Toward a Science Policy for the Social Welfare Field*

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Inhalt: Die Arbeit versucht, auf die Notwendigkeit und die wichtigsten Merkmale aufmerksam zu machen, die eine institutionell verankerte, wissenschaftlich-innovatorische Anleitung der Sozialpolitik betreffen. Daß Anleitung dieser Art „notwendig“ ist, folgt aus dem Umstand, daß die Mittel, die die Regierungen für sozialwissenschaftliche Forschung und technologische Entwicklung aufwenden, zunehmend steigen, die Ergebnisse aber, die sie bewirken, bisher – wie etwa auf dem Gebiet der Erziehungsforschung – sehr bescheiden geblieben sind.


Wenn man fragt, auf welche Weise Politik auf dem hier relevanten Gebiet verbessert werden kann („Metapolitik“), ist es notwendig, sich klar zu machen, wieviel Gewicht den genannten Machtrelationen auf dieser oder jener Seite jeweils zugemessen werden soll. Die Arbeit schließt mit der Aufzählung der wahrscheinlichen, politisch kurzfristig wirkenden Verbesserungen, die aus der Neuverteilung von Forschungsmitteln folgen können, geht auf Forschungsschwerpunkte ein, empfiehlt neue Wege der Forschungsorganisation und nennt die Möglichkeiten, die Forschungs- und Innovationspolitik politisch nutzen kann.

Abstract: This paper attempts to establish the need for, and some characteristics of, an institutionalized policy of science-based innovation for the field of social welfare. ‘Need’ derives in part from the increasing sums governments are spending upon socially-oriented research and technological development, and the very slight benefits which have resulted (for example, from educational research).

Two related analytical distinctions are made: one relating to the level of academic debate, the other to that of praxis. First, one may distinguish between a sociological debate (which defines deficiencies in the provision of social welfare benefits in terms of organizational weakness or poor conceptualization, either at the policy level or the professional/practitioner level) and a debate among scientists/technologists seeking a ‘reorientation’ of science and technology from economic and defence to social objectives (which defines deficiencies in terms of inadequate ‘hardware’). At the level of praxis, a distinction between policy (change) and (change in) professional practice can be made. These two analytical distinctions are related, technological innovations (or “technological fixes”) and social restructuring offering alternative approaches to social problem-solving, based upon very different problem definitions and values. Each will be attractive to a policy-making or professional group which shares its values and assumptions: alliances thereby forming.

In discussing how policy making in this area can be improved (‘meta policy’) it is necessary to decide how much weight is to be attached to these actual power relations. The paper concludes with a discussion of feasible short-term policy improvements relating to allocation of resources to research; deciding what research to do; the location and organization of research; and the political feasibility of a research/innovation policy.

(a) Introduction

Essentially, I want to discuss the feasibility and the desirability of developing a science policy for the field of social welfare or social policy, and a good deal of what I shall have to say will be concerned with the elaboration of these various terms. But this is a difficult area and one which is only beginning to receive the attention which (as I hope I can demonstrate) it needs, and I am bound to admit that my exegesis consists of very many questions and very few answers.

I propose to orient the discussion around four major issues:

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What do we mean by a "science policy?"  
The need for such policies  
How many policies be rationalized?  
Some problems in short-term policy improvement

Although questions of definition are going to be fairly central, I am not going to attempt any definition of 'social welfare'. We all have our own implicit notions of the meaning of that term, and for present purposes they will suffice. From time to time I shall draw examples from the fields of education, health care, or social work: these are obviously included, although it can be convincingly argued that, since almost all policies have social implications, the term should have very much wider meaning.

(b) What do we mean by a Science Policy?

For some years it has been common in science policy discussions to make a distinction between 'policy for science' and 'science for policy'. The first is concerned with the promotion of basic science. One is tempted to add "for its own sake" but if this was ever true it scarcely is today. So it is more accurate to take an institutional definition, and say that 'policy for science' implies the policy debates and decisions of agencies concerned with the promotion of physics, chemistry, sociology, etc. (in the UK, the various research councils, in France the CNRS, in Germany the DFG, in Eastern European countries the Academies of Science, and so on). 'Science for policy' is the term used to describe the promotion of science relevant to some other policy or objective, and to that extent to which it may contribute to that other objective: which may be economic, military, social, agricultural or any other function of government. At a first glance, then, it may appear that our concern here is very simply delineated as 'a policy towards science relevant to social welfare'.

This could be interpreted either in epistemological terms (as a policy towards relevant or appropriate scientific disciplines) or in institutional terms (a policy towards science to be adopted by agencies concerned with social welfare). In either case, however, this simple approximation is rapidly undermined by a number of complicating factors. In the first place, it is impossible to decide in advance what are the 'appropriate' scientific disciplines. As I shall show later, improvements in social welfare, or even in a single aspect of it, like health care, may derive at the one extreme from (say) new drugs, at the other extreme, from, say, an altered method of financing health care. In other words, biomedical research at one extreme, economics at the other, may stimulate improved provision of social services. Suppose we take the other interpretation, and say that we want to discuss the policy to be adopted towards research by national social welfare agencies. On the one hand this implies an in-built conservatism: that in our discussion we cannot be concerned with the overall structures and functions of these agencies, but only with certain aspects of the work of the existing agencies. Many would find this an unacceptable limitation. But in addition there is another problem, common to both interpretations. I used the term "a policy (towards science relevant to social welfare)", and it is necessary to consider whether our desideratum really is a policy: the point is more than one of semantics.

A policy implies complete co-ordination of scientific activity planning and budgeting on the part of all relevant agencies, for our purposes in the social welfare field. In the British context, this would imply the co-ordination, in a unified policy, of the scientific activities of the Department of Health and Social Security, the Department of Education and Science, the Home Office, the Department of Employment, the Department of the Environment as well as the various Scottish Departments. In addition, since some of the work of the various basic science supporting Research Councils will inevitably have implications for government welfare activities, these could not be omitted. It seems likely that the attempt to develop a policy towards science relevant to social welfare leads one inexorably towards a policy for science. There is no necessary reason to reject such a path, which seems to me also to follow from any rigorous attempt at introducing modern budgeting techniques (planning by objective, PPBS, etc.). Socialist countries work like that, and it can be argued that many Western European countries are indeed following the same path. Centralization of that kind might imply
the creation of a ministry of science, which would inherit all the research functions of existing ministries (executive, as well as planning). This could well have the effect of reducing the relationships between research and the practical responsibilities to which it would be relevant (which would remain with the pre-existing ministries). It might on the other hand imply the creation of a ministry of science with the more limited tasks of planning and co-ordination (but not execution). Such a ministry was established in Canada in 1971 but, according to a recent analysis (AUCOIN and FRENCH 1974), it has had very little effect.

In the United Kingdom the establishment of a ministry of science, recommended a couple of years ago by a Parliamentary Select Committee, has been rejected by government. The overt reason cited was essentially that such an initiative would detach research from the governmental activities and responsibilities to which it is relevant (the point made above). However, the question of the cultural values of the ‘political administrative community’ (HECLO and WILDAVSKY 1974) cannot be ignored. In Britain we have a tradition of ministerial autonomy which is much stronger than exists in many countries: so centralization tends to be resisted.

Thus my point is that the value and feasibility of any institutionalized centralization of responsibility for formulating science policy is at least questionable, and must be seen in the light of specific political values. Though a certain kind of co-ordination (to be discussed later) is of value, the emphasis of this paper will not be on the need for centralization (that is, on the benefits of ‘a policy’ compared to ‘policies’).

In order to explain our central concern, it is useful to consider briefly the evolution of economic thinking about research. The emphasis in economic discussion has evolved in a way which somehow parallels what I have said above, and shows a recent shift which may allow us to explain our problem. Until recently, the empirical literature was concerned principally with on the one hand contribution of R&D to the growth of productivity or output at the level of the firm or industry (e.g. the work of C.FREEMAN, E. MANSFIELD, etc.), on the other with the sources of inventions, or the environment of discovery (as in J. JEWKES’ work). But, in recent years, emphasis has shifted to a concern with the process of innovation, of technological change.1 Discoveries, inventions (which may or may not be based on systematic research) are then seen as one aspect (albeit an important one) of a wider process: the process by which innovations come about. Following SCHUMPETER, most economists use the term ‘innovation’ to describe the first commercial utilization of a new technology (whether this is a new product, or a new process for making something, or a new means of organization).

Let me now suggest that we follow the economists’ path, and take as our central concern not the promotion of science relevant to social welfare, or of the discoveries which we hope will follow, but of innovation in the social welfare field. Pursuing the economic analogy, we may hypothesize that there is little practical benefit in promoting science, or even in discovery, unless we can find means of harnessing scientific activity to the process of change. We do not speak of change for its own sake, any more than the industrialist seeks technically successful but commercially disastrous new products (of which there are many examples). A successful innovation, for the industrialist, is one which finds a receptive market (i.e. for which there is a demand). In practice, research has shown that a prior assessment of this market is an important determinant of success (SPRU, 1972).

Before using this analogy as the foundation of further argument, it is perhaps necessary to say a little in defence of its validity: of the validity, that is, of identifying (economically oriented) industrial innovation with social innovation.

My own view is that there is no real difference between innovations we would call ‘industrial’ and innovations we would call ‘social’, in the sense that both could be subsumed under most of the common definitions of the term ‘innovation’. The term is taken by ROGERS and SHOEMAKER (1971), for example, as referring to a

1 A valuable account of the evolution of economic thinking and research on problems of R&D and innovation is given in (FREEMAN, forthcoming).
subjective recognition of newness (newness which may inhere in an object, a practice, or an idea), combined with the changes in attitudes and behaviour which this recognition may bring about. Nevertheless, there are differences in the way various academic disciplines tend to conceptualize innovation: for example, as a social process of changing behaviour, or as a change in economic inputs to and outputs from production. It follows that the kinds of innovations likely to seem worth studying will differ. So too will the reference systems within which innovation is seen as taking place. The economists’ unambiguous orientation to the market is not paralleled in sociological inquiry. For example, professional, medical and geographic communities may provide alternative reference systems for examining spreading commitment to the fluoridation of water supplies. In other words, the difficulties surrounding the identification of economic/industrial innovation with social innovation derive not from ambiguous use of the term innovation, but from common differences in conceptualization between the ‘economic’ and the ‘social’. There is no logical reason why all kinds of innovations should not be amenable to any one mode of conceptualization/examination, nor why we should not (cautiously) turn to the literature on industrial innovation for occasional guidance.

Of course, if we are obliged to consider the success, or significance, of any innovation (as economists typically do) then important value considerations necessarily intrude. In the social policy field, most of us would not consider it proper to judge success in terms of the economist’s notion of demand (which implies an ability to pay), but would prefer to use the more problematic term ‘need’.

(c) The need for such Policies

Though I do hope ultimately do demonstrate the value of policies towards innovation of the kind I am discussing, the first and most urgent concern is that informed debate take place over the relations between science and social policy. The present state of this debate, which may after all determine allocation of very considerable sums of public money, is at present un-

satisfactory. If I may characterize it over-crudely, and this may be more typical of the British scene than what takes place elsewhere, it seems to consist of two rather unrelated strands. First, one finds some debate between sociologists interested in social policy, over such questions as the influence of social theory on policy, the real meaning of ‘social policy’, the meaning of ‘need’, the problems of sociologists vis-à-vis the policy-maker, and so on.

Though this debate does treat the relations between knowledge/research and policy (change), its concern is essentially with sociological knowledge/research. As I shall show later, this is only one part of the spectrum of sciences potentially relevant to changing social services. The second strand of the debate, which is international, takes place principally among those concerned with the allocation of resources to the ‘hard’ sciences. It is based on an ideological commitment to the re-orientation of the scientific system from military and economic goals to social welfare goals. The OECD’s so called BROOKS Report (prepared by a Working Group under the chairmanship of Harvard’s Dean of Engineering), one of the best known statements of this commitment, is largely based upon the need to increase “productivity” in areas such as education, health care, housing etc. R&D is seen as a major potential source of “growth of productivity” in these areas, if only its potential could be tapped. Though the importance of both social and natural sciences is stressed, it is clear that a substantial contribution is expected from re-oriented engineering, biomedical sciences and so on, and that these sciences lie at the front of the group’s thinking (OECD, 1971a).

Yet these two debating clubs are actually concerned with complementary aspects of a single problem, which need to be discussed in a unified framework. It is a single problem because, as I shall suggest later, the policy changes which interest the sociologists, and the hardware innovations which appeal to the natural scientists, are complementary or alternative attempts at dealing with similar social problems.

But let me now return to my theme, our need for policies towards the utilization of science in innovation which, hopefully, will flow from
such discussion. There are a number of reasons for thinking that this need is now rather acute.

First, one can refer to the rising costs of research supposed to contribute to the alleviation of social ills, improvement of the quality of life, etc. Arguments, like that of the BROOKS Report, prove to have carried a lot of political weight. The political need for a demonstrable commitment has been such that increasing volumes (and proportions) of research funds are now being devoted to such objectives. Statistics of the distribution of government R&D funds by objective show this clearly (see Table I, referring to a number of OECD countries). These are very substantial amounts of money. Considering just health alone, they represent in the case of the USA 1.400 million dollars (1971–72), for the UK 28.6 million pounds (1971–72) for W. Germany, 195 million DM (1971), and so on. It is clearly important that the attempt be made to secure value for money in the case of expenditures of this magnitude. Remember that in these days of generally static government R&D budgets, the additional resources which this commitment to 'reorientation' demands are likely to come either at the expense of other forms of research (eg. basic research aimed principally at the advancement of knowledge) or of funds committed to the social welfare services themselves: there will inevitably be a cut somewhere else.

Second, there is evidence (though unfortunately, empirical evidence is meagre) that in the social welfare area research has in the past had astonishingly little impact. For example, a President of the American Educational Research Association, in his presidential address, estimated (1966) that of 70,000 articles and reports of educational research written in the preceding 25 years, only 70 (0.1%) had had any significant impact (BLOOM 1966). This is generally born out by an OECD study of educational research in the USA (OECD, 1971b), by Ward's study of educational research in Britain (WARD 1973). It is unlikely that other European countries have been much more successful or that the situation is much different in other social welfare fields.

(As an aside, one cannot but regret the scarcity of empirical studies of the contribution of re- search to innovations in social services. There have been a number of studies, over the past eight or so years, concerned with the extent to which, and ways in which, research has contributed to innovations of industrial importance. Methods of inquiry are rather well developed, and might profitably serve as models for study of other kinds of innovations.2) We therefore have to find ways of overcoming our seeming inability to make use of research apparently done with practical application in mind.

These are some reasons for stressing the need for a positive policy towards science and innovation in the social welfare fields: there are certainly many more. Unfortunately, there are some who would disagree and it seems to be politic to deal with a typical argument put forward in support of this opposite view. Thus it is sometimes argued that research is irrelevant in this area: all that is needed is political will, resources, and problems can be dealt with. The necessary knowledge is all available. Well, there is a little truth in this contention. Political will is a necessary (though in my view far from sufficient) condition, and there is much more knowledge (and expertise) available than policymakers ever dreamed of. It is for this, among other reasons, that I am arguing for policy towards innovation rather than simply towards the support of (more) research. An element of this policy must certainly be informational. But the argument is more fundamentally wrong. On the one hand, it is clear that we lack any real understanding of the fundamental causes of most social ills, or of their interrelations. Most of us use fashionable catch words (eg. 'cycle of deprivation') to communicate, (as a way of by-passing our lack of understanding) and with little real comprehension of what might be implied. On the other hand, to reiterate, there is no clear division between the 'hardware' of social policy (new drugs, new methods of building houses cheaply or new designs for schools) which most would accept as being R&D dependent, and the policies themselves.

2 As in a current OECD exercise: "Policies for Innovation in the Service Sector".
The Rationalization of Policies

In broad terms, therefore, our theoretical interest is in the utilization of science (social science, physical science, biological science . . . ) in the improved provision of social services: where by 'improvement' we imply an innovation which meets some newly recognized or articulated social need, or better meets an old one. Our ultimate practical concern is with a policy or policies which will promote the utilization of scientific research in that way. For purposes of analysis, it is useful to make a distinction between two kinds of innovation to which research might be relevant: the first referring to a change in professional practice (teaching methods or curriculum, use of new drugs, increasing emphasis on community-based mental health care, and so on), the second referring to a change in government policies laying down the framework within which professional services are offered (e.g. reorganization of the education system, changing eligibility for social welfare benefits, or new laws governing immigration). This is a useful distinction here, because it permits us to discuss separately the problems of optimally structuring the relations between policy-makers and researchers on the one hand, and professionals (teachers, doctors, social workers, community health workers, etc.) and researchers on the other. These are rather different problems, since the two groups (policy-makers and professionals) are very differently organized, and may have very different attitudes and motivations in respect of research. I shall take this point up at the end of the paper, when I come to consider the political feasibility of institutionalizing policies for science-based innovation in this field.

However, though it is an analytically useful distinction, I want to emphasize that it is no more than that. There is no such clear cut distinction between policy and practice. This is partly because (at least in Britain) 'policy' (which I take initially to be the instructions given government ministries by Acts of Parliament) is often rather brief and inexplicit. A great deal is left to be interpreted by the ministry concerned. One example is where the ministry is also the executive agency (as for example in the award of what in Britain are called Supplementary Benefits (HILL 1972: Chapter 4)). Discretion (or interpretation) must be exercised at all levels — from the senior levels at which instructions to local offices are formulated, to the local officers themselves, with whom the clients directly deal. It is generally true that 'policy', in the sense I have defined it, almost never offers guidelines for all eventualities. In the cases of education, or health (where those directly offering the service have a great deal of professional autonomy), it would be unthinkable, as well as impracticable, for any government specifically to describe how the professional should deal with every situation. Of course, constraints are inevitably placed upon his behaviour: corporal punishment may be forbidden, the state health care system may discourage certain costly treatments or operations, and so on. Moreover, from the point of view of the client or recipient (which is surely the most critical perspective on these matters), policy and practice will be indistinguishable. A number of sociologists thinking about these matters have even begun to doubt the social significance of 'policy' (because of its vagueness), and to suggest that the critical issue is that of how professionals interpret these vague guidelines in the light of professional ideologies (of which there may be many within a single profession: e.g. among psychotherapists).

It follows that the same lack of clear differentiation must be recognized between policy change and change in practice: whether the latter involves some economic or organizational change, or a change in technique. In other words, there is no incommensurability between, say, the statutory abolition of selective secondary education and the introduction and dissemination of programmed learning educational technologies. Strictly, technological innovations may represent one approach to dealing with many of the problems of concern to social policy: these are what ETZONI calls 'technological short cuts to social change' (ETZONI and REMP 1973) or what WEINBERG earlier called 'technological fixes' (WEINBERG 1966). Others, of course, may feel that such an approach only deals with the symptoms and that more profound social restructuring (based upon theoretical understanding of causes) is required. The various approaches to the treatment of the mentally ill are illustrative.
"Mental health professionals differ considerably in viewing mental illness primarily as a disease, as a disturbance in the functioning of the personality, or as a problem in living."

For the first group, mental illness can be treated in comparable ways to physical illness, e.g. with drugs, whereas the second group conceive of the problems of the mentally ill:

"...as repositories of behaviour and patterns of feeling which have become deeply rooted as a result of the child's social development and which persist through time."

Treatment for them will involve psychoanalytic techniques.

For a third group, what we call mental illness derives only from:

"...confusion in communication, maintenance of particular social rules, and enforcement of certain moral standards. Such theorists maintain that persons are labelled mentally ill because they fail to conform to certain social standards either because of their own unique understandings and viewpoints or because of their failure to develop certain social skills which others define as necessary."

(MECHANC 1969: 4–5)

For this group the social environment in which an individual exists is very largely responsible for what is termed his 'illness'. From such a perspective it is but a short step to the call for a fundamental re-ordering of human relationships – of society – as the only 'cure' for mental illness.

Clearly, the research deemed necessary in the interests of greater understanding of mental illness will vary very considerably according to the perspective adopted: from that directed at the development of new psychoactive drugs to that of a more social kind.

Mental health care offers an illustration of a very much more general phenomenon. In almost every area with which social policy deals, there exists a variety of problem definitions, interpretations, and postulated causalities. This variety is to be found not only among professionals (practitioners) but at the academic/research and policy levels also. (Moreover, the co-existence of these alternative perspectives is frequently subject to the uncertainties of fashion, so that as time passes one or another may be temporarily favoured.) The question of why this should be is complex. At the academic/research level a number of factors would appear relevant, for example: the important place which values occupy in social science; the multiplicity of competing paradigms or research programmes; the lack of accumulation of consensual knowledge. I want to refer particularly to the lack of demarcation of 'problem territory' even between the major social science disciplines. For most social scientists, social problems exist 'out there', open to colonization (though territorial claims are never recognized) by the most adventurous discipline. There is no reason why the anthropologist, the economist, the political scientist, the psychologist, the sociologist, should not each individually seek to explain (say)unemployment: and each expect professional acclaim (as well as extrinsic rewards) from a 'successful explanation' in terms of the concepts of his discipline. (But because the 'object' of inquiry will actually have been differently defined within each study (concealed by recourse to everyday expressions like 'unemployment'), the relationship between the anthropology, economics, ... etc. of unemployment will not usually correspond to, say, the relationship between the physics, chemistry, and technology of polymers.)

A related situation obtains at the policy level, where social problems are typically defined by the various political administrative agencies which invariably have an interest in terms of their individual responsibilities and traditional modes of intervention.

Each of these situations may be attributed to disciplinary or administrative imperialism – the tendency to maximize jurisdiction. 'Alliances' may frequently form between administrative agencies and academic groups whose perspectives and interpretations are congruent with their own. The research policy of such an agency will then, in practice reflect the existence of such alliances as well as the influences of the academic groups in the policy making machinery. We may hypothesize that the first will in some way be reflected in the kind of research commissioned, the second in the magnitude of the agency's research budget.
A science policy formulation (even if embodied in practical recommendations) which wholly disregards the power dimension of scientific organization is unlikely to have much effect. On the other hand, of course, what may be called ‘ultra-realism’ (excessive respect for an existing power structure) can easily lead to a pessimistic attitude as to the possibilities of even incremental improvement in policy. The problem is to decide the weight which current realities are to receive in any general policy formulation, and this may itself be determined by the time scale attaching to this formulation (1 year, 2 years... 10 years).

It is therefore useful, in an analysis such as this, to distinguish what may be achieved in the short term from what may only be possible after some years of gradual rationalization. In the short term, we may hope for some improvement in the way research is financed and organized (i.e. in research management) and in the procedures by which research allocations are determined. In the following section I discuss some of the problems in the way even of this limited objective. We may also hope for some initial attempt at specifying more exactly the rationale of social policies (and practices) and of the supposedly relevant research activities. From this should emerge an appreciation of the complementarity (or opposition?) of various ‘technological’ and ‘social’ approaches and research strategies, and of their foundation in alternative systems of values, rationalities, and interests.

But in the longer term, rational policy should perhaps involve the confrontation of these alternatives and the disruption of traditional ‘alliances’. Only if this is seen as feasible, and is tackled, can such fundamental science policy issues as the proper balance between health-care and other social welfare research (see Table 1), the proper balance between technologically-oriented and socially-oriented attempts at problem solving, be usefully raised. But is it feasible? Does it presuppose an unrealistically objectivity (overriding the value and interest content of policy and of research)? Does it require the intervention of some central co-ordinating body which lacks programme commitments of its own and may adopt a wholly neutral stance? Upon what criteria could such a body then base its assessments? There are no obvious answers to these questions which, nevertheless, seem to me fundamental to the development of an integrated approach to welfare-oriented research.

(e) Problems in Short-term Policy Improvement

I hope that I have now given some idea of the purposes of the policies I have in mind, and of the reasons which make them something of a necessity. I want now to deal with a number of the issues which these policies must explicitly confront in the short term: the allocation of resources to research; decisions about what research to do; and the location and organization of the research. This does not exhaust the range of issues: far from it. It is scarcely representative of the kinds of issues which a total innovation policy must confront, since questions of disseminating information, and the diffusion of the innovations will be of very great importance (see e.g. ROGERS and SHOEMAKER 1971). I want to concentrate on the research end of the overall process, whilst making clear that an innovation policy must deal with much more, as the economics literature well shows.

(i) Allocation of resources to research

How does one decide how much money to allocate to ‘relevant’ research: we have not escaped

3 A case in point may be PECK’s economic analysis of the slight impact of industrially oriented research and development upon British economic growth. He showed that the major hindrance was over-investment in aerospace R&D, at the expense of other potentially more profitable industries. Yet, the 6–7 years since this quite convincing analysis was published have seen no real shift of R&D resources. (See PECK 1968).

4 The full social (as well as private) returns on an industrial innovation can be secured as a result of its adoption by a single firm. One firm can offer the advantages of a new product, or reduced costs, to a substantial number of people: if the market works properly, and purchasers are rational, all benefit. One school, or one hospital, is of limited capacity, and does nor operate in a comparable ‘market’.
TABLE 1  Expenditure on Social Objectives as per Cent of all Government R&D Expenditure

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Notes: 1. "Public welfare" includes educational research, research on social services, etc. "Other services" includes law and order, as well as common services such as statistics cartography and meteorology.

a – 1963 data  b – 1970 data

Source: OECD: DAS/SPR 73.35

In theory, the allocation of public resources to research should depend mainly upon an assessment of its likely usefulness, or 'pay-off'. The growth of public research expenditures in most western countries during the 1950s and early 60s was largely due to the political success of the scientific community in arguing the economic usefulness of science. Economists contributed to the debate, for example, by demonstrating the tendency of private industry to underinvest in basic research (eg. NELSON 1959). Subsequent empirical economic research has shown the impossibility of estimating ex ante the profitability of a scientific advance, and so the political debate has not been transferred to the more scientific level. When we turn from economically motivated R&D to socially motivated R&D the problem is more acute, since we lack any method of even defining quantitatively what we mean by 'pay-off'. Presumably such a method must rest upon some kind of social indicators. In the absence of any quantitative
methods then, the politics of the allocation procedure must necessarily be of a 'traditional' kind.

However, there is a useful clarifying point to be made. The decisions involved in allocating resources to research will depend in an important way upon the method in which the budget is drawn up. As I indicated earlier, in some countries there exist science-budgets (budgets for all of research) whereas in others research is the budgeting responsibility of individual operating ministries. Although this distinction between complete centralization on the one hand, complete departmental autonomy on the other, is 'ideal typical' (most countries falling somewhere between) it does have analytical use. For the fact is that the kinds of comparisons implied in re-allocating resources will typically differ from one case to the other. Under a centralized system, an increase in socially-oriented R&D will almost certainly be at the expense of research devoted to other kinds of objectives. The policy-maker will look at data showing the distribution of R&D expenditure by objective (of which a part was given in Table I), and on the basis of his commitment to social welfare, will suggest that R&D resources be transferred from industrially-oriented R&D, or military R&D, or from purely basic research. But under the decentralized system, he has a rather different, perhaps more difficult decision to make. His data requirements are rather different. He will wish to know what proportion of all health expenditure, or education expenditure is being devoted to R&D and perhaps to make comparisons with other fields. He must then choose between additional research effort and (say) more hospital beds or an improved teacher:pupil ratio or more nursery school places. A simple commitment to improved social welfare is no longer enough, for this more difficult choice. Since we lack any means of estimating the likely pay-off on research there is at this time no quick way in which the science policy analyst can help the decision-maker with his dilemma, although it may be possible to improve the decision making structure so that appropriate options are considered.

(ii) Deciding what research to do

In the field of basic research funding agencies (CNRS, DFG, MRC, NSF, etc.) traditionally adopted a passive policy: they sat back and responded to the applications for finance which scientists made to them. But increasingly (in Britain, France, Germany, etc.) they are moving towards a more interventionist role, attempting to stimulate research in areas selected as of national importance and requiring greater effort.

It is more appropriate for an agency sponsoring socially-oriented research to establish formal research priorities than it is for a basic research agency. These priorities should embody assessments both of the relative importance of the areas with which the agency deals (eg. nursery schooling, education of immigrants, curriculum change, school building, problems of employment of school leavers, and so on) and of the extent to which research can be of use in each of these areas. Each of these determinants of a rational research policy is itself problematic. So far as the first in concerned, 'importance' may in part reflect the relative costs of programmes: this may indeed determine their 'viability' to policy-makers. At the same time it may reflect dissatisfaction with current practice: seen perhaps as wasteful or unsuccessful.

5 Unfortunately, data of this kind are not available for international comparison, but percentages in the social fields are known to be very low. For example, in the education field, a recent OECD report concluded: "... the proportion of the education budget devoted to R&D never exceeds 0.5% and very often represents less than 0.1%" (OECD, 1972). In the health field, the figure is probably rather larger, but not comparable with (say) defence where for countries like France, UK, R&D is of the order of 10% of all defence spending.

6 In practice I suspect the relative costs of programmes are scarcely ever reflected in research efforts in the social fields. Consider for example the field of education. The research effort can be roughly divided up by sector of education. In Britain it turns out that secondary education attracts nearly 50% of all research funds, higher education 16% and pre-school and primary education only 13%. Yet this latter area absorbs some 28% of the whole education budget. There are more glaring disparities. Further education, (ie. non-academic post-compulsory education) which is the most rapidly growing area of education, receives only 4% of research funds. See WARD (1973: 36–6).
It may reflect a dissatisfaction with current policy: this is a point which I shall take up below. The second criterion — the potential contribution of research — is likely to be exceptionally difficult to use, and in practice will probably be based upon the views of scientists. The fact is that however activist a ministry may wish to be in its research policy, it would be ill-advised not to pay considerable attention to the views of the research community (as well, of course, as to the demands of practitioners and to the increasingly articulate consumers). Given the widespread commitment of high calibre researchers to theory-derived research, one element of policy must be to motivate such talent to work in areas of practical importance. This cannot successfully be done with the stick, but must make use of the carrot.

Thus, assuming that general priority areas for research have been chosen, the determination of specific projects should be a process of negotiation: a process of compromise between the interests of the researchers and the interests of the sponsors. How difficult a process this will be will depend upon how far apart the two groups initially are. The gap is likely to be greatest when the question at issue is concerned directly with official policy, and involves the application of such sciences as sociology and politics. Administrators are rarely anxious to subject their decision-making procedures, or the assumptions underlying their policies, or the limitations of their policies, to the critical scrutiny of researchers. (In general, only when these policies have been called into question by widespread professional disaffection or by public opinion). Yet these are the very issues upon which researchers wish to focus. Whereas one group will tend to define the problem in terms of deficiencies in practice, the critical sociologist (for example) will frequently wish to focus upon the political context of professional practice. Moreover, the administrator will wish the research he commissions to be useful to him, in the framework of his job as currently defined. Consider the implications of this for the sociologist. The administrative utility of a piece of sociological research, in my view, places a number of constraints upon the theoretical perspective and methods which the sociologist can use. In the first place, his explanatory variables must in some way correspond to the factors which the administrator can manipulate: ‘policy variables’. For example, in discussing differences in educational attainment, an explanation in terms of variations in available resources is likely to be more interesting to the administrator than one in terms of parental educational levels. Secondly, administrators are likely to wish to see explanatory models of a ‘causal’ kind, rather than more ‘reflexive’ sociological accounts. Models must in some way reflect the rationality of administrative action. All this may, or may not, be in opposition to the theoretical preferences of many researchers.

In other kinds of research the differences are unlikely to be so fundamental. Nevertheless, to reiterate, within determined programme areas the specification of research projects must involve a process of mutual compromise between sponsors and researchers. This in turn requires that the sponsors have a sufficient level of professional expertise to translate their interests into research terms and to engage in fruitful dialogue with the researchers. The presence of some professional expertise within the administrative/sponsoring body is a sine qua non of an effective research/innovation policy.

There is a further problem which follows from what I have said above. The ‘mental health’ example given earlier illustrates the way in which social problems can be differently explained (there, in terms of disease, childhood experience, or social values and sanctions). It follows that the search for ways of alleviating these problems will appropriately focus, depending upon the preferred explanation, on the search for new technologies (eg. drugs) at one extreme, or on the nature of social processes at the other. The results of the former research are likely to impact mainly on professional practice, whereas research of the latter kind may have important implications for government policy or for the role of govern-

7 Or, in the extent to which the provision of a service meets the need (only) of the group to which the service was consciously directed. This is often regarded as best achieved by a survey, "considered", as VAN DEN DAELLE and WEINGART (1974) put it "the most advanced technique and achievement of the social sciences".
ment. It is almost inevitable that the policy-maker or administrator will prefer to define deficiencies in our ability to handle social problems in terms which do not affect his own role or freedom of action: that is, in terms of needed improvements in professional practice or in resource-terms. He is therefore likely to attach a much higher priority to research of the first kind than of the second: that is, he will prefer to support research designed to stimulate improved professional practice or better (more equitable? more efficient? more economic?) use of resources. Because of the way in which all of this is bound up with values and ideologies, it is hard to say that he is right or wrong. But it does follow that the academic social scientist whose perspective is quite different may have to seek financial support for his work elsewhere. Moreover, detached from the ministry, he may then have to consider other ways in which he might influence policy.

(iii) The location and organization of research

A further element in the research policy must focus upon where the research should be done: the balance between (say) work done intramurally and that contracted out to universities or (where they exist) independent research institutions. The obvious criteria in making such decisions should be that the research be well done, by competent people, and that organizational arrangements maximize the chances of the research findings being put to some use. The significance of the second criterion derives from studies of industrial innovation which demonstrate the importance of organization for the utilization of research in innovation. (I do not propose to summarize this literature, as I have recently done so elsewhere: BLUME 1974).

In practice, the policy maker is likely to make a clear distinction, if only implicitly, between research related to the effectiveness of professional practice, and that concerned with overall policy. His attitude will differ from one to the other: he is more likely to wish to support one than the other, and whereas research of the second kind may sometimes appear as a possible threat, the first kind will tend to be seen as inherently useful. So far as practice-directed research is concerned, the policy-maker is likely to want it organized in such a way as to maximize its usefulness. If sufficiently interested, he may turn to the literature on the management of industrial R&D to which I referred earlier. He will find a number of suggestions made: the necessity of involving the ‘customer’ for the research in the formulation of projects, the need subsequently to maintain close personal links between researchers and those responsible for later stages of the innovation process, and so on. Unfortunately he will soon recognize that his own position differs substantially from that of the senior industrial manager, for whom research, development, production and marketing are various parts of a single organization over which he has ultimate control. Of course, the extent to which central government can exercise control both over research and the practitioners who will eventually make use of it will differ, according to the responsibilities of government, between one social welfare field and another, and between one country and another. But control corresponding to that of the industrialist will be rare. Let me illustrate the problem by reference to one example.

In Britain the Department of Education and Science funds, directly or indirectly, a large part of all educational research. However, most of this research is carried out either in universities or in independent research institutes (such as the National Foundation for Educational Research). The organization of education is largely the responsibility of local education authorities, and the nature of the education provided is left very much to individual schools and teachers. Good managerial practice would suggest a decentralization of educational research to somehow parallel the organization of educational itself. In fact, we know that there are rather poor relations between the education researchers and the bulk of teachers, but it is hard for the central policy maker to bring the two groups together. He can do something to improve the process by which the results of research are disseminated (for example via the Inspectorate of Schools), but whilst important, this may be only a part of the battle. The fact is that there are clear limits to his ability to
implement an innovation policy. For example he may recognize that the disinterest of many teachers in research is related to the fact that most have been trained in institutions wholly lacking a research tradition. In learning to act out their professional roles, therefore, it would be unlikely that they had learned to place much value on research. If teachers must be encouraged *themselves* to take an interest in research, this may have important implications for the organization of their training: generally seen as a rather separate issue in educational policy. So far as the policy maker is concerned, then, he is likely to wish to press for the utilization of research, but to recognize the limits placed on him by the limitations on his power and responsibility, by the wider implications of a thoroughgoing innovation policy, and by the uncertain effects of any of the mechanisms available for intervening in the innovation process. On the other hand the limits on his interest in such a policy are likely to derive from the financial costs of innovation (comparable with the industrialist's concerns with the capital investment needed to 'embody' innovation); and the attitudes of professions concerned, which may possess substantial resources of political power.

Turning now to policy-directed research, the situation is rather different. First, to reiterate, the policy maker will view this with rather more anxiety.

His interest in it may be principally to legitimate decisions already taken or planned, or as a substitute for immediate action when a policy has already publicly been called into question. Sometimes he may desire an exploration of the policy options open to him in attaining a particular goal, in the light of certain (eg. economic) constraints. The policy maker will want such strategic research conducted close at hand, by people he can trust. He will be anxious to ensure that only the research findings which support his own position or policies become public knowledge, for fear of weakening the credibility of the policies. He will therefore want such research to be done either within the ministry, or within independent institutes which will not only obey this rule but will accept the restrictions on options and methods referred to earlier. His desideratum here is thus a centralization of research, in contrast to the decentralization preferred for practice-directed research.

Clearly, then, all of this cannot be divorced from the self-interest of the political or administrative decision maker, which is inevitably involved. Research is an uncertain process which may produce powerful political weapons: but for whose use? Democratic values may indicate the need for alternative centres of policy research, less tied to the self-defined needs of policy makers. But these cannot rely upon executive ministries for support.

(iv) The Political Feasibility of a Research/Innovation Policy

It is not difficult for a government ministry of health, education, social welfare, to set aside a sum of money for research and, on the advice of a few eminent scientists, to allocate it to the proposals coming forward which best meet standards of scientific excellence. But I have been arguing for something more positive: a policy designed to harness research to improvement in the quality of these services. If research funded on account of its supposed relevance for this objective is to be of any real value, then such a policy is a clear necessity. This much we can safely claim by analogy with the field of industrial innovation. But I hope I have said enough to show that this analogy cannot be pushed too far. After all, to make one further point, government's responsibilities are complex and interrelated, and not *bounded* in the way that a firm's are. If a company finds that its ability to innovate is restricted by the quality of staff in a particular area, it can usually change its recruitment policy, or train people when they arrive. But if government concludes that educational innovation is hampered by the quality of teacher training, it cannot avoid the fact that teacher training is also its responsibility — but involves many other consideration and powerful vested interests, as well as matters over which it lacks control.

If there is one question which focuses attention on the fundamental difference between the two areas — the industrial and the social — it is this:
In whose interest is an institutionalized innovation policy for social welfare provision? After all, planned change only comes about if somebody really wants it, and after a process of debate. The result of this process will depend upon the weight of argument which each side can bring to bear: but intellectual argument is simply one of the weapons available in the balancing of each side's resources of power. The fact is that in this area, the only participant with an interest in a thoroughgoing innovation policy may be the consumer of the services: whose resources of power are usually minimal. In industry the situation is quite different.

Not only must the political administrator be constantly aware of the political consequences of his actions but we, in prescribing policies for him, must be aware of the way in which self interest inevitably enters the decision-making process. For these reasons I suggested that he will often prefer to define problems by emphasizing the need for improvement in the way in which services are actually provided (ie. professional practice) rather than in terms of deficiencies in policy. (Obviously, changes in government provide an exception to this rule). His research policy will therefore be imbued with this kind of ideological bias: this is as inevitable in a policy for social welfare innovation as it is in social policy itself. It is hard to believe that it could be otherwise: that innovation policy could ever be wholly based upon objective criteria, such as those of 'good management', as might be possible in other fields.

Even accepting this, we must recognize that the administrator faces additional political difficulties. His scope for action will be limited by the extent to which it is politically feasible for government to intervene in the process by which services are given, and for example, by its relations with the service-giving (professional) group. (This is parallel to the government's inability exactly to specify any social policy.) The professions will differ in their attitudes to research, to changes in their way of acting, which will to some degree be based upon their exposure to research in training and general professional values. They may therefore welcome or resent an innovation policy designed to encourage them, or force them, to change their ways. For their part, the professions may prefer to attribute deficiencies in the provision of services to the failings of policy. At the same time political/administrative values relating to centralization/decentralization of authority within central government may determine the extent to which innovation policies specific to individual social welfare areas can be moulded into a unified whole.

In stressing these problems, these indeterminants I have not intended to produce a feeling of scepticism as to the feasibility of the supposedly rational policies advocated. I have wished rather to underline the inherent complexity of the problems. Let me therefore conclude with three positive conclusions as to what may be achieved in the short term.

1. Rationality here is a rationality of interpretation and execution, within alternative systems of values (including such aspects of self-interest as the maintenance of credibility and authority). There are therefore alternative sets of 'rationalities', depending upon different values, but permitting improvement in execution within each.  

2. Given that the political administrator will base his preferred research/innovation policy upon criteria, values, some of which can be guessed, there is a need for social service-oriented research based upon different values. For example, there must be independent and critical research founded upon interpretation of social problems in terms of fundamental deficiencies in the social structure.

3. More research on the appropriate determinants of an innovation policy (ie. on the kinds of issues I have been discussing) is needed, so that policies within alternative sets of values may be improved. For example, it would be valuable to have some understanding of the differing attitudes to research in various professional groups.

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8 The point here is analogous to that raised by philosophers of science in discussing the rationality of scientific advance within alternative and (according to some interpretations) incommensurable scientific paradigms. See TOULMIN (1972).
References


1971b: Educational Policy in the U. S. A.


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