

**INITIATIVE TAKING IN
RESEARCH POLICY: SOME
SOCIOLOGICAL INDICATIONS**

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The objective of this paper is to consider the possibilities of 'initiative-taking' in stimulating scientific research, particularly the different possible modes of intervening, in the light of the 'state of the art' of the sociology of science. It therefore represents a tentative essay in 'applied' sociology of science.

INITIATIVE-TAKING BY RESEARCH COUNCILS

'Initiative taking', 'interventionism', 'dirigisme' increasingly characterise the activities of research councils and equivalent bodies in the larger Western countries (though this is not true of smaller European countries, such as Denmark, Norway, Switzerland in which project funds represent a smaller proportion of all academic research support). It is useful to begin with an outline of some of the approaches being adopted,¹ going on later to consider underlying objectives and assumptions in the light of current knowledge.

In France the CNRS has traditionally acted on the one hand through its own laboratories and research groups (127 laboratories, 98 research groups in December 1970), on the other through contractual agreements associating specific academic research groups with itself. Senior scientists in universities and other 'grands établissements' have long been able to seek support for their work in this way on a 3 or 4 year renewable basis. Such support would often include the finance of special research posts. In 1966 a new method of support, 'Recherche Cooperative sur Programme' (RCP) was introduced. RCP's are intended to foster co-operation between research teams in one or many laboratories which may even be geographically scattered.

Each RCP is steered by a 'Commission Permanente' composed of individuals with knowledge of the theme specific to each programme. The commission is intended to advise the director of the RCP both on scientific and administrative matters. In 1971, based largely upon the highly successful 'actions concertées' of the 'Délégation Générale à la Recherche Scientifique et Technique' (DGRST), the CNRS initiated a programme of 'Actions Thématiques Programmées' (ATP's). Certain areas of research are selected by the CNRS as of special priority, and scientists invited to seek support from funds allocated for this purpose. This procedure is an attempt to orient academic research, at least in part, towards areas of research felt to be particularly compatible with national goals. At present about 5% of CNRS resources are devoted to the ATP programme, a figure which is not expected ever to rise beyond 10%. However the CNRS hopes that funds spent in this way will exert an influence far beyond their quantitative volume. The indication of specific areas as being of particular importance, may serve to

elicit applications in the fields of declared ATPs, but outside the ATP programme. Beyond the initiation of such new mechanisms of support, there is one other important way in which the CNRS is seeking a more active role. This is in its increasing concern with regional aspects of its policy. The remarkable degree of research concentration in the Paris region (61% of researchers) is well known, and is of concern to the CNRS. Current policy is devoted on the one hand to the selective development of provincial universities which are already of high quality (e.g., Grenoble, Strasbourg, Toulouse), on the other to the development of a more effective research capability in certain universities in the less industrialised regions of France.

In Germany, the DFG unlike the CNRS has no institutes of its own. (However, similarly autonomous research institutions are operated, often in close contact with universities, by the Max-Planck Society). The role of the DFG is largely confined to the support of individual scientists. From its foundation this has been done principally by supporting the best of the applications made to it, on their own initiative, by individual scientists (whether from a university or some other institution). Support is generally offered for a limited and fixed period. This is what we have been referring to as the traditional function of research councils. But as early as 1952 a separate mode of finance, the 'Schwerpunkt' (priority) programme was introduced, partly in the attempt to fill gaps in German science resulting from the war. Areas of research are recommended for designation as 'priority areas', whether by academic scientists, officials of the DFG, representatives of public organisations concerned with applied science, etc. Representatives of the academic disciplines concerned are consulted on the value of such designation, upon the precise delineation of the area to be covered, and upon the names of scientists who may be especially competent within it. When the programme, and the list of scientists, has been approved by the various relevant committees of the research council, the scientists nominated are invited to apply for support under the specific priority programme. Whilst it is important to note that initiation of such programmes is not predominantly by the DFG itself, the priority programme is a clear step away from passive response to the requests of scientists. It is interesting to note the relative evolution of these two schemes. Expenditure on the normal or traditional programme rose from DM 10.6 million in 1955 to 72 million in 1965 and 98 million in 1969. Expenditure on the priority programme rose from 13.9 million in 1955 to 51 million in 1965 and 58 million in 1969. The priority programme, which has not particularly increased its share of resources, is also largely limited to the natural and engineering sciences.

In addition to the priority programme, a scheme of special research areas (Sonderforschungsbereiche) was introduced in 1967. In 1960 the German Science Council (Wissenschaftsrat) had suggested that the needs of much modern research (in terms of scale of resources, need for multidisciplinary co-operation, etc.) were incompatible with university structures. Much research was seen as requiring large specialist research groups, with expertise crossing the traditional institute boundaries. The necessary scale of such activities was such that no university would be likely to allocate them to a single field, nor could they properly be duplicated from one university to another. Specific new fields of research should be concentrated in one or another university. It was also intended that the programme would on the one hand facilitate closer co-operation between scientists from universities and from other research institutes who might work together irrespective of institution; and on the other to ensure a higher degree of co-ordination of research between the research institutes of a given area, land, or throughout the whole country. Universities may suggest research fields for inclusion in the list of special research areas. In doing so they are required to give their reasons for making this suggestion, details of the work proposed, and of scientists and institutes who might be involved. The agreement of the Land Education Ministry must be secured, since the existence of such a programme is seen as a profound influence upon the future structure of the university. The German Research Council then evaluates the suggestion from a supra-regional point of view, focussing less on the needs of the university than on those of German science as a whole. If the detailed proposal is subsequently found scientifically satisfactory, support may then be made available by the Council. It is important to note that a programme for approval requires that the host university commit itself to increased support from its own resources (in the form of buildings, appointments) even at the expense of developments in other fields. Hitherto in selecting areas for inclusion special attention has been given to medicine, the natural sciences, and technology: in 1970 DM 64.5 million was spent on 77 special research areas.

In the United Kingdom the 5 Research Councils all work in slightly different ways, differing in particular in the extent to which their work is conducted in their own institutes independent of universities. Traditionally all the Councils have offered research grants in response to application from university scientists, for projects 'of a special timeliness and promise'. We are concerned here with the developments in policy towards university research indicating modification of the traditionally passive role.

Both the Agricultural and Medical Research Councils have long operated their own Research Units in association with universities. The Medical Research Council (MRC) has about 75 such units, the Agricultural Research Council (ARC) about 11. The intention in establishing such a unit is to support a problem-orientated research activity not easily accommodated within the university structure. Units are explicitly formal around a specific individual who may be a university professor or a Council employee, who is usually given the university status of Honorary Professor. In fact the percentage of all research council support to research in and on the university which has been in this form has decreased substantially over the years, from about 50% in 1957 to about 25% in 1967. In 1960 the MRC introduced a new form of support, the Research Group, to be distinguished from the units on the basis of their intended integration into the university structure. The groups are established where it is felt that the development of a given area of bio-medical research is in the national interest and can best be achieved by the provision of a grant to a particular university, to enable that university to expand the staff and resources devoted to the line of research in question. The objective of the scheme is to ensure the institutionalization of the research on a greater scale (e.g. as a new department of the university). The university is required to regard the development of this field as a major priority in the allocation of its own resources, and to undertake to fund the group from its own resources after a period of some years.

The Science Research Council (SRC) (the largest of all) does not carry out research of its own, or operate its own units, other than the national facilities provided for the use of all university scientists. Recent years have however seen important developments in SRC's research grant policy. Specific innovations in policy must be seen against the background of the Council's 'increasing concern to support work of economic and social value' and in the light of its view that 'the implementation of these policies will inevitably mean that SRC will exercise more influence over university research'. Two important innovations spelt out and widely publicized in 1970 are the policies of Selectivity and Concentration. The first was defined as follows:

'Certain areas, within a discipline or embracing a number of disciplines, will be selected for more favourable than average support during a given period, on the basis of a review of their special potential for advancing basic science, or their economic or community value, or all three. Other important criteria will be the economy of scarce manpower and the optimum utilization of unique or expensive facilities in universities, national and international laboratories and in industry'.

In the selected fields the intention is to facilitate the development of research groups, and scientists have been invited to apply for funds specifically allocated to each of these fields. At present about a quarter of the support given in the basic science fields and one-third of that in engineering is given to the selected fields: proportions which are not expected to rise greatly. The Concentration policy works in conjunction with the Selectivity policy: support being concentrated in selected departments as well as in selected fields.

So far as the United States National Science Foundation is concerned, I shall refer only to the Foundation's increasing concern with applied research, (following upon the Daddario Kennedy Act of 1968 which specifically authorized it to support applied research). In this context the Research Applied to National Needs (RANN) programme is important.

RANN's responsibility is to link basic knowledge and the application of knowledge by identifying opportunities, supporting research to elucidate these opportunities, proving some of the most promising concepts and transferring the results of this effort to users. During fiscal year 1973 the RANN programmes were organized in three major areas: Advanced Technology Applications, Environmental Systems and Resources, and Social Systems and Human Resources² Criteria used in selecting areas of activity are said to include importance of the problem, the 'pay-off' to be realised', the 'leverage of science and technology on the problem' and the readiness of capable manpower to work on it.

It is clear that these various procedures represent attempts to compensate for rather different perceived failings in the 'free market' research support mechanism. In addition to seeking to promote an area of research the intention may be to stimulate institutional co-operation (as in the SFB or RCP schemes); eventually to institutionalize research which is not readily accommodated in the university system (as in Research Units) or which, it is hoped, ultimately will be so accommodated (MRC Groups). The CNRS ATPs and the SRC 'selectivity' policy seek to alter the focus of scientific interest, without necessarily affecting the structural conditions of research. RANN seeks to develop and to promote the utilization of problem-oriented research. Whilst noting that initiative-taking is actually a more complex process in terms of stated (or unstated) objectives, for purposes of this paper I shall consider only that aspect aimed at stimulating research. That is, I shall not consider the implications of additionally trying to achieve structural re-organization (beyond the needs of the science in question). Nor shall I consider the problem of the identification of areas for initiative-taking, or the procedures by which this might be done, to which committee structures and the characteristics of members are certainly relevant.

Accepting these limitations, we may make the assumption that research initiatives may originate in one of two fundamental concerns. The intention may be to promote a research area or specialism which is newly emerging within the scientific community, perhaps at a point of conjunction between traditional areas. Or the intention may be to promote research relevant to some practical (eg economic or social) objective; either in the hope of finding 'a solution', or of generating a body of relevant (and in some way applicable) knowledge. Such research may be initially conceived of as monodisciplinary or as multidisciplinary. I shall return to this problem, for such it is, later.

RELEVANT SOCIOLOGICAL AND SCIENCE POLICY RESEARCH FINDINGS

Though no doubt many of the procedures mentioned above have been the subject of internal evaluations in the agencies concerned, so far as I know there are no systematic research studies which have sought to examine their intended and unintended effects on the intellectual and social structures of science, or on the utilization of science. As I have argued elsewhere sociologists of science have not tended to address themselves directly to policy questions, and policy oriented 'research on research' has largely been purely descriptive or has focussed on the antecedents only of industrial (economically-oriented) innovations.³ So we must turn to an essentially sociological literature, whose principal concern has been the theoretical one of elucidating the conditions and (social) processes of cognitive development in science. It will be possible to deduce some implications for the practical question with which we are concerned, though it is necessary to point out that here, as so often in juxtaposing social science with public policy issues, research contributes very largely by reinterpreting the question, demonstrating its complexity and indicating the frailty of administrative assumptions. (Caveat emptor!)

The Case-Study Approach: Emergent Research Areas

We may begin with a growing 'case-study' literature which has sought to analyze the social characteristics of new research areas and specialities and, more recently, to relate processes of social change to the cognitive development of the field in question. Among the better known of these are case studies of psychology⁴; molecular biology⁵; ethnomethodology⁶; invariant theory⁷; X-ray crystallography⁸; and radioastronomy⁹. Reference may also be made to two reviews of this literature published by Griffith and Mullins¹⁰ and by Mulkay, Gilbert and Woolgar¹¹.

In his study of the 'phage group' approach to molecular biology, Mullins sought to characterize the stages of social development through which the speciality passed, and to indicate (though not to causally relate) its intellectual structure and 'problematic' at each stage. Beginning with the movement into biology of a small number of physicists (eg Delbruck, Luria) whose standing was such that they could provide both leadership and resources, he suggests the following developmental process.

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 (information) 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 collegueship, 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 this area, and (ii) a corresponding decrease in disconnected or independent persons". (pp 58-9). Meetings are now held (1945-53), and a number of the phage scientists are now in a position to recruit post-graduate students to the field. ("Recruitment was crucial to the growth of the network" and most recruits were graduate or post-doctoral students). 1954-62 was the "cluster period".

"A cluster forms when scientists become self conscious 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, eg 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" (p74). We have now reached the "puzzle solving" phase, at which a consensus exists over acceptable solutions to problems, ways of obtaining solutions, etc. 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" (p77). Growth is now slower (unless those now graduating in the field are included).

In a later paper Mullins goes on to suggest that precisely the same developmental process can be used to describe ethnomethodology as a speciality (stretching the term somewhat) in sociology.

Mullins' analyses are compatible with Mulkey and Edge's study of the development of radioastronomy in Britain, although their treatment of the relationship between social and cognitive change is somewhat different. These authors too stress the catalytic role of intellectual migration: in this case the movement into astronomy of physicists (many of whom had been engaged in wartime radar work), but physicists marginal by virtue of their lack of commitment to any existing line of research within physics. Here too the importance of intellectual leadership (provided at Manchester by P.M.S., later Lord, Blackett), access to money and journals and to graduate students is stressed. The ability to recruit graduate students, thanks to their university affiliations, was crucial to both Manchester and Cambridge groups. (Indeed, it has been pointed out by Fisher, in his study of invariant theory, that this mathematical speciality 'died' - though its problems were not solved - for want of graduate students).

Long lasting British and Australian leadership (particularly vis-à-vis the USA) in radio astronomy is discussed, its origins being attributed to the substantial wartime efforts in radar development which provided both technically competent scientists and perception of research opportunity. (In other words, the wartime creation of practical problem-oriented multidisciplinary research groups gave rise ultimately to a new scientific speciality. Could this have been foreseen?

I return to this important matter below). There may be another lesson to be learned from the delayed development of radioastronomy in the USA. Though there had not been an equivalent investment in radar research, America had invested heavily in optical astronomy. Why did her optical astronomers fail to perceive the contribution which radio techniques could make? Two reasons are advanced. First, there was a lack of technical competence: optical astronomers did not know how to construct the necessary apparatus. Second, the tremendous investment (financial and intellectual) in optical instruments and techniques - which produced much more 'precise' results - was itself a barrier to methodological innovation". Indeed, Mulkey and Edge go on to suggest that integration between the two branches of astronomy is still only partial, to the extent that "radioastronomers tended unknowingly to reinvent techniques which have well established optical analogies (like the transit telescope), to repeat historical errors, and to struggle with classical problems while the basic principles they required were common knowledge among optical astronmers".

An issue related to that of 'technical competence' is raised by Collins, in his study of the development of the TEA (Transversely Excited Atmospheric Pressure CO₂) Laser.¹³ He was concerned to examine the way in which the kind of knowledge enabling a group of scientists newly to construct this apparatus can be acquired. It became apparent that this knowledge had an important 'tacit' (technique, skill, non-formal) component, which was not transmitted through the published literature. Laboratories, other than the originating one, learned to build working TEA lasers by contact with a previously successful laboratory only by personal visits and telephone calls, or by transfer of personnel. The extent of necessary contact depended partly on the extent of existing expertise within the learning laboratory. The point to be made here is that there is a general agreement that all scientific research has such a 'tacit' component (though it is likely to be greater in empirical than in theoretical endeavours).

Mulkey, Gilbert and Woolgar have attempted a synthesis of the literature on the development of new problem areas and specialities within science to which we might now refer. These authors suggest a three stage developmental process. "(The) most significant feature of stage one is the perception by scientists already at work in an existing area, of unsolved problems, unexpected observations or unusual technical developments, the pursuit of which lies outside that area". An individual's decision to move (or not) into the newly identified area will be a complex process, involving the weighing of a number of considerations; a point to which I shall return. The result of these individual decisions is, initially, that a number of researchers take up the new problems, isolated from one another and unaware of similar work elsewhere. It is suggested that initial success at this early (and soon often highly competitive) stage depends upon a

number of factors. Amongst these, "experimental ingenuity" is cited, since this early exploratory research often depends upon the design and construction of apparatus which is not commercially available. Resources are also likely to be crucial, especially research funds, of access to graduate students, as well as publication outlets and "approval by those in neighbouring fields who can help to confer scientific legitimacy on the new research enterprise". All of this may commonly depend upon the sponsorship of a scientist of high prestige, who may himself be active within the new area, or who may be active only at its margin, but who may support his innovative protégés.

Publication of the first papers, recognition of common interests, sparks off intensive information exchange and interaction, and a process of intellectual 'negotiation' as to the scope of the new research area, its priorities, and (perhaps) a division of problems. This leads into stage two of the area's development, characterised by rapid growth in numbers of scientists, as well as certain intellectual and social changes. Amongst the latter is to be noted: "the formation of clusters of collaborators or of teams of researchers working at the same site, which tends to occur at this stage, is a crucial social development because it promotes the adoption of common scientific aims and standards, as well as fostering commitment to the new area". The third stage of development is marked by the looser patterns of communication normal in science, levelling off in size, institutionalized procedures of recruitment and funding, formal organization and, most important, a routinization of research.

In their review, Griffith and Mullins stress the importance of leadership roles, distinguishing intellectual leadership from organizational leadership. These roles may or may not be filled by the same individuals. They too suggest the need for a 'critical mass' of researchers at a single centre (or 2 or 3, not more). Certainly, in the specialities which they considered this was the case: quantum mechanics (Copenhagen); phage research (Caltech, Cold Springs); modern algebra (Göttingen); operant conditioning (Harvard, Columbia); audition research (Harvard); ethnomethodology (University of California). Centres of this kind will clearly facilitate the 'intensive interaction', crucial at the earliest stages of development. And again, to reiterate, access to and successful recruiting of graduate students are essential prerequisites of necessary growth.

All of this seems to have certain implications for policy designed to facilitate the development of new research areas. Broadly speaking, a successful process of development seems to depend upon

1. facilitation of the 'intellectual migration' of scientists
2. the concerned involvement of one or two eminent scientists whose sponsorship can produce necessary
 - a. research funding
 - b. publication outlets for what will initially seem unorthodox or 'deviant' research to most scientists
3. technical back-up or know-how (which may require learning through visits; exchange of personnel)
4. later; possibilities of 'intense interaction' with other scientists now seen to be working in similar areas (through travel, summer schools, etc)
5. creation of one or two centres of 'critical mass' producing
6. possibilities of recruitment, especially of graduate students.

This is not the sketch for a general research policy. The case studies referred to, and which suggest such generalisations, are all concerned with research areas emerging within the scientific community and as a result of the changing perceptions of active researchers. It does not follow that creation of the approved social conditions for development of a research area identified in some other way will necessarily have the same effects. That is, the intellectual processes (cognitive development) also characterising the successful emergence of a new research area cannot be reproduced from a combination of (practical problem for research) + (social conditions of specialism-emergence). However, the perception (better, hypothesis) that internally-generated research areas pass through a series of developmental stages, each with its own functional requirements, does have implications for some parts of research policy. This will correspond to one of the stimuli to Research Council initiative-taking.

Differences in Ways of Working Between Sciences

In the light of empirical work in the sociology of science, generalisations of these kinds seem well-founded and potentially useful. Yet all of this must be regarded as a first approximation, for it depends upon the hypothesis that research areas pass through similar social and intellectual stages, irrespective of the problems which they confront, methodologies, etc. The attempt to draw out

policy implications depends also upon the assumption that similar sorts of social, organizational configurations (policy initiatives) will have similar sorts of effects upon different sciences. For the sociologist of science, today, these should be regarded as questions for examination.

In a study of the relationship between the size of the research group in which an individual chemist works, and his research performance, Mrs R Sinclair and I showed that the strength of this relationship varied substantially from one sub-discipline to another.¹⁴ Reflecting on this point I subsequently wrote:¹⁵

"Thus ... 'modus operandi', ... 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/organizational variables. Unfortunately, there have been no attempts to characterize and contrast scientific disciplines in these terms ... The distinction between 'applied' and 'basic' research ... is but an over-crude approximation".

In the terms of this quotation, then, the effect of any given organizational configuration upon research 'performance' will depend upon some complex of research objectives, tasks, and ways of working characteristic of the given discipline.

A similar question has recently been broached in more theoretical terms by Whitley,¹⁶ who makes use of an ideal-typical distinction between 'restricted' and 'unrestricted' sciences borrowed from Pantin.¹⁷ The basis of this distinction lies in the nature of the systems analysed by the two kinds of sciences: the 'richness' of their constitutive phenomena. Thus, 'restricted' sciences are seen as concerned with events or phenomena which are themselves esoteric, existing (in a sense) only within the terms of a complex theoretical structure. Sophisticated and specific techniques (relating to the nature of the conceptualization) are needed for the manifestation and examination of these phenomena. All of this would, for example, be true of modern high energy physics. At the other extreme, the geological sciences approximate the 'unrestricted' science, in which objects/phenomena for examination are more nearly 'given by Nature' (or at least, are less theory-dependent for their meaningful existence).

It seems to follow that manifestation/examination is less precisely specified by theory: a greater variety of technical approaches (techniques) may be deployed.

It is suggested that different forms of work organization typify (and are appropriate to) the two kinds of sciences. According to Whitley, in restricted sciences (in which specialisms will be more clearly differentiated), a more rational division of labour is possible, and well developed criteria exist for the evaluation of alternative topics for research and methodologies (and hence, for the evaluation of research grant applications). The possibility of rational allocation of research tasks means that major problems can fairly readily be broken down into 'packages' appropriate to Ph.D. students. Clear hierarchies of problems, in terms of their interest/centrality to the field, will be found: so too will differentiation, specialization (serving in part to alleviate competition). This differentiation carries the danger, it is pointed out, that concern with the central integrative perspective will be resisted since it carries the possibility of a radical reformulation of priorities. It may follow that the attempt to exercise necessary intellectual leadership, associated with such concern, will require high prestige (or better, what Bourdieu calls 'cultural capital'), and will appeal to but a few institutions. All of this is much more open in 'unrestricted' sciences. Here, it is suggested, choice of problem and of research technique is less pre determined (whether by theory or by social pressure) and more open to personal judgement. It is less easy to divide up research tasks: whether among Ph.D. students or among (competing) groups - who may be starting from very different perspectives. Lack of accepted rules or criteria for setting priorities means that the task of grant-awarding bodies is a more complex one - and indeed selection of the membership of such bodies will also be more 'difficult' since there may well be a plurality of relevant elites.

The implication of all this is that different sorts of policy initiatives will be appropriate to sciences differing along a 'restricted-unrestricted' continuum. For example, what may have been appropriate in the early days of (restricted) radio astronomy - in which in Britain two major groups were able effectively to divide up the field between and then within themselves - may not be appropriate in (unrestricted) cancer research. Perhaps there is here the basis for a theoretical underpinning to the rather intuitive argument of those (such as Nisbet)¹⁸ who have argued against the unquestioning transfer of funding and organizational procedures which have proved successful in the physical sciences to the social sciences. However the present state of understanding does not seem to me to carry any more practical lesson than one of caution. Very much more

research is needed before the functional requirements of, and policy initiatives appropriate to, different sorts of research areas, specialisms and disciplines can be elaborated.

Practical Problems and the Initiation of Research

I made the point earlier that the case-studies considered above related to research areas emerging essentially from within the scientific community itself. Though external (social, economic) factors may have played a part in producing the necessary changes in perceptions or interests these were not treated in the analyses (with the partial exception of the role of wartime radar research in relation to radio-astronomy) and must be presumed of only indirect importance. This thus differs from the situation in which the decision is taken to mount a research attack upon a practical problem, significant in terms of the perceptions of administrators or professionals rather than of scientists.¹⁹ This too is an important aspect of initiative-taking in science, but one beset by complexities of a somewhat different kind from those discussed earlier. We shall see, and may as well stress the point here, that the most striking contribution of the social sciences is the demonstration of these complexities: rendering problematic matters which have generally been seen as wholly unproblematic.

The question of the translation of practical problems into research-(able) terms provides a convenient starting point. It may well be that in the deliberations of Research Councils even practical problems are raised ab initio in scientific terms, so that the translation problem is less salient. However in political-administrative settings - and perhaps in Research Councils to the extent that they may seek to respond to external demands - discussion of current concerns (whether social security or nuclear energy) is conducted in rather different terms. There has to be a translation process: handled organizationally in some Government Departments by committees made up of (administrative) customers and (research) contractors. There are real conceptual problems here, since policy concerns - usually rather broadly expressed - can often be defined for research in various ways. (An over-simple but illustrative way of expressing the matter is by distinguishing between 'technological fixes' and the search for more fundamental remedies for social woes.²⁰) The French mathematician Lichnerowicz has used the term disciplinary 'imperialism' to refer to the way in which scientific disciplines seek to extend their scope, or explanatory hegemony, over increasing ranges of phenomena.²¹ Similarly, my view is that disciplines will

frequently compete for the right to define a newly emergent practical (policy) issue in their own terms, partly in the hope of influence and resources. (Though it often happens that some disciplines will be denied the right to compete: the possible social scientific contribution to energy research/policy seems a general case in point). Two recent papers illustrate the nature of this competition (Robbins and Johnston), and the response of a scientific specialism to an emergent practical concern (Nelkin).

Robbins and Johnston are concerned with a recent American dispute over environmental lead levels conducted, on the whole, between medical scientists (eg toxicologists) on the one hand and physical scientists (eg geochemists) on the other.²² The dispute proves to have both cognitive and social (status) dimensions. In cognitive terms the geochemists criticised the medically crucial concept of 'threshold' which depends upon the identification of clinical symptoms of lead poisoning only at higher body lead concentrations. Lower (typically occurring) levels were then to be considered 'natural' and acceptable. By contrast entirely different geochronological methods showed that biospheric lead levels in the pre-industrial era had been about 1% of those common today - suggesting that this was a 'natural' concentration. Robbins and Johnston write: "The controversy bore all the marks of a conflict between self-contained systems of belief; lacking concepts and terminology in common, the protagonists tend to 'talk through' each other". The dispute is shown also to have a professional, or status dimension: the medical toxicologists responded by denying the chemists' and geologists' competence, or right to pronounce, in what they insisted was a purely medical area. The paper well illustrates the 'competition' which can take place over the right to 'colonize' a new phenomenon or concern, and hence demonstrates the social process by which issues come to be defined in terms of one specialism or another.

Dorothy Nelkin has tried to assess the effects of rapidly developing public concern for the environment upon the specialism of ecology.²³ Ecologists initially regarded the environmental crisis as an opportunity, and it was one which they seized. Rapid disillusionment, according to Nelkin's analysis, followed upon appreciation of the problems associated with political roles, their own lack of influence, and the 'direction' being given to ecological research:

"Practical problems, such as finding better indices to measure air pollution or technologies of insect control, neither satisfied the social ideal of ecologists seeking basic answers to environmental problems nor did they lead to very interesting research. Ecologists feel strongly that realistic answers to environmental problems will come only from predictions developed from basic research".

Weingart, in a related study of the effects of the environmental movement upon research in Germany, has suggested that the major effect was a 'relabelling' of on-going work to fit the terms of the new fad. Who can say whether or not the scientists are right in reacting sceptically in this way: for example, by criticising severely the assumptions and the effects of the U.S. Cancer programme? There are, after all, some questions which cannot be answered by today's science (called by Alvin Weinberg 'transscientific'²⁴) no matter how socially or politically pressing they may be. As Weingart has also put it: "Not every social or political goal is rational (in the epistemological sense) or able to be pursued by particular fields of science on the basis of the respective body of knowledge. In other words, the cognitive orientation complexes serve as selection criteria determining the range of 'external' goals that may be integrated into science".²⁵

Though I believe this discussion to some extent shows the way in which sociological analysis serves to make explicitly problematic some of the unstated assumptions of (science) policy, it does also have a practical moral. This is that the process of translating a practical (policy-derived) concern into scientific - and thus research - terms has to be recognized not only as important, but as problematic. It is not self-evidently true that a particular disciplinary approach, even one traditionally employed, has an unassailable claim. Nor can the reaction of a disciplinary (or other) segment of the scientific community to the expression of such a concern be easily predicted, as the environmental example (and no doubt also the Cancer example, were information to hand) makes clear.

Frequently the expectation in presenting a practical matter to the scientific community, or at least to its academic section, will not be a once-for-all solution. It will be rather the creation of a new research tradition or competence, or a body of practitioners, often of a multi-disciplinary kind. Such a development may offer the hope not only of a flow of innovations and advice, but ultimately of a more fundamental understanding of the issue. Of course, there

are many examples of scientists, technologists, social scientists from diverse backgrounds being brought together to work on some practical matter. In some cases the ultimate result has been the creation of an ongoing research endeavour with all the social trappings, at least, of a new academic discipline: ergonomics, management science or biochemical engineering may be examples.

These social trappings, and notably the institutionalization of the new field in the university system, may be enough to produce a growing body of practitioners trained eclectically to draw upon a range of academic disciplines (and ultimately upon the distillation of practical experience - as, say, in town planning) in tackling real-world problems. It may be contested, however, that this alone cannot (or at least does not necessarily) generate any new theoretical insights of the conceptual basis for more fundamental understanding. For example, it may be said of science policy research that whilst the problems tackled in multidisciplinary science policy research are indeed drawn from outside traditional disciplinary concerns, the theoretical bases for explanation remain those of the constituent disciplines: sociology, economics, etc. The same would almost certainly be true of educational research, with its much longer history. The question then is: is this enough, or is there 'something extra' to be gained from the reconceptualization characteristic of a genuinely new discipline?

It is necessary to go somewhat deeper into the objectives of initiative-taking, and into the problems presented, in these practically relevant areas. Consider for example some of the areas selected by SRC as of special national need for the United Kingdom, eg polymer science and technology, control engineering, manufacturing technology. In their 1974-5 Annual Report (pp9-10) the Council have this to say:

"Once an area has been selected by the Council for special support the response in terms of good research proposals has been satisfactory in subjects where there is already an active scientific community. However, it has been difficult to stimulate good proposals for work in fields which, though accepted as being of national importance, have not already proved attractive to academic research workers".

For example, in the polymer science and technology fields, designated in 1969 ...

"By late 1973 it was clear that, while the programme had been successful in polymer science, the scale of activity achieved in polymer engineering fell short of the national needs".²⁶

A special programme has therefore been established with the following objectives

- "i. to provide the highly trained engineers needed by the British polymer engineering industry;
- ii. to generate research programmes in universities and polytechnics in order to establish the base of knowledge on which participating firms can build their own applied research and new programmes; and
- iii. to ensure that within about five years the training and research projects established in universities and polytechnics have attracted the active collaboration and financial support of firms in the polymer engineering industry".

iii. is outside the scope of this paper. But we must note that the principal incentive in support of objective ii. is "more comprehensive support for identified extra costs than is normal on research grants". Leaving aside the stimulation of university-industry relations, then, it seems as though the fundamental aims/assumptions of the scheme are to persuade more scientists/technologists to work in the field; thereby to generate the requisite 'base of knowledge'; and to seek to do this by more lavish financial treatment.

Can scientists be encouraged, or motivated, to work in an area which though it may be of great economic importance is not scientifically fashionable (or interesting)?

There are a number of very different approaches to this question. Schmookler's classic economic analysis of historical trends in inventive activity suggests that, ultimately, sufficient scientists are so motivated.²⁷ That is, by comparing trends in output and in patent activity within various U.S. industrial sectors, he showed that economic demand tends to lead to the necessary scientific and technological activity: "The evidence strongly suggests that the output of a commodity and invention relating to it vary together, with invention tending to lag ... even basic discoveries which establish new industries are often, and perhaps usually, induced by economic forces like those which appear to operate in established industries". Schmookler's view is that the exploitation, at least, of basic discoveries - their embodiment in inventions - is determined by market forces ("unfolding economic needs and opportunities arising out of a changing social order").

Though of very great interest this neo-classical view has no place for initiative-taking, since proper regulation is by the market, and not by the dynamics of an only randomly utilitarian science. Other economists have argued that the market does not function properly in the scientific area, tending to produce an underinvestment in basic research.²⁸ Subsequent economic analysis has sought to explain the determinants of R & D investment by nations, by industries and by firms (alongst other things). Even setting aside the limitations of pure economic analysis (which I contrast here with political economy), my view is that the economists' conceptualisation of the problems derived from the SRC Report does not correspond to their practical manifestation. This is most generally because, although recent empirical work has explored the sources of innovation within (or, pace Schmookler outside) the research system, the crucial dependent variables for economists are 'invention', 'innovation', 'diffusion'. These do not correspond precisely to the concepts of 'research', 'development', still less to any conceptualisation of scientific change. Finally, the economic calculus has scarcely been extended to cover innovation outside the industrial area or (more generally) outside the market.

So, it may be that for present purposes it is more useful to turn to sociological and psychological approaches, and to pose the questions afresh. I suggest that the three aims/assumptions derived from the SRC polymer science/technology initiative may be re-expressed in the following problematic way, in order to be set against research findings:

- a. What directs/motivates scientists/technologists in their choice of research topics (for example, how relevant is the availability of financial support?)
- b. Under what conditions does a larger body of researchers working in a field imply a more effective generation of knowledge?

Question a. has been approached both psychologically and sociologically. My own view is that the psychological study of scientists and scientific activity is too dependent upon individualistic notions (eg of creativity unrelated to its intellectual context and conceived rather as problem solving) to be of broader use. There are exceptions: for example Nowakowska has sought to distinguish various scientific 'career strategies'²⁹. A major distinction in her typology is between scientists who seek to maximize their scientific (or intellectual) authority from those seeking to maximise their social (or organizational) authority. (This broadly corresponds to choice between the

ultimate exercise of what Mullins termed 'intellectual' and 'organizational leadership'). The kinds of problems selected for research by an individual are categorised in terms of their perceived difficulty and the subjective perception of likely success. The distinction is made between scientists who choose 'difficult' problems (which may appear more, or less, 'difficult' to the scientific community at large, depending upon the individual's abilities), from those who choose 'easy' ones: that is, between a 'high risk-high reward' strategy, and one which seeks to minimize the risk of failure. An approach of this kind, it seems to me, could be integrated into an approach informed by appreciation of the social nature of scientific development (and may have gone beyond the programmatic form of the paper cited). It ought, for example, to be relatable to Ben-David and Collins' perception of intellectual migration as facilitated by competitive pressures in their analysis of the origins of psychology.

This leads us to the sociological approach. A great deal in the sociology of science is concerned, directly or indirectly, with questions related to this one. I propose to discuss the literature only briefly since, at this stage of development of the field, the practical implications are few (though not unimportant). I should start by pointing out that, to my knowledge, no one has directly examined the extent to which scientists can be attracted into a research area by the possibility of lavish resources nor (equally important from the policy perspective) the kind, or quality, of scientists attractable. Interest has been much more in the significance of various characteristics of specialities themselves: it is an unstated assumption that these are of prime importance. The interaction between such characteristics and external (eg financial, organizational) factors in promoting scientific development are not yet properly understood, although we have already seen that given organizational configurations appear to impact differently in different scientific areas. The distinction between 'restricted' and 'unrestricted' areas seemed helpful in this context. The same distinction may be invoked again. The possibility of scientists being interested in a research area, by whatever inducements, would seem to depend - at least in part - upon the scientific interest (or cognitive status) of the area, as well as the degree of consensus among scientists in appropriate discipline(s) over the relative importance of questions for research and the social 'enforcement' (by the disciplinary elite) of adherence to this consensus. It was suggested earlier that consensus and enforcement' would be lower in unrestricted fields, scope therefore being greater.

We are also concerned particularly here with the stimulation of research areas of extrinsic (social, economic) significance, and have to consider the significance of this additional factor. What weight do such considerations have in the selection of research projects and in the advance of science? The issue has received a number of very different treatments.

There is, for example, a long Marxist tradition which has sought to explain both selection by individuals of research topics and the advance of science itself in terms of class interests. Hessen's analysis of the origins of Newton's 'Principia' in terms of dominant economic interests is a classic of this tradition.³⁰ Yet socio-historical analysis of this sort is of no more practical use here than Schmookler's neo-classical economic analysis, since it cannot separate ('so called') national needs identified in policy from the pervasive economic interests of which they are but a single, and by chance conscious, manifestation. In recent years the view that social, economic and political considerations play an important part in science, both as advancing body of knowledge and as social activity, has begun to (re)inform non-Marxist sociology and history of science. But it seems to me that this work does not, as yet, offer relevant practical lessons. Perhaps rather more is to be learned from the variety of 'critical' movements which have emerged in and around science and technology. Community science and technology groups, 'alternative' technology groups etc, are concerned - some theoretically, some practically - with promoting through science and technology the interests of the individual (and especially the minority or deprived) citizen.³¹ (The National Science Foundation is now trying to come to terms with this movement with its 'Science for Citizens' Program!) There is little doubt that ideological commitment would secure the interest of a segment of the scientific community in initiatives of a certain kind.

A rather different research tradition in sociology of science has focused upon the normative commitments of scientists. We now accept that scientists are far from equally committed to the advancement of basic understanding, to pure research, or to the norms (rules of social behaviour) generally said to govern research of that kind (such as autonomy in selection of projects). Adopting Cotgrove and Box's typology,³² we may say that whilst some scientists are committed to basic research and to the norms of scientific behaviour, others seek especially to apply technical skills to challenging problems and thus to gain the esteem of their immediate colleagues. A third group seeks the tangible rewards of a successful organizational career: for them, science is a means to an end. Empirical studies suggest that industrial and governmental laboratories include all types, whilst we tend to assume that university scientists are predominantly of the first kind (or else are

committed to teaching). My own study of university chemists in the United Kingdom³³ suggests that, in this field, there is a minority of university chemists with any manifest commitment to questions of industrial relevance: yet this minority proves not to have been highly productive (in purely scientific terms), nor to be generally esteemed within the scientific community,

The implication seem to be that few academic scientists are likely to be greatly swayed by considerations of practical relevance, and it may be that these will not always be the most gifted. Efforts in this direction are likely to encounter resistance from the scientific elite (highly socialized into the ethos of pure science, at least in most disciplines) which possesses considerable normative authority and powers of coercion. The likelihood of simple 'relabelling' of work (suggested by Weingart) may be a real, and not always easily spotted, probability. Perhaps university scientists are not necessarily or not always the best target for 'initiatives'.

There is a third line of research, now being pursued mainly in West Germany, which should be mentioned. Like the 'restricted' and 'unrestricted' distinction, this too suggests that the scope for external influence upon scientific development will differ from one area of science to another. The focus of the work is upon the elucidation of those intellectual, and to some extent social, characteristics of scientific specialities which determine their potential utility (or 'receptivity' to external demands).

In 1973 Böhme, van den Daele and Krohn described their concept of 'finalisation' in science, according to which when a scientific speciality had reached maturity its subsequent theoretical development was susceptible to external control.³⁴ That is, instead of existing theories being applied, post hoc, to practical situations/problems, these problems could initiate new (and by implication more relevant) theoretical development. Though the idea of 'finalisation' remains controversial among sociologists and philosophers of science, subsequent German work has probed the highly germane question of the utilizability of sciences. A recent paper by van den Daele and Weingart may be cited.³⁵ The thesis here is that at different stages of development (of which the 'finalised' stage is the ultimate) sciences are capable of responding to (or are 'receptive to') different levels of external demands, of which 'assessment/description' is the lowest, and what is termed 'systems formation' the highest. For example, the social sciences are said to be effective in offering assessments/descriptions of existing situations but not in developing means of intervention. Practical demands for something more than description may be made only of more 'cognitively developed' disciplines or specialisms:

theoretical metallurgy, for example, could suggest how a new material with given characteristics could be produced. Further, the more theoretically developed a discipline, the greater is the differentiation of specialisms, the more strongly rank-ordered by significance are problems, and the more stratified are statuses: "In so far as scientists belong to disciplines which have obtained a cumulative theory development, externally oriented 'applied' research will, as a rule, be ascribed a lower rank in the discipline's prestige system". It follows, the authors suggest, that for problem status reasons physicists are less likely to welcome applied research initiatives than physiologists. (The possibility of whether or not this status system can in any way be (consciously) modified, for example by the scientific elite, seems not to be considered).

The second question, b. deduced earlier from the assumptions underlying the SRC initiative in polymer science and technology was: under what conditions does an increase in the number of researchers in an area lead to a more effective generation of knowledge. There are one or two points which may be made with a certain confidence in this context. We showed above that in the early stages of new research areas, when numbers were very small, scope for expansion (especially by recruitment of graduate students) was crucial to the development of the work. But later, when the subject was growing a-pace (having secured post-graduate programmes, become established in universities etc) the scope for really major discoveries had passed, developments being of a more routine kind. The implication was that the significance of manpower growth depended upon the stage of development of the field. Considering rather different scientific activity, development work (say, large weapons systems, or much industrial R & D) does seem to depend upon substantial inputs of resources (including manpower). Size of individual R & D groups, upon which some work has been done, may be an acceptable proxy for overall resource inputs which are our concern here. In this sense, my own findings of a very weak relationship between research group size and research performance in academic chemistry³⁶ may be contrasted with Freeman's demonstration of a 'threshold' effect in industrial R & D.³⁷ That is, a reasonable hypothesis is that approaching the applied research/development end of the spectrum of scientific activity the more significant scale of inputs becomes. However, we may also say, taking up points made earlier, that at the basic end, significance will vary with the nature and stage of development of the scientific field concerned.

Frequently, of course, multidisciplinary research teams will be set to work on matters which may be perceived as like those initiating new scientific specialisms. This is perhaps particularly true of a variety of issues crossing the boundary between the social sciences on the one hand and the natural sciences/technology on the other. Sometimes an incorrect analogy with industrial development is drawn, as when it is claimed that resources such as those which put man on the moon could solve the problems of the city. I suspect that what is very often required is a degree of understanding which depends upon the emergence of new theoretical perspectives. Where this is the case (and I suspect that gifted scientists can recognize when it is so) I do not believe that rapid expansion of manpower or resources is at all productive. On the other hand, nor do I believe that anyone knows how this theoretical development can be stimulated or hastened - or whether indeed it can.

CONCLUSIONS

Research on science has not focused upon the specific problem of how initiatives can be taken, though I hope to have shown that a number of lines of research have relevance for a proper understanding of the problem. Broadly speaking, the practical lessons seem to be as follows.

There is an indication that where new research areas are emerging in the scientific community, they do seem to have similar needs at different stages of their development. These needs have to be recognized, and met, if work is to develop. But taking a broader look at scientific change, it appears that similar organizational initiatives (the kind of things that policy can do) impact differently in different areas of sciences. Increasing sizes of research groups, expansion of Ph.D. activity, finance etc differ in their effects upon scientific areas depending upon the kinds of phenomena confronted and the nature of theories and techniques.

Differences between the sciences constantly reappear as a factor making general prescription difficult. It seems likely, for example, that the scope for interesting scientists in specially selected problems, or problem areas, will vary between one field of science and another: some disciplines or specialities have rigid hierarchies of problems in terms of interest and importance. When these selected problems are of essentially practical importance (and perhaps of relatively smaller intrinsic scientific interest) additional difficulties emerge. Not only are most academic scientists not likely to be swayed by this consideration (and we cannot be sure of the quality of those who are), but additionally there will be a temptation to 'relabel' current interests in accordance with the current 'political' fad. Moreover, in certain instances scientists may be right to react

in this sceptical way, since not only are some problems beyond science ('trans-scientific'), but problems may frequently be beyond the capacity of their particular discipline to respond (at its current stage of intellectual development). Finally, practical problems do not in general correspond in any absolute way to particular scientific disciplines. Disciplines may compete for the 'right' to redefine a problem in terms of their particular conceptual frameworks. Where a multidisciplinary research approach is preferred, careful thought should be given to what precisely is being demanded of the researchers. Multidisciplinary teams do not possess (almost by definition) new theoretical insights, and where these are needed expansion alone cannot help in generating them.

Science policy research (that is, the various disciplinary perspectives which go to make it up) cannot yet provide the basis for a 'scientific' approach to research initiative-taking. It suggests caution; it suggests that initiative-taking will not always be wise (hence mode of intervention cannot really be separated from choice of area); and it suggests that there can be no overall procedure which will work irrespective of the science concerned. Perhaps, then, it suggests further research on the nature and development of sciences, to provide a firmer base of understanding for science policy.

Perhaps a moment's reflexion upon the very slight articulation between a growing volume of research upon the social dimension of science (and I am thinking here particularly of sociological research) on the one hand, and the complex matters of science policy on the other is called for. In my view the proper contribution of such research to practical policy has never been realized, and its proper relationship never clarified. However characteristic this may be of some more general phenomenon (the relations of social science to public policy), a counsel of despair is not in order. We may have here the basis for new insight, even innovation, in respect of this more general problem.

1. See Stuart Blume: 'New Teaching Research Relationships in Mass Post Secondary Education' in Structure of Studies and Place of Research in Mass Higher Education (Paris: OECD 1974)
2. National Science Foundation: Annual Report 1973 (NSF 74-1 Washington DC 1974).
3. Stuart S Blume: Science Policy Research: Its Current State and Future Prospects (unpublished report to the Social Science Research Council: mimeo'd, Sussex University, 1975)
4. J Ben-David and R Collins 'Social Factors in the Origins of a New Science: the Case of Psychology' American Sociological Review 31 (1966), 451-65
5. N C Mullins 'The Development of a Scientific Speciality: The Phage Group' Minerva 10 (1972), 51-82
6. N C Mullins 'The Development of Specialities in social science: the case of Ethnomethodology' Science Studies 3 (1973), 246-275
7. C S Fisher 'The Last Invariant Theorists' European Journal of Sociology 8 (1967), 216-44
8. J Law 'The Development of Specialities in Science: the Case of X-Ray Crystallography' Science Studies 3 (1973), 275-303
9. M K Mulkey and D O Edge 'Cognitive, Technical and Social Factors in the Growth of Radio Astronomy' Social Science Information 12 (1973), 25-61
10. B C Griffith and N C Mullins 'Coherent Social Groups in Scientific Change' Science 177 (1972), 959-964
11. M J Mulkey, G N Gilbert and S Woolgar 'Problem Areas and Research Networks in Science' Sociology 9 (1975), 187-203
12. In a later paper Fisher discusses most elegantly the effects of methodological or problem commitment (or, intellectual investment) upon the scientists. See his paper 'Some Social Characteristics of Mathematicians and their Work' American Journal of Sociology 78 (1975) 1094-1118

13. H M Collins 'The TEA Set: Tacit Knowledge and its Relevance to Research on Scientific Networks' Science Studies (1974)
14. S S Blume and R Sinclair: 'Research Environment and Performance in British University Chemistry' (Science Policy Studies No 6 HMSO, 1973)
15. Stuart S Blume: Science Policy Research (loc cit)
16. R Whitley 'The Sociology of Scientific Work and the History of Scientific Developments' (to appear in S.S. Blume (ed) Perspectives in the Sociology of Science (Wiley).)
17. C F A Pantin 'On The Relations Between the Sciences' (Cambridge: University Press.1968)
18. R Nisbet 'The Degradation of the Academic Dogma' (London: Heinemann 1971)
19. This is not to say that arguments from practical usefulness may not play a part in the political tactics of those seeking to advance a new field of research. There is no reference to 'tactics' in the examples discussed earlier. For the arguments used in favour of, and against, early (experimental) physiological research see R D French Antivivisection and Medical Science in Victorian Society (Princeton, University Press 1975)
The utilitarian argument is not new.
20. See A Etzioni and R Remp Technological Shortcuts to Social Change (N Y Russell Sage 1973). Examples of technological shortcuts considered are methadone in the treatment of heroin addiction, antabuse in alcoholism, breathalyzer testing, and the intrauterine device. In practice the research programmes of agencies engaged in areas with both 'technological' and social dimensions (eg health, environment) will embody elements of both research approaches. It is an (unresearched) matter of some interest to consider under what circumstances these actually represent alternative approaches, of course, tend to presuppose, and to offer, solutions in a way that social approaches frequently do not.
21. A Lichnerowitz 'Mathematic and Transdisciplinarity' in Interdisciplinarity (Paris: OECD, 1972)
22. D Robbins and R Johnston 'The Role of Cognitive and Occupational Differentiation in Scientific Controversies' (to appear in Social Studies of Science, Autumn 1976)
23. D Nelkin 'Scientists, Professional Societies and the Public Interest: the Experience of Ecologists' (unpublished paper, Cornell University 1975)
24. A Weinberg 'Science and Trans science' Minerva (1972)
25. P Weingart 'On a Sociological Theory of Scientific Change' in R D Whitley (ed) Social Processes of Scientific Development (London: Routledge, Kegan Paul; 1974)

26. Polymer science draws largely on chemistry, but also on physics, metallurgy, etc. Unfortunately the interpretation placed upon 'successful' is not made clear.

27. J Schmookler 'Economic Sources of Inventive Activity' Journal of Economic History (March 1962), 1-20

28. R Nelson 'The Simple Economics of Basic Scientific Research' Journal of Political Economy (June 1953), 297-306

29. M Nowakowska 'A model of Scientific Careers' (Warsaw, Institute of Praxiology. Unpublished paper 1974)

30. B Hessen 'The Social and Economics Roots of Newtons Principia' Science at the Crossroads (193 reprinted London: Cass 1971)

31. For a general overview see Chap 5 of S S Blume 'Toward a Political Sociology of Science' (New York: Free Press 1974) See also various treatments of alternative technology etc.

32. S Cotgrove and S Box: Science Industry and Society (London: Allen and Unwin, 1970)

33. S S Blume and R Sinclair loc cit

34. Böhme, G, W van den Daele and W Krohn 'Finalisierung der Wissenschaft' Zeitschrift für Soziologie 2 (1973) 128-144 (English version to appear in Social Science Information)

35. W van den Daele and P Weingart 'Resistance and Receptivity of Science to External Direction' (Starnberg and Bielefeld, unpublished paper, 1975)

36. S S Blume and R Sinclair loc cit

37. C Freeman 'Research and Development in Economic Capital Goods' National Institute Economic Review 34 (1965)