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Other

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Norman Clark

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Norman Clark is Vice-Chancellor of Kabarak University, Nakuru, Kenya. Previously he was Professor of Environmental Studies and Director of the Graduate School of Environmental Studies at the University of Strathclyde, Glasgow, UK. **This paper is an input to the training course *"Institutional Learning And Change – A Capacity Development Approach To Exploring And Strengthening Post-Harvest Innovation Systems In South Asia." Bhubaneswar India***

I Introduction

This paper explores the economic content of science and technology policy[1]. It represents an attempt to define the topic in a way that is understandable both to the scientist and to the policy maker. In particular it argues that while resource allocation to science is often at bottom an economic question, the role of the scientific community and how it conducts itself is also of fundamental importance. In addition, however, the paper argues that systematic study in the 'social dimensions of science and technology' is unusually complex. This is so because those who concern themselves with science policy issues come from widely different disciplinary backgrounds, with differing appreciations of what constitutes legitimate scholarship, how problems may be defined and tackled, what is the most appropriate technical language for communicating ideas, and so on and so forth. The area is, therefore, essentially 'interdisciplinary' and from a pedagogic point of view at the very least, 'interdisciplinarity' is hard to handle. The problem is a twofold one. Scientists are formed in a regime of disciplinary excellence. Very often their interests (and capacities) do not go beyond this tight boundary. Policy makers on the other hand (and the problems they deal with routinely) require more subtle and rounded guidance. But they often do not know how to benefit from the knowledge that would simplify their task. Both “estates” would gain from interaction. The issue is, however, how best can this be brought about?

This paper argues that professional scientists themselves should to take on board some of the characteristics of their social science colleagues. For it will always be much more difficult for the latter to act as “knowledge brokers” for public policy towards science. The paper sets out to aid them in this quest. It starts by summarising how the need for S/T policy began, both as a necessary part of government decision making in an increasingly knowledge intensive world, and as a response to the social and intellectual enquiry that has inevitably followed. It goes on to explore the epistemological foundations of S/T policy analysis. It argues that while ultimately the policy decision has an economic content the nature of the accompanying analysis is so complex that to be successful an interdisciplinary approach must prevail. The paper ends by outlining some key points that should inform such a perspective.

II The Genesis of Science and Technology Policy

Interest in science policy analysis has arisen arguably from three separate but related forms of social demand, those emanating directly from the state, those related to scholarship and academic interest, *and* those related to popular concerns over the pervasive (and not always beneficial) impact of science on the lives of ordinary people. The aim of this section is to present an outline of these with a view to providing an initial overview of the kinds of issues which typically arise.

1. For simplicity I shall use the term 'science policy' as a shorthand for 'science and technology policy' except in cases where this is clearly inappropriate

(i) The State

It is commonplace nowadays to argue that in some general sense technological changes have had a fundamental impact upon economic growth since the industrial revolution. However, the form this impact has taken and the social relationships involved are even now understood only very imperfectly. In particular, the influence of social expenditures upon science (through R & D, scientific institutions etc.) is problematic. There is no question but that there have been significant "economic" breakthroughs as a result of systematic scientific research, but productivity advance has also occurred as a result of organisational changes, the migration of skilled manpower, induced changes, learning, luck and a variety of other social mechanisms and combinations of them.

Nevertheless, expenditures upon organised science have increased dramatically over the past 50 or so years and much of this has been funded by governments to be spent 'intramurally' through their own agencies/ministries etc. or 'extramurally' through private bodies, mainly industrial firms. Thus in 2003 the UK government, for example, planned to spend £4,200m on R & D[2]. Clearly, such high and growing levels of expenditures have involved governments in a resource allocation problem. If massive sums of money are to be spent in 'harnessing science and technology to the national interest' then criteria are required which will permit the disposition of such sums on a rational, and it is to be hoped socially optimal, basis. Typical policy questions are:

- How far should governments fund R & D?
- What mechanisms should they adopt to this end (e.g. direct subsidy, tax relief, provision of special facilities, public purchase)?
- What sectors should be given priority?
- What areas of basic science research should be funded by governments, in what proportions and through which institutions?
- How should higher education and science policy be co-ordinated?
- How should spending ministries fund intramural research (i.e. through their own, in-house' laboratories)?

These (and other) enormously complex policy questions may be grouped into three categories:

A. Questions of resource allocation *per se* within and between categories of scientific expenditure (e.g. how much should biosciences be given in relation to radio astronomy?) however defined. Policy questions of this kind are intimately connected with more general policy imperatives on the part of the state. Thus in the early post-war years R & D efforts in EU countries went primarily into nuclear space and aviation research rather than into industry or overseas development, thus reflecting more general government policy priorities. More recently there has been a shift to more direct 'economic' R & D particularly in view of problems of energy shortages and also as a result of growing competitive pressures arising from the

2. See DTI (2004) p 13.

globalised economic system.

B. A second category of questions concerns the growing science and technology content of the ordinary functions of government. Modern governments provide goods and services to the public, funded by taxes and other sources of public revenue. Much of this provision represents a 'productive act' and hence requires the services of modern technology if it is to be done efficiently, but even where it does not (e.g. in the making of policy for intervention in the private sector) there is still a need for appropriate technical advice. In the UK, 'spending ministries' make use of their own government R & D establishments to fulfil this type of need and, naturally enough, there are all manner of concomitant policy questions about what kind of role such bodies should play, how they should allocate their resources, and so on. One issue that keeps arising is the inefficiencies that result from the "balkanisation" of sectorally differentiated R&D bodies.

C. Finally, there are questions concerned with the 'administration' or 'government' of science, since evidently there is a need to explore the most suitable mechanisms for taking advice, for taking decisions and for monitoring the disbursements of funds. Indeed within the context of the UK there has been continuous debate on these matters since the Second World War. One famous example is that of the 'customer/ contractor' principle, which was advocated by the Rothschild Report in the early 1970s, and which raised all sorts of interesting science policy questions[3]. Basically the 'customer/contractor' principle was intended as a means of establishing a kind of 'market' for science and technology expenditures through which the 'customer', a government spending department, would identify research needs arising, or likely to arise, from its normal activities and request the 'contractor' (which would often be one of its own laboratories, but not necessarily so) to carry out the relevant research. The reform arose largely out of a feeling that the large and growing state science and technology infrastructure was tending to pursue autonomous research activities, which tended not to be congruent with its formal responsibilities and hence represented a form of social inefficiency. The hope was that by establishing a form of 'market mechanism', government laboratories would be 'disciplined' into social relevance; or at least things would be moved in that direction.

Quite clearly the Rothschild reforms, and the type of thinking that underlay them, raise all sorts of interesting questions about the 'economic' content of science policy decisions. In particular they show how such decisions are 'economic' in the sense that they are usually concerned with questions of resource allocation under conditions of scarcity. These are important questions since the decision to allocate £X to function A_1 means that this £X will no longer be available to support functions $A_2 \dots A_n$; i.e. function A_1 has an 'opportunity cost', namely that of *not* supporting other possible functions or projects.

One way of viewing things is to argue that the committal of resources to science and

3. For a discussion on the Rothschild experiment see Gummett (1979) chapters V and VI. The original source is Lord Rothschild, 'The Organisation and Management of Government R & D', in *A Framework for Government Research and Development*, (London, HMSO, Cmnd. 4814, 1971).

technology projects represents a form of social investment in the sense of ‘adding to the available stock of capital' and as such, at least in principle, it can be handled analytically in much the same way as any other form of investment appraisal i.e. as a problem in constrained optimisation. Thus we may define science and technology policy as being concerned with making optimal decisions with regard to the allocation and mobilisation of resources devoted to science and technology. 'Optimality' may be defined in terms of the conventional socio-economic calculus and therefore in terms of spreading investible resources so as to maximise net social gains to the community. At the bottom *that* is the decision problem and correspondingly one may categorise a basic function of science policy research as that of unravelling the complexities of any given allocation problem so that the final 'decision' is as fully informed as possible.

What then are the chief factors that make the ‘scientific’ investment decision a particularly complex one? In the *first* place the estimation of costs and benefits is not easy since it involves the calculation of resource flows and prices well into the future (investment in science is often a long-term activity). Of course similar problems are faced with all planning activities. The decision to construct a steel plant according to a given set of specifications is also a long-term one and assumptions have to be made about future levels of market demands, materials inputs, prices and so on. What gives particular complexity to the scientific investment decision is a *second* factor, namely the uncertainty that attaches to research as an activity.

This is so even by definition especially as one moves away from the ‘development’ end of the research spectrum and into the realms of basic research. Most applied research projects cannot be assessed accurately in terms of their costs and likely future outcomes. And even if they could there would still remain the problem of placing a valuation on research outcomes, which have yet to be realised in commercial terms. Put another way, the uncertainty is a double-edged one consisting first of *technological* uncertainty and secondly of *commercial* uncertainty. In agricultural development the problem is exaggerated by uncertainties associated with environmental factors.

A *third* problem is that in public scientific activity (and indeed in most research activity) it is not easy to separate ends and means in a clear and rigorous way. Thus in contrast to our steel plant, there is a necessary and continuous interchange between the ’outputs' (the results of any experiment or set of experiments) and 'inputs' (the design and execution of subsequent work). Very often the research process throws up new and unexpected results, which open up new commercial possibilities and alter (sometimes radically) the nature of the initial problem. A related point is that the nature of much publicly financed activity is such that there are a number of possible steps before the ultimate services are provided, each decision point being amenable to various types/forms of research activity. Hence one of the problems of a Rothschild-like arrangement is that the 'contractor' often has to recommend to the 'customer' what kind of research ought to be done, or at the very least needs to be associated closely with the final policy decision. Clearly it may be seen that the pressures to scientific autonomy are still there and indeed one of the chief criticisms of the customer/contractor principle is that it has not changed the research/production interface very much, merely adding layers of bureaucracy to older, more autonomous arrangements.

A final problem is that many goals of contemporary science and technology are often not directly concerned with economic output and to that extent (unlike our steel plant) investment in science and technology cannot be fitted into a conventional project appraisal analysis. For example, the allocation of research resources to defence, health or educational goals cannot be judged in productivity terms except in the rather narrow sense that these are given comparative value weights by some political authority. For each of the above reasons (and others) the setting of science and technology policy questions as 'economic' problems pure and simple is to oversimplify such questions dramatically.

(ii) Academic Interest

What I have tried to show above is that an important set of social demands for science and technology policy analysis has originated from the 'practical' needs of the state. A second set of interests can be categorised as that arising from the conceptual side - mainly the academic community which has tried over the last 20 or so years to give conceptual clarity to the impact of science on society in a rather different sense than was previously the case.

Traditionally the study 'of science' as a social/intellectual activity was very much dominated by history and philosophy and treated as sub-sets of these disciplines. Thus the history of science described the evolution of science as an intellectual activity through chronicling in detail its various discoveries and achievements, by describing the lives of famous scientists and by charting its professionalisation in terms of the evolution of its important institutions. Debates there were, as for example that between the 'internalists' and the ‘externalists',[4] but overall the treatment was disciplinary and specialised and there was no important sense in which such 'academic' discussion was expected to inform questions of national policy towards science. Similarly with the philosophy of science which moved from its original concern with the nature of matter (the study of natural philosophy) into more specialised concerns about the nature of scientific propositions and the rules governing their validity and acceptability - including those relating to experimental evidence. Again there was no (and still is not any) significant sense in which philosophers of science were expected to pronounce on science policy questions. Conversely, such questions were decided by direct recourse to the scientific community through its representative institutions (such as the UK Royal Society) or through its centres of professional activity (the universities, for example).

A number of developments were to change this state of affairs. First, as we have seen, the growing costs of science expenditures (including those on 'big science') and the resultant need for a more explicit 'science policy' on the part of government organs spilled over on to the academic community which found itself increasingly being asked to give advice on all sorts of policy issues, advice which, coming from 'specialists', could not in the nature

[4] See R. MacLeod (1977) pp. 149-95.

of things be disinterested. Secondly, however, there occurred a number of intellectual developments which gave a significant impetus to the validity of treating the study 'of science' in an interdisciplinary way.

Very important amongst these was the publication of Thomas Kuhn’s The Structure of Scientific Revolutions in 1962.[5] In this historical and empirical account of the development of science Kuhn confronted the 'profession' with a detailed critique of some widely held, if often rather implicit, views on the nature and progress of scientific knowledge. More specifically he argued that science, as socio-intellectual process was not nearly as rational as its practitioners had made it out to be, and at times approximated to religion and politics in its overt characteristics. Thus Kuhn demonstrated that scientific research and development was not in practice an ever-increasing discovery of the 'truth' about nature through the progressive use of experiment and logical reason. Rather it was revolutionary and cyclical in character with long periods in which certain scientific traditions became established and were maintained often in the face of evidence, which appeared to contradict their theoretical foundations. The famous Kuhn-Popper debate was one in which Kuhn argued that scientists did not behave in ways that Popper felt they ought to (so as to maintain standards of good science) but in fact functioned as social groups often do, subject to fashion, political intrigue and other less then cerebral factors.[6] Indeed at times Kuhn got close to maintaining that the actual development of science was, and is, as much a function of the sociology of the scientific community as it was of the experimental method. Certainly it did not obey simple rules of logic, whatever the philosophers might say.

Kuhn's work sparked off a spate of research into science as it is actually conducted, how it is professionalised and how the institutional behaviour of scientists influences matters of public policy. Since the scientific community could no longer be regarded as a homogeneous source of disinterested expertise, it was important to assess how it actually behaved if only so as to determine how far and to what extent the advice of its practitioners could be accepted. Again the important feature of a lot of this work is that it is interdisciplinary and policy-oriented in a sense that most of the earlier research never reached. This academic interest has thus served to make science policy analysis respectable.

A second ‘academic' development was that economists rediscovered 'technological change' and the act of doing so had certain significant repercussions. It is important to recognise that for nigh on 100 years the economics 'profession' treated technology as a datum to its analytical procedures, although prior to this many of the 'classical economists' (Smith, Ricardo and Marx for example) took technological changes very seriously indeed. However, from around 1870 onwards economists became concerned mainly with questions about how scarce resources were allocated in the short run (i.e. when the stock of capital was invariant) and in the long run (i.e. when the stock of capital was allowed to vary in certain ways). Thus, in the main economists were interested in

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[5] T. S. Kuhn (1970, 2nd edn).

[6] Discussed for example in I. Lakatos (1970). There are a number of interesting and readable papers in this volume.

questions such as how markets behaved and how they could be classified, what determined the prices of resources and how prices related to economic efficiency, how income was distributed, how money (and monetary institutions) functioned, what determined foreign trade patterns and so on. The concerns were functionalist and analytical within well-prescribed boundaries. In so far as governments and government policy were included, prescription has always tended to concentrate upon a small range of monetary and fiscal instruments.

One very important canon of economic analysis has been that of 'comparative statics', namely that of comparing two ‘states' of the particular system under investigation with a number of potentially relevant variables held constant and with one 'independent' entity allowed to vary. Often the two 'states' are held to represent 'social equilibria' which are stable in themselves, so that movement from one equilibrium position to another is brought about by the shift of the independent variable. Many well-known economic propositions (such as those relating to the market price of a commodity under varying conditions of supply and demand) are built up using this sort of technique and indeed the method has a great deal of value as an analytical device i.e. in clarifying thought in a very complex area of enquiry.

However, there are certain things economic analysis does not do very well and one of these is to shed much light on the process of economic growth. The major reason for this is that it is designed to deal with a very small range of variables at discrete time intervals under restrictive assumptions about social behaviour and (most importantly) technical conditions. Indeed one major drawback of economic analysis is that it often has weak empirical power i.e. in the sense of producing non-trivial propositions which can be tested and thereby enrich our knowledge of the economic environment.

In the case of economic growth much of the earlier interest centred on charting how economic systems would grow through time in terms of their important sub-aggregates (consumption, investment, incomes, foreign trade etc.) and under restrictive conditions regarding technology (labour-saving, capital-saving, neutral). But such analyses told us little or nothing about the causes of growth. Nor were they intended to. As far as most economists were concerned growth simply took place and if it was determined by anything it was by the rate of investment i.e. by the rate of addition to the stock of capital within the economic system under consideration. Conveniently enough this made it possible for economists to treat technical conditions as *exogenous* -that is as a variable not determined by the system under study - and, by extension, something that need not concern professional economists.

This tradition was placed in question by a remarkable study carried out by Robert Solow in 1956.[7] Using conventional 'comparative static' techniques, Solow attempted to provide a statistical explanation of the causes of US manufacturing growth over the period 1911-56. He concluded that only 12.5% of the observed growth of labour productivity (output per worker) over this period could be 'explained' by increments in the stock of capital, the

[7] R. Solow (1957), pp.312-20.

remaining 87.5% being a 'residual' or an unexplained 'technological change' or ‘improvement in productivity'. What was significant about this study was that a major economic magnitude (the rate of growth) could not apparently be explained by the established economic variable. It was not so much the rate of investment that was important but rather the *productivity* of investment - and that remained unexplained. Again, as with Kuhn’s study, there followed a spate of investigations designed to test Solow's results, to specify the composition of the residual and to explore more specifically the factors affecting technological changes within the firm and the industry. Again much of this work has proved to be interdisciplinary and policy-oriented and although a great deal has been done we are still far from having a full understanding of this complex social process.

(ii) More General Concerns

In addition, finally, to the state and academia it may be useful to categorise one other expression of social demand - that of popular concerns over the ’disbenefits' of modern science and technology, some of which have become more general political issues. Thus, for example, concerns over environmental pollution have concentrated around issues such as global warming. Similarly with the growing use, and abuse, of drugs in modern medicine, the apparent lack of popular control of major (and apparently irreversible) national project decisions such as those in power generation, or the seeming connection between modern technological changes and prospects for adult employment and income distribution.

Of course this 'Faustian' tension is not a new one. Bertrand Russell, writing in 1952,[8] noted that one very important consequence of 'modern science' and its 'impact on society' was the seemingly natural way that the conduct of science as a social endeavour becomes aligned to large-scale organisation, since only this way will the necessary co-ordinated mobilisation of resources on a sufficient scale take place. Similar views were later expressed by Galbraith, who argued that the social exploitation of modern science and technology requires 'bigness' especially on the part of economic units. Hence modern science and technology may have contributed substantially to social welfare but at the social cost of alienating the ordinary member of the public even further from a direct understanding of, and control over, the social forces of production. Since no one group is now able to comprehend the totality of modern production there may be a tendency to leave decision-making to 'experts' in conjunction with a bureaucratic machine which itself is only vaguely aware of the implications of its actions and which tends, often for the sake of convenience or ease of administration, to hide behind given policy paradigms. Paradoxically, therefore, while at a social level technological changes require a constantly radical approach, they bring with them the seeds of a rather pernicious authoritarianism with, in some cases, unfortunate consequences.

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[8] B. Russell (1952), chapter 2.

III Policy Analysis

Up to this point I have tried to 'define' science and technology policy in terms of a notional set of institutional and popular 'demands'. But how exactly can we define it? What is 'science policy analysis', how ‘scientific' is it, what are its characteristics and to what extent can empirical 'research' inform such analysis. The main reason for exploring these points is that there is often doubt in people’s minds on the precise standing of this sort of work and how it relates to policy-making. Although there is no clear-cut view on such questions, it is possible to set them in perspective and in particular to 'lay a few ghosts' about the role of economic analysis.

(i) Science Policy as Social Policy

To begin with I shall define science and technology policy analysis as being concerned with how and why social units commit scarce resources to science and technology, what sorts of problems arise in so doing and what sorts of improvements might be made. Defined in this way it should be clear that the subject ultimately is concerned with social choices and the social context within which these choices have to be made. I emphasise this since it is sometimes suggested that the solutions to science policy questions are 'obvious' or that they can be decided by 'great men' (often senior scientists/engineers who have made reputations within their own disciplines). What I hope this paper will show is that such solutions are not all obvious and will almost always require interdisciplinary expertise for their articulation - and usually this will involve inputs from both the natural and the social sciences. Similarly whatever may be the potential contributions of senior scientists (and these are many) such people are often not trained to handle difficult questions of social choice. Indeed one of the problems that natural scientists often face is that they do not fully understand how the social sciences work.

One of the best places to start is with Brian Easlea's interesting discussion on the ‘objectivity' of the social sciences.[9] Easlea argued that there was an essential distinction to be made between the natural and the social sciences in the sense that the latter could only be regarded as analogous to the 'applied' natural sciences and hence that the normal distinction between 'pure' and 'applied' work was not valid in such a context. He made the point in the following way. Natural scientists proceed by identifying relationships between states of nature, conceptualising these with reference to a body of accepted 'theoretical' knowledge and testing them using the experimental method. Thus they know that under specified contextual conditions the state, say T_1 will change to state, T_2, since that is what nature has shown to be true, or at least not yet false. If state T_2 is undesirable compared to another state, say T_2^1, then either the initial state T_1 *or* the contextual conditions must be altered in an appropriate way. To show how this can be done in specific instances is the function of the applied scientist or engineer.

Now the contrast with social science is interesting in the sense that there are no (or at least very few) non-trivial cases where we can say with confidence that given any social

[9] B. R. Easlea (1973) See chapter 6, which contains also a discussion about the role of 'values' in social analysis.

state S_1, nature will lead automatically to state S_2. Conversely social scientists are *applied* in the sense in which natural scientists would normally use the term i.e. they identify social problems and recommend how things might be altered so as to deal with them. This activity is called policy analysis but it does not relate to 'theory' in any way that natural scientists would understand. Moreover, the altering of 'states of nature' (our S_1) is not something that policy analysts are allowed to get involved with since these are usually political decisions and therefore, by definition, come within the jurisdiction of different people and institutions.

The essence, then, of social science and by extension science policy research is that it is a diagnostic exercise. The trained social scientist is someone who has built up a certain understanding of a given part of the ‘social universe', who identifies problems which he believes to be important (or who is commissioned to analyse problems that his sponsors regard to be important), who then tries to analyse the various social forces which have interacted to bring about these problems and to identify the major causal factors. He will then use the analysis to prescribe possible remedial policies and to suggest what the implications of these might be. However, since the world is a very complex place and since he will not in any case be responsible for policy execution, the social scientist ought not in general to be too categorical about the 'advice' given and should if possible anticipate the consequences of certain courses of policy action.

What then does this sort of activity imply? Clearly an important implication is that there is *no direct recourse to any unified ‘theory’ that* will instruct the analyst as to how to proceed. This is partly because of the inherent nature of social policy analysis as described above, but it is also due to the fact that most social problems are by definition *interdisciplinary*, and those that concern science and technology policy are very definitely so. Thus their analysis requires some elements of sociological understanding because, for example, it is necessary to understand certain features of the behaviour of the scientific community. They require similar expertise in political science because there are important power/institutional dimensions to science/technology policy decisions. Similarly, as we have seen, with economics. More often than not some detailed natural science or engineering knowledge is required because there is a need to know about the scientific activity in question or the nature of the productive activity under investigation. There is little point, for example, in studying the impact of the microprocessor if one is not willing to learn about what the device is, what forms it takes and how it is used as a building block in more complex systems.

The analyst is forced into drawing from a wide range of concepts and postulated relationships and herein, of course, lies a major difficulty since academic work is traditionally structured along disciplinary lines. Things have developed this way because it has proved the most efficient (in an 'academic' sense) method of extending the boundaries of knowledge (the research function) and to communicate a coherent body of ideas to students (the teaching function). The 'discipline' then determines the way that scholars think, the problems they regard as important, the 'language' with which they communicate, the research techniques they adopt and, ultimately, the criteria they use to judge the performance of their peers and to determine the ’ex ante' acceptability of research projects. These features then become enshrined in more general bureaucratic forms including budgetary procedures, levels of decision-making within academic institutions and grant-awarding bodies, and career structures for staff. Hence knowledge ‘about the world' is pursued and diffused in ways that are not in general consonant with practical problems faced by people and institutions who have to make decisions on matters of public policy or, indeed, people who are concerned with economic production. In a very general sense, therefore, there is a problem of 'academic alienation' and the science policy analyst is often faced with the difficulty of having to know enough about the different disciplines so as to be able to use them in the evaluation of a given issue.

Aside from difficulties of a theoretical or an interdisciplinary kind, there are often severe problems with regard to the acquisition, processing and interpretation of data. Evidence is important, of course, simply because one needs it to substantiate (or not) the validity of the propositions one makes about the issue under investigation. One cannot say much, for example, about the structure of UK R & D without a good deal of information about how much has been spent upon it, who has spent it, how the money has been allocated through time, across industrial sectors, and so on. However, even if there is apparently available evidence, it still has to be handled with great care for a number of reasons.

First, there may be inaccuracies in the raw data themselves due to poor data collection procedures and the analyst needs therefore to reassure himself about his basic material. *Secondly*, there is almost always a lack of congruence between the raw data and what one wishes to use them for. For example, one well-known aspect of R & D statistics is that they are often not a good indicator of 'innovative activity' since firms often report under this heading routine engineering activities which cannot be classified as innovative. Hence if one requires a true measure of innovative activity one may have to reprocess the original information or supplement it in some way. Another well-known example of this relates to the use of national income measures to compare relative standards of living amongst countries.

Thirdly, there will be difficulties in how information is to be used. One does not collect data simply for the sake of it (those who do are sometimes accused of 'casual empiricism') but because there is need to establish some sort of relationship between variables that are significant for the purpose in hand. Usually this will mean that the evidence will need to be 'processed' in some way, often using established statistical techniques such as correlation/regression analysis. Again there are all sorts of pitfalls in this area that require care.

Fourthly, there is a set of difficulties concerned with the *interpretation* of data. One way of looking at this is by viewing social analysis as taking place on a variety of different 'levels' of increasing generality. The higher the analytical level the more it approaches the policy question at issue but, paradoxically, the further it moves from its susceptibility to empirical research. Thus the debate between 'monetarist' and 'Keynesian' interpretations of macroeconomic instability is a very important one but is not apparently susceptible to empirical research (or at least its proponents do not seem to hold their positions because of empirical factors). Conversely it is usually possible to establish an empirical relationship between a small number of variables under given defined conditions, but this information may be only of marginal importance for wider policy questions.

A *fifth* set of problems concerns *prescription* since even if one can establish a set of propositions, which relate to the issue under consideration, the jump from what *has happened* in the past to what *will happen* in the future (given any set of policy prescriptions) is a considerable one. And this is so for very obvious reasons. Contextual conditions change. Policy instruments may prove to be imperfect mechanisms for getting the desired policies to work in the intended way. Policies may have untoward side-effects which vitiate the intended results *or* which are undesirable in themselves or whose consequences require further policy measures, and so on. There is an infinite regress of things that can go wrong and the policy analyst (and maker) is always working in an imperfect environment. Indeed nothing could be further removed from the safe, well-articulated world of natural science where there is (relatively speaking) constant and productive dialogue between 'theory' and 'experiment'.

Finally, there is the fundamental, but inescapable, problem that social analysts have, like all human beings, their own values, prejudices and belief systems that they will bring to bear on the question in hand and which will introduce an element of subjectivity into their work. How great this element is will be a function of their capacity to abstract from any entrenched position, which they might hold, and/or their readiness to concede that such a position might be mistaken in the face of evidence. Notice that this is a much more debilitating problem in the social sciences than it is in the natural sciences since the complexity of the social order and the relative lack of recourse to the experimental method make it much easier to 're-interpret' the evidence in ways that are ideologically acceptable than is the case with research in the natural sciences. However, the more analysts do this the poorer the quality of their work.

It should now be clear that policy analysis in general, and therefore science policy analysis in particular, is as much an 'art' as a 'science'. The practitioner must be prepared to be problem-focused and to accept the lack of direct theoretical guidance, to accept also the need to be interdisciplinary in approach, to have recourse to empirical evidence but to use it with great care, to be prescriptive but not categorically so and to try where possible to abstract from value positions when making judgements. Unfortunately these are very hard skills to develop.

(ii) The Role of Economic Analysis

What then can we say about the usefulness of economic analysis in this field? You will recall that one reason we have to pay attention to economic questions is because of the resource allocation implications of science policy questions. A second, and connected reason relates to concept formation. For purposes of analytical clarity it is essential to have an unambiguous grasp of the many social /institutional categories which are used in discussions of techno-economic changes and the role of science in these. Concepts such as 'Gross National Product' (GNP), 'unemployment', the 'capital goods sector', 'capital intensity', 'productivity' etc. abound in the literature and are widely used in both theoretical and empirical discussions. To the extent that their use is ambiguous the ‘tightness' of both analysis and prescription is likely to be compromised.

However, I should at this stage emphasise a number of points of reassurance to those who come from a disciplinary background in the natural sciences or the humanities and who sometimes misunderstand the nature and purpose of this sort of discussion. In particular, it is essential to grasp that the method of exposition is very much that of telling a 'story' or a 'fable' in which analysts 'abstract from reality' in a number of important respects. Thus they will confine their analysis to a very small number of social variables and explore the likely relationships between these variables *holding everything else constant*. Once they have explored the logic of their position in terms of what they know about 'human and technical conditions' they will then be in a position to ‘relax assumptions', starting with the strongest and so on down the line of reality.

Empirical testing of the resulting propositions is a major problem since the more you relax the assumptions the less amenable to statistical analysis many propositions are, mainly because they tend to become much more ambiguous. Conversely strong statements may not be testable simply because the assumptions made are unrealistic. The result is the well-known hiatus between conceptual work in economics (often heavily mathematical, relatively obscure and with little empirical value) and empirical work using relatively simple techniques to assess crude relationships - two 'styles' of activity which do not reinforce each other symbiotically in ways common to the natural sciences. In fact it is probably best to regard economic analysis not so much as a branch of ‘science' (to which it does not conform closely) but rather as a branch of 'philosophy' where the use of simple logical procedures can serve to highlight the important relationships involved. If you like, economic analysis represents a series of intellectual ‘pegs' on which to hang some sort of ordered appreciation of a very complex environment, but it is only ever a tool, to be used with great care along with many others.

Looked at in this light, economic analysis can be quite useful. Indeed, as we have said and as will be seen, it is essential in so far as much of the related theoretical literature on science and technology policy rests upon it and depends upon it in a number of respects. For example, it is difficult to discuss the influence of technological changes on employment without tracing through some of the 'compensating' price and income effects of any given change on the economic system as a whole. Similarly much of the recent literature on innovation can only be fully understood within the context of the behaviour of firms in competitive relationships with other firms. Unfortunately, to say this is only to scratch the surface of social relations, which are still only very imperfectly understood, not least because their evaluation requires also the complementary skills of other disciplines. How to weld these together productively is a problem of a different order altogether.

IV Some Concluding Remarks

Let me recap finally on the points that I have been making.

1 Science and technology policy analysis is still a relatively new academic area. It may be defined very broadly in terms of how and why social units commit resources to science and technology, what sorts of problems arise in so doing and what sorts of improvements might be made.
2 Much of the reason for its development depends upon demands on the part of the state for ‘expert assistance' in the making and monitoring of policy, although two other relevant factors are the evolution of interdisciplinary academic interest and more general popular concerns.
3 Science and technology policy should be regarded as a form of social policy and therefore its analysis is akin to that of the social sciences rather than to that of the natural sciences.
4 This means that analysis is problem-focused and interdisciplinary in the sense that no one academic discipline by itself can provide the necessary conceptual background. In particular, however, the examination of any issue will usually require complementary input from the natural sciences.
5 The gathering, processing and interpretation of empirical evidence are an essential part of science and technology policy analysis. However, satisfactory handling of empirical evidence is a subtle and difficult task involving an appreciation of the strengths and weaknesses of data in any given context as well as the ability on the part of the analyst to abstract from value positions.
6 The role of economic analysis is central to much of science and technology policy analysis partly because at bottom the issues involve the allocation of scarce resources, partly because much relevant theoretical discussion uses economic conceptualisation and partly because the analytic technique, if correctly employed, provides a useful method of sorting out the complexities of any given set of issues.
7 In this context, however, it is probably best to regard 'economics' as a branch of philosophy rather than as akin to the natural sciences. Also economics should not be regarded as the only relevant discipline in the analysis of any given issue.

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