin, 1968.

> could remain of high quality and effectiveness without an intimate association with and support from relevant basic research..."

Moreover:

> "Relying mainly on the buying of science at second-hand without a counter-balancing capacity to offer advance in return, could only, in our view, result in an increasing dependence on the technologies of other countries, an outflow to those countries of the best of our scientific talent, and an increasing inability to recognise and gain advantage from scientific advances made elsewhere. To withdraw from the line of advance in basic science now is we believe to accept the future position of an economic and technological satellite."

Finally, the 'sophistication' problem of science-expenditure is referred to: the growth of the fundamental needs of research for new and more costly equipment which becomes available, independently of, and in addition to, the effects of inflation.[5)]

It is difficult to assess the contribution of (science policy) research findings to these arguments: the second and third certainly rest largely upon faith. But what of the first, economic, argument? Irrespective of whether or not the CSP and its Secretariat were aware of the literature, a number of economists had by then tried to make an economic case for the public support of basic research. Richard Nelson (1959) had argued that because the social return on basic research would greatly exceed the private return, private industry would tend to underinvest in it. Harry G. Johnson (1965) made a similar point. In the same volume (Basic Research and National Goals) Carl Kaysen gives:

> "Four different kinds of benefit to the community that flow from basic research".

First comes the basic research (--> applied research) --> innovation argument; second the relationship between research and higher education; and third the dependence of applied research upon basic research. His fourth argument stresses the value of scientists in coping with crises:

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5) Studies were initiated to measure the actual rate of 'sophistication' in research expenditure. But it subsequently became apparent that these studies actually yielded nothing more fundamental than an estimate of money made available for additional equipment, over and above the inflation in costs.

"an important reserve of capability ... that can be drawn on when national needs dictate." The example given, which puts the whole debate in historical perspective, is the contribution of scientists to the World War II military effort. Subsequent 'crises' inevitably make one rather more sceptical!

One final point. Since I have suggested that the study of scientific advice and advisors has an important role to play in the integration of the disciplinary contributions to science studies, or science policy research,[6] perhaps a brief word on the historian's contribution is apposite: especially so since I have concluded with a résumé of a debate of essentially historical interest. There is no doubt that our understanding of the role of advisory bodies, concepts of 'representation', arguments used for (for example) pressing the case for additional research resources, need to be set in historical perspective. All of these inevitably change with time, and it is for the historian to show us how, and why.

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6) I would now attach equal centrality to studies of politico-scientific controversies. Studies which show what sorts of political controversy rooted in technical disagreement can be of interest, are those of Nelkin (reviewed in Nelkin, 1975). See also Robbins and Johnston (1976).

7. THE EVALUATION OF NATIONAL SYSTEMS OF RESEARCH

There are particular difficulties in trying to offer a coherent résumé of the literature (and its problems) relating to the analysis and evaluation of national systems of research. Much of this work is comparative, and of course, there are important methodological difficulties in any kind of comparative research. Indeed within certain disciplines (e.g. political science, education) a considerable debate takes place as to how these might be usefully resolved.

But this is only one aspect of our problem here. Another, and more fundamental one, is the question of what exactly it is we wish to compare. Broadly, of course, the concern of this paper is with the kinds of factors which seem to influence the effective production and utilisation of knowledge, or in other words, of high quality basic and applied research and innovation. Some of these factors have been discussed in the preceding sections, particularly those which may partially determine the 'productivity' of a research-performing unit or institute. Clearly, national differences must to some degree be the result of aggregate differences in these institutional influences: average quality of research management, average quality of university-industry links, and so on. The range of issues, beyond this, which <u>may</u> be relevant is potentially enormous, and in the absence of any theory indeterminate. Amongst them, for example, we might plausibly suggest the following: levels of R&D resources (manpower, finance); distribution of R&D resources: between sectors, categories of research, disciplines, projects; industrial organisation; organisation of the provision of services; tax system; international trade agreements; a whole range of potentially relevant government policies, goals, and priorities; the organisation of the policy-making process (including the use or non-use of various planning and programming techniques); and many more, some as intangible as 'social values'.

A theory of performance would, of course, involve a selection of certain of these (and other) variables as of particular importance, and suggest their relationship to the dependent variable, but it seems apparent that we have no such theory. We cannot, at this time therefore, adopt any obvious framework for analysis. Instead, for present purposes we shall start at the other end, and see what kind of analyses actually make up such Comparisons of National Systems as actually exist. To be sure, principally under the aegis of international bodies (UNESCO, OECD), many such comparisons have been carried out.

I shall suggest that not only is the choice of variables for examination highly arbitrary, but that the prescribed relationships between these selected variables and 'research performance' is frequently based upon little more than intuitive conceptions of 'reasonableness'.

7.1. Comparative Analyses

At the one extreme are those analyses which are wholly statistical: giving, for example comparable data on R&D expenditure by sector for a range of countries (e.g. OECD, 1968 et seq.). Leaving aside the (substantial) problems of definition and of collection, these have been relatively unproblematical in their interpretation.[1] Their value does not derive from any implicit assumptions made by the statisticians as to the relationship between expenditure on R&D and 'returns' on this expenditure. It derives, very simply, from the interest which policy-makers attach to the monitoring of changing R&D expenditure in itself, by reference to other countries. Sometimes, of course, these data may stimulate, or may be used in support of, the view that a particular country is spending too much/too little on research, or too much/too little in one sector or another. Such statements necessarily rest upon assumptions such as that there is a 'correct' level or distribution of expenditure which may be deduced from the practice of other countries.

More recent statistical comparisons which have focused on national differences and trends in R&D expenditure by objective, rather than by sector, are somewhat more problematical. To the extent that expenditure by objective replaces expenditure by institution as the principal method of budgetary control and planning, the usefulness of such statistics will presumably increase. But their interpretation remains problematical.

> "Suppose we find that a particular government has increased its expenditure on R&D designed to combat pollution from $100,000 to $500,000 three years later. What does this mean? Is it necessarily the demonstration of clearly changing priorities? It might imply:
>
> i) an expanding allocation of funds for environmental control, of which a constant percentage is earmarked for R&D: i.e. an increase in concern for the environment;
>
> ii) a conscious decision to earmark a greater percentage of funds allocated to environmental control for R&D - i.e. an increase in recognition of the need for R&D in combating environmental pollution;

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1) Of course, the choice of indicators by means of which to represent R&D expenditure (e.g. in money terms, as a percentage of GNP, per capita, per Q.S.E. etc.) may have important implications for the conclusions suggested.

iii) the conscious diversion of funds from some other area of mission-oriented R&D - i.e. an increasing recognition of the potential benefits of this kind of R&D (whether economic, social, etc.) when compared with other kinds;

iv) the conscious diversion of R&D resources from fundamental research to research clearly related to the needs of (perhaps commissioned by) government departments - i.e. the application of a much broader policy within the environmental area;

v) the unplanned response of a research funding agency (e.g. a Research Council) to the changing balance of demand for research funds from outside (e.g. academic) researchers - i.e. changing priorities in the scientific community;

vi) the re-classification of statistics for political or other reasons. (For example, a study of the transmission of sound waves through large structures may be regarded as principally relevant to civil engineering, or to the reduction of noise pollution.)" (Blume, 1976).

It is not possible unambiguously to interpret the original data in terms of any clear shift in priorities without some knowledge of the kinds of decisions which were taken. That is, we need some understanding of the budgetary process, of the kinds of transfers-of-resources which actually took place in order to interpret policy. Similarly, before we can make statements like "more money must be spent on socially-oriented R&D" we have to know from where these additional resources are likely to come. Such data do, of course, allow us to conclude that more money was spent on a particular objective, but the interpretations which may be placed upon the bare statistics are limited.

Another kind of approach is that of Unesco. A number of reviews have been published in their Science Policy Studies and Documents series, of which No. 17 National Science Policies in Europe is usefully illustrative for our present purposes. The twenty-six national studies were each prepared by the national authorities according to a common framework, for presentation to a European Conference of Science Ministers. Under these circumstances it would have been unrealistic to expect detailed and critical analysis. One part of each report is concerned with human and financial resources for R&D. Another major section attempts to describe the research performing and policy-making institutions: their structures, functions, and inter-relationships.[2)] A third section, headed "National Science Policy", sets out some of the assumptions and priorities which underlie such policy and the planning processes (if any) by which such policies are formulated. The potential interest of such analysis is to some degree mitigated by the high degree of generality of the national reports. To take the Swedish one as an example:

> "The national policy goals which have been the chief stimulus in the vigorous development of research during the post war period in Sweden, as in almost all other countries, have been economic and industrial development, national defence and social development".

There follows one account of initiatives taken under each of these three headings. Under "economic and industrial development" it is stated:

> "The rapid expansion of the research resources, and the growing importance of research for national development, increase the need for long-term planning in this field. (However, partly because of the international nature of science) no explicitly formulated long-term detailed plan has been considered possible: the work is done on more flexible and general lines."

Now consider as an example of a country in which detailed central planning is practiced, Czechoslovakia ("science, as a decisive factor for the country's economic and cultural progress as well as for raising its living standards, must be directed in accordance with an integrated plan.") Some characteristics of the science and technology plans are indicated, together with certain underlying assumptions (e.g. "Small countries like Czechoslovakia cannot afford to develop basic science along its entire vast front, but must of necessity focus on selected sectors of science, where the country has genuine possibilities for attaining world standards.") and priorities (e.g. sectoral priorities in applied R&D). The approach adopted by Unesco does not, however, allow us to comprehend the problems associated with (different approaches to) planning or non-planning of science and technology as illustrated by Czechoslovakia and Sweden. Partly this is because countries very rarely attempt to explain their difficulties in science policy formulation and implementation - i.e. the problems which administrators face in working within the existing framework of structures and tools. In many cases (and the UK is unfortunately a good example) the picture given is one of a smoothly and

2) This exhausts the coverage of the earliest international analyses of science policies. But we have progressed from there.

effectively operating machine quite able to deal with the few real difficulties (which are of course under consideration). There is little self-criticism, and little attempt at focusing upon the problems of science policy. It is therefore not surprising that the Introductory analysis (by the Unesco Secretariat) deals almost exclusively with statistics and structures.

The OECD has attempted to become increasingly evaluative rather than descriptive in its recent studies, and has begun to move away from the 'assessment' of national science policies towards consideration of more limited aspects of such policies. This probably reflects growing doubt as to the feasibility of such global assessments. In contrast to Unesco, OECD has conducted its own evaluations, through its own Secretariat and through the utilisation of outside and independent experts. Let me take as an example of their more recent approach Volume I of their studies of the Research System in a number of countries. The purpose of the studies (subtitled "Comparative Survey of the Organisation and Financing of Fundamental Research"), can probably be described as the attempt at "describing and analysing the structures on which the flourishing of fundamental research as such depends, (and) also of bringing to light the conditions which encourage its use in the form of applied research" (Introduction, p. 16). The first volume is concerned with three countries: France, Germany and the UK, and the sections are arranged not nationally, but sectorially. For the purposes of this review I do not propose to summarise the analyses. I want simply to indicate the kinds of evaluative judgements/normative prescriptions which are made. In other words, what kinds of factors have the analysts selected as determining the quality of research production/utilisation, and in what ways are the relationships expressed? I have therefore tried to distil from this volume all the pertinent evaluative judgements, or prescriptions. They seem mostly to boil down to the following list:

- "reducing to the minimum the formal organisation of the system for the financing and orientation of research has obvious merits: minimum of red tape, great flexibility...";
- closer links between research and "the general functions of government" are needed;
- over-production of scientists (by the higher educational system), driving some directly into management, will improve the quality of industrial leadership, but this is an inefficient and expensive means of achieving this aim;
- the importance of mobility, both for individuals and institutions, is stressed;

- government programmes designed to overcome disciplinary and institutional boundaries, and promote co-operation, (e.g. the "actions concertées") are valuable, but not so valuable as direct initiative on the part of those concerned;
- the importance of multiple sources of funding for basic research programmes is stressed;
- academic institutions should formulate their own individual research policies;
- "it may be regretted, in the case of France and the UK that the institutions which have been set up do not include Councils with access to the Prime Minister, able to voice publicly and in complete independence recommendations and observations on science policy";
- the vitality of research staff can only be maintained if there is an adequate inflow of new people: this can be promoted by "redeployment of redundant scientists";
- there is a need to "internationalise" research groups and research advisory committees;
- it is necessary to "forecast the duration and cost of programmes designed to achieve specific aims", but these forecasts must be continuously revised;
- "Informal, person to person contacts not only allow far more encounters and hence far more industry-university links, they are the most efficient way of knowledge transfer that exists."

If this seems a rather small distillation from a 253 page volume, it is, I think, for two reasons. In the first place a good deal of the material is purely descriptive: it _is_ still quite useful to contrast various ways of organising and decision-making for a single function. Secondly, a good many of the most fundamental assumptions remain unstated, being, as it were, 'too obvious'. The need for close university-industry links is a case in point: debate is transferred to the discussion of alternative ways of promoting such links.

However, what can we say about these kinds of statement. Some of them, like 'the need for close university-industry links', have become so much a part of the conventional wisdom of science policy that their repetition seems platitudinous or trivial. One or two are certainly contentious, and reflect the personal views of their authors. More importantly, the number of _kinds_ of factors regarded as influential are remarkably few when compared with the enormous range of potential influences. The fact is, in my view, that international analysis of this kind (assessment by experts) rests upon little more than a series of consensual intuitions. There is little attempt at detailed characterisation of the variables, or at setting out and evaluating the alternative methods of realising an organisational change. The importance of "mobility" is stressed - but how can it be realised, and what are the obstacles? "Flexibility in planning" is stressed - but how can it be achieved? In other words, not only does the imprecise use of concepts minimise the academic value of such work (except as a source of descriptive data), but the failure to treat alternative strategies of achieving prescribed goals must reduce its value to the policy-maker. I shall suggest below that part of this problem derives from the international scope of these studies. A further difficulty derives from the broad scope and lack of definition of the problem. Other international analyses have focused upon rather narrower issues, and may have been more successful. But the fundamental issue, which international policy research (science policy among others) has never confronted is this: under what conditions are approaches developed successfully in one country transferrable to another?

One who has attempted to confront the issue, at least in part, is Wald, in his analysis of the science-industry interface in Israel, (Wald, 1972). This report is of considerable interest for a number of reasons. One is that it attempts to go much beyond the usual level of discussion of the relations between science and the university on the one hand and industry on the other. Many manifestations of disdain for the practical in the academic world are pointed out, and the author was able to try out some of his own ideas for improving the status of engineering (for example) in discussion. We learn, for example, that the idea of an Academy of Engineering, to parallel the Academy of Arts and Sciences, was not at all well received! But second, and particularly relevant here, is the attempt which the study makes to replace the USA by small European countries as the proper model for Israel to follow.

> "It is a working hypothesis of this report that the small, highly industrialised countries of Western Europe provide the best reference case and hence the least unrealistic model to emulate for a country like Israel...
>
> Israel's economic goals and problems have much more in common with those of small European countries than with those of the United States...
>
> Making the right comparisons is more than a purely intellectual exercise; France and the United Kingdom have made some very costly mistakes because their research and technology policies have too often been inspired by American precedents."

But for the science policy analyst to convince himself of the proper international model for a nation to follow is very different from convincing science policy-makers of the nation concerned, as Wald recognises. The arguments are well-chosen and lead on to the policy recommendations made.

> "...each country achieved with little or no foreign aid and without real sacrifices exactly that degree of independence in the development of weapons which it wanted to have: complete independence in Sweden, near complete independence except in aircraft systems in Switzerland, independence in certain specialized items with a high export potential in Norway or Holland."

International trade is seen as the crucial driving force, and technology policy is successful in so far as it leads to the development of "key industries" and specific "technological niches" within them, in which a small nation can establish itself internationally.

Thus, though we may not be able to offer a general answer to the problem of transferrability of approaches, we are I think able to indicate more and less valid models for emulation or study.

7.2. International and National Analysis

I suggested earlier that the weakness of many international studies, when they seek to be evaluative or prescriptive, derives from their international character. In the absence of any proposition-generating theory, evaluation has tended to rest upon 'experience' or 'intuition'. I now suggest that 'experience' will naturally place greatest confidence in relationships which appear to hold in many countries, even though their importance, and their interpretation, may depend upon individual, political, economic and social circumstances. The conventional wisdom of international science policy is based upon the search for the Lowest Common Denominator.

Let me therefore turn to some evaluations of, and prescriptions for, individual national systems. The most global aspects of policy (e.g. overall allocations of funds to R&D, the organisation of the policy-making function etc.) have probably been more often discussed in official documents than in academic analyses (at least in the UK). Let me therefore start by going over a number of recent British science policy documents, and try to pick out the perceptions and assumptions which underpin their analyses and their prescriptions.

On the one hand the 'conventional wisdom' finds a place: there are many taken-for-granted assumptions which have never been challenged. These are mostly similar to the international analysts' assumptions. For example, it has always been accepted that university research is best financed through the so-called "dual finance" system.[3] Increased staff mobility between the various research-performing sectors (university, government, industry) has been emphasised, as an important aspect of promoting inter-sectoral relations. Ways must be found of reorienting government laboratories which have outlived their initial missions. The scientific/technological expertise of government departments must be improved, and scientists and administrators must increasingly work together in the formulation of science-related policies. There is an increasing need for long-term financial planning with respect to science and technology. Since all academic institutions cannot be pre-eminent in all research areas, there is an increasing need for specialisation. At the same time, because both of the increasing costs of research and the benefits of large research groups, research in specific areas should increasingly be concentrated in a limited number of centres. It is wasteful to fund research at a sub-threshold level.[4]

> "The maintenance of expertise in wide areas, both pure and applied, depends on our holding our own in the fundamental disciplines on which the rest depend" (Council for Scientific Policy, 1968, p. 2).

In the field of international scientific relations, few would probably dissent from the view expressed by the then Council for Scientific Policy in its first Report: that we must distinguish "the natural and essential interchange of men and ideas which is at the heart of science" from specific international collaborative projects which "need careful scrutiny to ensure that full value is being got" (p. 14). Of course, it need hardly be said that whatever the overt unchallengeability of such propositions, their interpretation in policy may vary from year to year.

But on the other hand, there are a variety of issues on which 'official' thinking <u>has</u> changed over the years, and on which overt disagreement

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3) Although its exact working, and especially the balance between the two types of source, have been questioned.

4) These views have certainly been challenged by the scientific community-at-large. Indeed, the present author carried out a study designed to test their application in the field of chemistry on behalf of the then Council of Scientific Policy. Though its findings offered little support for such policies in the chemical field, the entrenched wisdom, supported by financial considerations, is un(likely to be) shaken. (Blume and Sinclair, 1973a).

still exists. An example of this kind of issue in the UK is provided by the need for some kind of central co-ordinating responsibility in relation to government R&D, and the kinds of powers such a body should possess. The advantages of a Minister of Science were spelled out by a Parliamentary Select Committee a few years ago, but this measure of centralised authority has not generally found much favour.

Instead, in the past few years we have had a Chief Scientific Adviser to the Cabinet, and, rather more briefly, a Central Advisory Committee on Science and Technology. Both of these have now vanished.[5] In Britain as in the USA and elsewhere co-ordinatory and advisory roles come and go as government machinery is re-organised. If there is any one continuing trend towards increased centralisation in many countries it derives from the introduction of financial management methods (PPBS etc.). But such centralisation may not favour a Minister of, or for, Science!

The same kinds of disagreements exist over other aspects of the organisation of government research planning and funding machinery. Few formally concerned with national science policy would now be prepared to agree with the (1963) Trend Report that "it is inconsistent with the conception of an advisory council that it should include official members" (p. 26). The Trend Committee accepted the need for the complete autonomy of the Research Councils, basing its view in part on the Haldane doctrine. Their complete freedom from political or official interference (within the limits of the funds allowed them) was seen as the best way of contributing "to the Councils' ability to promote research and development". By the early 1970's this view was under powerful attack. Seeking to defend the status quo against a proposal to transfer the Agricultural Research Council (the Council with the largest percentage of applied research) to the Ministry of Agriculture, Fisheries, and Food, a Committee of the CSP was obliged to give ground - in a sense - on two points

> "Research Councils will have to become increasingly well-informed about national needs and objectives, so that they may try to deploy scarce resources in the most appropriate direction, and be seen to be doing so."

> "It is necessary to have a coherent policy for the whole of (basic and strategic science) activity, especially during a period when costs are likely to grow more rapidly than resources."

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5) A Chief Scientist has recently been appointed within the Central Policy Review Staff ("Think Tank") of the Cabinet Office. He reports to the Head of the C.P.R.S.

- whilst nevertheless trying to preserve the most crucial element of the traditional doctrine:

> "Whatever organisation is ultimately adopted to manage basic and strategic research: it should be one that unifies rather than fragments scientific activity, one in which the determination of scientific programmes is in the hands of scientists and one which retains a close association with the education and training of the scientists of the future." (Council for Scientific Policy, 1971a).

(Their attempt at preservation is thus based upon three Powerful Truths: fragmentation must be avoided; scientists are best able to fix scientific programmes; research is bound up with higher education.)

Lord Rothschild, in an alternative and ultimately more influential[6)] set of proposals, was much more critical on scientists' ability themselves to articulate "national needs and objectives". Whilst rejecting the wholesale transfer of Research Councils to (most) appropriate Ministries, he sought to reduce their freedom of action in a number of ways (Rothschild, 1971).

Was Trend right in 1963, and Rothschild right in 1971? Or was one right and one wrong? Can 'right' have its usual meaning in this context, or are we really talking about political preferences? The arguments these reports themselves use do not enable us to tell. How could we tell?

7.3. Academic Science Policy Research

I want now to say something about the role, and scope, of academic research in all this. It is, after all, unrealistic to expect policy-makers, or official reports, to subject their own assumptions to detailed scrutiny, or to greatly concern themselves with problems of methodology. To what extent are academic researchers actually concerned with these global assessments? It is clear that economists have (almost alone) had an interest in questions of this kind for some considerable time. For example, in the 1950's and 1960's some effort was devoted to the quantification of the contribution of 'advances in knowledge' to national economic growth (Abramowitz 1956; Denison 1962, 1967). As Freeman has shown, the focus of interest among economists has since changed considerably, but interest in these broad issues remains. Let me now turn to one recent study as an example.

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6) Why?

A coherent interpretation of the British R&D system has been given by the American economist M.J. Peck, in a contribution to the Brookings Institute study Britain's Economic Prospects (1968). His theme is that science and technology contribute inadequately to British economic growth, and his concern is with the sources of, and remedies for, this inadequacy.

The fundamental source, according to his major hypothesis, is that Britain is trying to do more with its science and technology than its resources permit: manpower resources being seen as crucial. The UK science-technology-industry system is characterised, according to Peck by (i) a higher expenditure on R&D/GNP than the US (at appropriate US prices) with a labour force much less well qualified in science and technology; (ii) a very (over?) large volume of military and aircraft R&D; (iii) a very (over?) large and high quality basic research establishment; and (iv) an industrial structure in which research-intensive industries contribute more to manufacturing industries' share of GNP than in any other country. The overstretching of resources is manifest, above all, as a shortage of necessary engineering skills in those industries where the greatest economic potential exists.

The manpower shortage is explored in detail. It is seen as an 'historic' phenomenon, in which continuously rising (relative) salaries indicate a continuing (short-run market) shortage of engineers. At the same time the failure of the educational system to respond to this shortage (according to the usual market model) by increased output (i.e. supply) has maintained the disequilibrium. Industrial shortage of engineers has had to be alleviated by substitution, on the one hand of scientists, on the other of (non-professional) technicians, in functions best filled by graduate engineers. Evidence of the inadequacy of this substitution is provided by, on the one hand comparison with US industry (in which the ratios of engineers/scientists and engineers/technicians are both higher) and of relative salaries in British and American industry.

Four types of remedy are proposed. The supply of university educated engineers must be increased. The volume of basic research must be reduced, though not to zero, and not in industry. This is because it attracts too much of the best talent. A certain volume of basic research is of course justifiable (to the economist - in terms of the need to retain an 'interpretative' capacity; of its utility in solving problems arising in the course of applied research; and on account of its links with higher education). Basic research in government laboratories is most in need of reduction. Thirdly, the scale of the British aircraft industry must be greatly reduced. This industry is seen as characterised by its very high percentage of all industrial R&D, and its continuing unprofitability. This unprofitability is itself attributed, in part, to the shortness of production runs even of successful aircraft; the greater cost of the international collaborative projects, and a choice of projects "influenced by ... the pursuit of pure technology: aircraft development becomes an end in itself, too little constrained by the utility and costs of the result. Projects appear to be justified by a desire to maintain capability for future projects, rather than the output of the project itself". Finally, increased measures must be taken to promote research-intensive industries other than aircraft (electronics, chemicals, electrical and mechanical equipment, etc.), which are seen as crucial to the UK's future prosperity. His other three recommendations are all designed to stimulate the flow of high quality engineering manpower into these industries.

The elegant analysis suggests that by focusing upon a single function of R&D the economist is able to provide a clear, coherent, and practically useful evaluation of an R&D system. Its elegance depends not only upon the selection of a single R&D function. In my view it depends also upon the clarity and coherence of the underlying (economic) model, to which the importance of resource allocation is fundamental, and upon the identification of a single resource (manpower) as critical.

There is no doubt that the assumptions made could be criticised as over-simplistic, in strictly economic terms. They may also be questioned in other terms. For example Peck fails to discuss the crucial issue of how to choose which industries should be favoured, and structural factors, attitutes, managerial competence, political imperatives etc. play only a marginal role. Perhaps most critical is the question put by Freeman: why is it, that in spite of this widely read analysis, in 1974 the level of investment in the aircraft industry remains more or less the same (Freeman, 1977)? The answer, as Freeman suggests, must be sought in the politics of government R&D. As when considering the influence of scientific advisory bodies (particularly in relation to policy for pure science) at an earlier point, we must once again turn to political economy.

This should come as no revelation. J.K. Galbraith has indicated the relationship between the overall organisation of economic systems and technological innovation (see for example, his The New Industrial State), and the importance of government incentives, contracts, etc. - which may themselves be politically determined. At a different level of analysis, I suggested earlier that the importance of R&D statistics (showing, for example, 'commitment' changing between one objective and another) could only be understood in the light of what Wildavsky nicely calls The Politics of the Budgetary Process. In other words, just as growth in basic science expenditure could only be understood in terms of the influence, and the arguments, of the spokesmen for the scientific lobby, so too much of the macro-analysis of economists depends upon this political understanding. For example, an understanding of the range of options open to government, and the instruments available, in promoting industrial innovation, clearly requires political science and economic expertise. The political scientist can explore the reasons for which major development programmes are initiated and maintained, and the political weight attaching to the economic analyses of such programmes (e.g. the relationship between the economic report on the (then proposed) 300 GeV Accelerator and the decision finally reached at that time). The fact is that (as Diesing has pointed out) there exists a political rationality which does not necessarily correspond strictly to economic rationality. The major function of economic arguments may be in public legitimation. (An excellent example of the political scientist's contribution to our understanding of programme success in the high technology field is Sapolsky's The Polaris System Development.)

Of course, when we come to consider the initiation and maintenance of major research programmes which have an overtly social, rather than economic purpose, the kind of expertise required for analysis may be different. We may assume that the place in the decision-making rationale previously occupied by economic arguments will be replaced by others based on social values or social statistics. Unfortunately science policy research has not as yet been much concerned with programmes of that kind.[7)]

Thus, it seems to me that the attempt to understand and evaluate national R&D systems must depend upon theoretical comprehension of two complementary kinds. The first is of what might be called the 'rationality of public discourse': the terms in which the advantages and disadvantages of programmes are overtly argued. For the most part, this will consist of economic analysis (though not necessarily, as I have suggested, in the case of basic research policy or socially-oriented R&D). But it is, secondly, for the political scientist to explain the weight which has actually attached to these, and other, arguments which will be judged, in part at least, in terms of the standing of those who have deployed them.

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7) Though research programmes relating to environmental issues tend now to be considered in social and economic terms. 'Social impact' is now an increasingly relevant consideration in the energy field too. More purely social factors may be seen as properly governing the distribution of effort in the biomedical area: a 'rational' medical research programme could in some way reflect the distribution of diceases in society. (Though because intrinsic scientific interest as well as the possibility of breakthrough will also be relevant I doubt whether any such clear correlation would be found in practice.)

CONCLUSIONS

THE STRUCTURE AND PRIORITIES OF SCIENCE POLICY RESEARCH

On the basis of a necessarily subjective selection from the research literature I have tried to give some idea of the current scope, findings and limitations of science policy research. In the selection and presentation of this material I have tried to avoid distinguishing clearly between the various disciplinary approaches used (sociology, economics, etc.), and to emphasise rather their complementarity and integration. Similarly, by orienting the discussion around the determinants of effective research and effective application I have tried to build in an implicit policy-relatedness, without adopting too strict a selection criterion of utility. To have done otherwise, in each case, would have been to deny _a priori_ that there exists the potentiality for coherence and theoretical development in the field as a whole. In this final section I want to suggest how science policy research as a whole can be conceptualised, and to give some indication of what, in my view are its current priorities.

1. The Structure of the Field

At this time science policy research can be seen as consisting of two rather distinct foci of inquiry. One of these is the conditions of scientific development, the other is the study of the relationships between science (whether conceived of here as a body of knowledge or as a research process) and economic and other 'external' goals. Before going on to elaborate on this distinction, I want to make clear that I am _not_ distinguishing either between theoretical inquiry and policy-related inquiry or between specific disciplinary approaches. As indicated earlier I am anxious to avoid these temptations. I shall try to show that in each case theoretical questions as well as policy-related questions are confronted, and that in each case various disciplines contribute to the ongoing research endeavour.

How can the development of science be understood and influenced?

Two distinct (and ultimately ideologically based) approaches to this question can be found. First, there are those who argue in terms wholly internal to science itself. This would include most philosophers of science (who have tended to discuss theoretical developments in science in strictly epistemological terms); so-called 'internalist' historians of science; and the dominant (Mertonian) school of sociologists of science. The latter have focused essentially upon the social structure of science, which has been visualised as insulated from external influence. At the policy level, though the term is hardly appropriate, the most outspoken spokesman for this point of view might be said to be Polanyi, who has argued strongly that science must be left to develop independently if it is to prosper (Polanyi, 1962). If it is assumed that the ultimate theoretical and structural development of science is (and should be) wholly independent of external influences, it is necessarily assumed that science <u>policy</u> is at best irrelevant, and at worst can do profound damage (Lysenkoism being a case in point). There has therefore been no conscious interest in government initiatives or attitudes to science, except in those extreme cases where perversion and damage could be demonstrated. Similarly at the microlevel, studies of scientists in industrial environments sought to demonstrate an incompatibility between the internalised dictates of scientists and the requirements of the organisation (see Marcson, 1962; Kornhauser, 1962). Until quite recently this perspective was dominant both in various disciplinary approaches to the study of science, and at the policy level. This is rather less true today. In sociology and history of science external factors are increasingly seen as important to cognitive and social processes within science, whereas at the policy level traditional <u>laissez-faire</u> policies are being abandoned by research-funding agencies.

So we turn then to the second, and increasingly important perspective, which assumes that external factors may or always do affect science. This fundamental assumption unites a variety of research approaches. We may first distinguish those in the Marxist tradition who have attempted to explain developments in the theoretical interests or cognitive structure of science in terms of economic interests (e.g. Bernal, 1939; Hessen, 1931) or in terms of ideological commitment (e.g. Rose and Rose, 1974; Althusser, 1974). Second, there are those who have tried to demonstrate the influence of external factors upon the structure or size of the scientific community. Here we may include political scientists concerned with the impact of political commitments or political processes (e.g. Gilpin, 1968; Greenberg, 1967; Doern, 1972); economists demonstrating the effects of market pressures (e.g. Schmookler, 1966); sociologists interested in the effects of societal values etc. (e.g. Fournier, 1973; Blume, 1974a). Third, this same assumption necessarily underlies the study and the practice of science policy (in the sense of 'policy towards science'). Research on the structures of funding systems, or on international scientific co-operation; assessments of the impact of policies such as of 'selectivity and concentration' or of the Rothschild Report: all assume that science policy does or can have some effect. And it is apparent that Research Councils, in developing interventionist policies (or initiatives), also assume that these policies can have some effect: if not upon the final state of scientific knowledge 'when all is known', then at least upon the rate and direction of progress. From this perspective it therefore becomes of theoretical interest to seek to evaluate the effects of various kinds of policies towards basic science, to examine the effects of economic demands for certain technologies, or to explore the implications of political and social values and commitments (e.g. the environmentalist movement, the energy crisis, the decision to join Europe). There is, in other words, a certain identity between the kinds of issue which may interest policy makers, and those of theoretical interest from an 'externalist' perspective (although there may, of course, be differences in priorities).

The relationship between science and external goals is the second focus of interest. By far the best developed aspect of this area of inquiry is that focusing upon the relationship between research and economic growth or economically-motivated innovation. Here there is a traditional interest which can be traced back to Adam Smith, and Marx and Engels, and evolving through the work of Schumpeter, Galbraith and into the empirical work of Jewkes, Freeman, Williams, Mansfield, etc., and the less strictly economic work of (in Britain) Pavitt, Langrish, Gibbons and Johnston and so on. Political scientists (e.g. R. Williams), and behavioural scientists (e.g. Pelz and Andrews, Burns and Stalker) have also made a substantial contribution here. At the policy level, R&D has long been seen as an important factor in the achievement of economic goals: for example, some 25 per cent of the UK government's aid to industry is given in support of R&D (Pavitt and Walker, 1974, p. 65).

Let us now turn to consider some of the other objectives of national policy, and in the interests of which governments support R&D. In the military field, like the economic field, the relevance of R&D has long been recognised, and is reflected in the statistics (e.g. in France, UK 10 per cent of all defence spending is devoted to it - OECD 1973). Here too, there has been some research on the way in which science contributes, and can best be made to contribute, to military goals notably by political scientists such as Sapolsky, 1972; van Dyke, 1965; Green and Rosenthal, 1963. We might also include here work critical of the relationships between science and military objectives (e.g. on chemical and biological warfare - e.g. Rose, 1968; American scientists and the ABM - Cahn, 1974), including much produced by the radical science movements. The relationship between science and foreign policy can also be conveniently noted at this point (e.g. Skolnikoff, 1967). When we turn to consider the various social objectives of government (health, housing, education, social welfare, etc.) we find a situation which contrasts in two important respects with the economic and military/foreign affairs fields.[1] In the first place[2] statistics suggest that research has

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1) I have discussed the following issues in much greater detail in a recent paper "Towards a Science Policy for the Social Welfare Field" (Blume, 1975).

2) Excluding here pharmaceutical research, which has both medical and economic importance.

never been accorded the same importance as a means of achieving these objectives.[3] (It must be noted that 'research' here includes social research as well as research in the 'hard' sciences.) Improvements, say in the provision of health care, can derive either from research leading to the development of new drugs or from (say) socio-economic research leading to new methods of financing health care. Deficiencies (i.e. the need for innovation) can be defined either in 'technological' terms, or in terms of inadequacies in policy: preference being essentially an ideological matter. Secondly, there is a virtual absence of research on the relationships between science and these social goals. Such work as there is (which must be taken to include a good deal of theoretical speculation notably among sociologists) owes little or nothing to methodologies developed in the study of economic relationships of science. Moreover, it tends to be an area of debate quite distinct in personal involvement, context, and approach, from the rest of what is usually called science policy research. There seem to me to be no valid reasons why this need continue to be so, or why this area should continue to be underdeveloped; quite the contrary.

2. Centrifugal and Centripetal Forces

Academic interest in the study of science therefore tends to focus upon these two distinct groups of problems. Yet the two are in fact related in a number of important ways, and before going on to consider how the field should now develop, it is necessary to say something about this.

In the first place they are related by the practical requirements of policy making. It is often the case that policy towards the support of an area of research seeks simultaneously to stimulate the development of science and the better utilisation of research in the achievement of some external goal. This is true of much medical and agricultural research, and of the areas selected by SRC as requiring special support and attention (e.g. polymer chemistry, enzyme technology, etc.).

Policy-makers would dearly love to have some set of criteria which enabled them to 'rationalise' this process of choice; and which would necessarily involve the summation of 'internal' and 'external' evaluations. The same kind of problem obtains when we turn from policy concerned with identifying areas for support to policy concerned with the development of instruments and mechanisms for support. How can the

3) The statistics I refer to are socially-oriented R&D/total social expenditure. Such statistics are admittedly scanty. There is however evidence that socially-oriented R&D/total R&D expenditure is increasing in numerous countries (see OECD, 1974).

maximisation of academic research's educational potential be reconciled with the demands of science itself? For the Research Councils this is an important issue which necessarily underpins the way in which they choose to finance research (for example, the relative weight attached to university research project support and the support of independent institutes). Similarly, government attitudes to international co-operative research ventures will frequently represent a compromise between the needs of scientists, economic considerations, and foreign policy objectives.

A second linkage between these two areas of interest derives from the political process itself. Arguments in terms of science's contribution to external (in the past economic) goals have been powerfully deployed by the scientific community in pressing for greater resources. Science policy research which has demonstrated the uncertainty of this contribution, and thus reduced the power of the argument, may have been one factor in the recent decline of the rate of investment in science.

Thus it is that the exigencies of policy-making and in particular the superficiality of the analytic distinction between 'policy for science' and 'science in policy', requires some integration between the two foci of academic interest. Yet the fact is that because of the polarisation of research interest, these intervening problems (such as those referred to in the preceding paragraphs) continue to be seen as the most intractable of all. It should be clear to all concerned in the policy-making process that such problems cannot be resolved without considerable advances in fairly basic understanding.

But the realities of policy-making exercise a certain centripetal force too. For agencies principally concerned with policy objectives other than the development of science, research is necessarily one means by which such objectives might be attained. Policy-making necessarily involves judgements of the relative advantages, say, of seeking to develop a new weapons system as against investing similar amounts of money (which in this example will be substantial) in further purchases of, or modifications in, existing systems. If policy-oriented studies are to aid the decision-maker with such critical choices, then they cannot be limited to the research-development option. From this perspective, which is equally applicable in all other areas of policy-formulation, science policy research (here the study of science's relations with external goals) has to be set in a broader context of 'policy research'. Since the focus of this paper is upon issues of concern to the Advisory Board of the Research Councils I shall not pursue this line further. It is however relevant to a 'total' conception of science policy research.

In considering the development of science policy research, therefore, the inherent tension deriving from the realities of the policy-making process has to be borne in mind. The relative saliency of these two perspectives (linking science's relationship with external goals on the one hand with the development of science, on the other with non-research methods of realising policy objectives) will depend upon the central functions of the customer agency. For the Research Councils it may be that the most important issues derive from the reconciliation of scientific development with the furtherance of external objectives (medical, social, agricultural, environmental,etc.). It therefore follows that, taking up the point made two paragraphs ago, a number of extremely difficult issues need to be confronted. Moreover, in my view we currently lack the theoretical base from which such an exercise could be mounted. Without denying that theoretically simple studies of a practical kind can be of use, I suggest that the really critical issues of modern science policy demand fairly fundamental advances in knowledge. In so far as what is needed can be compressed into a single question, it is this: under what conditions (cognitive, organisational, social) are the development needs of science(s) compatible with the external direction of science?

As I mentioned in the text of the report, the 'Finalisation' thesis attempts to deal with a part of this question: specification of the cognitive conditions which render an area of science susceptible to external direction of its theoretical development. Whilst this thesis may or may not be valid, it seems to me to represent the kind of theory which is needed. Its appeal for me as a method of speculation lies in the fact that it not only offers the possibility of theoretical integration of the two foci of research interest, but it provides an entrée to the most intractable problems of research-funding agencies.

3. Priorities in Research

Before moving on to give some idea of the kind of research which would seem to be particularly valuable, let me try to summarise my conclusions up to this point, and indicate their relevance for research priorities. I have argued that academic interest seems to be polarised such that some researchers are preoccupied with the problems of scientific change and development (many of them acknowledging the relevance of 'policy' for this process), others with the relationships of science and external policy goals (of which only economic goals have received systematic attention from science policy researchers). Subsequently, I went on to suggest that the real problems of policy-makers do not correspond to this polarity. The most fundamental problems of Research Councils seem to cut across it, and depend upon a reconciliation of questions in each field of interest. Their resolution must depend upon a fundamental understanding which we currently lack. On the other hand the major science-related problem of executive ministries requires a balancing of research-development and other strategies of attaining selected objectives. For science policy research to contribute here it must be integrated into a broader context of policy research.

This seems to me the current state of science policy research, and indicates very broadly the directions in which it has to move. Bearing this broad scenario in mind, it is possible now to consider the kinds of issues which, in the light of the literature review, seem to merit particular attention. In each of the examples given below I have tried to balance theoretical with practical value, arguing neither for a general theory (which I see only as a long term objective) nor for a quick response to the day-to-day needs of decision-makers. The examples are not given in any order of priority, and are meant only to be illustrative.

(i) Can scientific disciplines have 'needs' (research priorities) which do not correspond to the current interests and perceptions of practitioners? This would seem to be the view of many sociologists of science today. If so, how can they be identified?

(ii) It was suggested that the organisational correlates of effective research (e.g. optimum size of research group) seemed to differ from one area of science to another, and that there is therefore some set of variables intervening between these micro-sociological factors and performance. 'Task-type', 'technology', 'the kind of problem being tackled' seemed to be aspects of this set. The identification and explication of these variables would seem to be crucial if scientific policies are to reflect the varying needs of sciences.

(iii) What combination of factors in practice determines the resources made available to different fields of research? It proved useful to distinguish between the (overt) rationality of discourse, and the (closed) rationality of decision. The former traditionally consisted of economic arguments about the 'pay off' from science, which formed one weapon in the political process. Doubts about growth and the demonstrable uncertainty of this 'pay off' have reduced the power of this weapon. As a consequence, scientists seem to carry less weight in the policy-making process. But there is nothing objectively 'proper' about the new equilibrium; it reflects the changing political armoury. Can the long term future prospects for science be assessed in the light of new arguments which may become available?

(iv) How can the effects of various mechanisms for supporting research, or for promoting research-user relations be assessed? How can methods of supporting research in the social sciences be developed, which take cognizance of their clear differences from the natural sciences? What has been the effect of transferring organisational practices which have apparently been successful in the natural sciences to the social sciences? We know almost nothing about how to assess the effects of science policy mechanisms.

(v) Can science policy learn, whether from history or from the practice of other nations? To take up the second of these, which has important implications in many fields of policy-making, under what conditions can procedures (problem solutions) apparently successful in one country be transferred to another? (This has some similarities with the problem of the transfer of technology, some similarities with the diffusion of concepts between fields of science. But it may be more complex than either.)

(vi) What is the relationship between science and innovation in non-economic (and especially social) areas of government responsibility? What are the barriers to research-based innovation in the practises of the 'social' professions, and how can the utilisation of research be promoted? Equally, improvements in the provision of social services may come about through changes in policy (and the equation of deficiencies in provision with inadequate policy or with inadequate practice is a matter of ideology). What are the real relationships between research of different kinds and policy-change? (For such a study it would be necessary to distinguish, for example, between research leading to the availability of new 'technologies', such as drugs, and research concerned with the fundamental assumptions or options of policy-makers.)

These seem to me to be examples of the kinds of problems - all multidisciplinary, all combining theoretical interest with ultimate practical significance - which science policy research must now confront. In Britain the number of people working in the field is relatively small, and it is possible that the capacity to tackle such questions does not exist within the field. To be sure, question (vi) would seem to require an input from a variety of pure and applied social sciences which is not now in sight. In my view some way must be found of interesting political scientists, and other social scientists, in the problems of science policy. It is noteworthy that the relative overdevelopment of the economics of R&D owes a great deal to the apparent importance of R&D in economic growth, compared to its apparent irrelevance in other areas of policy-making. Yet only those for whom career considerations presently militate against any intellectual commitment to science policy are equipped to assess this relevance or irrelevance. I fear that a policy for science policy research must involve more than the identification of research priorities!

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