THE COMPLEXITY OF SCIENTIFIC CHOICE: A STOCKTAKING

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I

THE questions about selection and priorities implicit in all discussions of science policy are both difficult and inescapable. They are *inescapable* for the less-developed and the industrialised countries alike: for the former, because large-scale patronage of researches irrelevant to their urgent social and economic needs is a luxury they can scarcely afford,[1] and for the latter because, once calls for expenditure on research and development exceed some $1\frac{1}{2}$ to $2\frac{1}{2}$ per cent. of the gross national product, the sums involved inevitably—and properly—become subject to political scrutiny alongside other comparable items in the national budget, such as social insurance, schools and defence. (Evidently, this point has now been reached even in the U.S.A.: witness the current cut-back in the overseas operations of the National Institutes of Health and the difficult passage to which Congress exposed the latest appropriations for the National Science Foundation.) Questions about scientific priorities are *difficult* in two distinct ways. They are difficult, first, because we are still sheerly ignorant about many of the relevant factors and relationships—*e.g.*, what long-term repercussions certain fundamental but expensive lines of research, such as neutrino physics, will eventually have on the rest of pure science, or how the results of scientific work can be most effectively translated into industrial innovations. (Part of this ignorance we can do nothing about, except wait for new insights within natural science itself: part we could remedy in time, given a systematic study of the sociological, economic and organisational questions involved.) But questions about priorities are difficult also for a second reason: because the essential problems are still to some extent *out of focus*. Though we can see, and state, many of the considerations having a significant bearing on these problems, we cannot yet put them all together so that they yield a coherent and consistent administrative doctrine. This second class of difficulties we should be able to do something about at once, since the problems they pose are essentially analytical ones. What is required in this case is not new knowledge but a clearer and crisper vision of the questions

[1] *Cf.* Dedijer, S., "Underdeveloped Science in Underdeveloped Countries", *Minerva*, II (Autumn, 1963), 1, pp. 61–81; and also Carter, C. F., "The Distribution of Scientific Effort", *Minerva*, I (Winter, 1963), 2, especially pp. 173–174, 179–180.

actually at issue in the formulation and administration of a science policy; and our first aim must therefore be to remove any fog due to ambiguities, cross-purposes or hidden assumptions.

For present purposes, I shall confine myself to this last, limited task. This will be, so to say, a stocktaking. When Edward Shils stated the aims of *Minerva* in his editorial manifesto, he declared that, " by the improvement of understanding, it [*Minerva*] hopes to make scientific and academic policy more reasonable and realistic ",[2] and already the periodical has published four important articles on the government of science: *viz.*, Michael Polanyi's " The Republic of Science: Its Political and Economic Theory ",[3] Alvin M. Weinberg's " Criteria for Scientific Choice ",[4] C. F. Carter's " The Distribution of Scientific Effort ",[5] and John Maddox's " Choice and the Scientific Community ".[6] The subject has meanwhile been very near the surface throughout the public debate about the Trend recommendations [7] and some powerful sidelights have been thrown on it in a collection of essays recently issued by the Council for Atomic Age Studies of Columbia University, New York.[8] So this is perhaps an appropriate time to review the *Minerva* debate about the central problems of scientific choice. Here I shall concentrate on three questions: (i) Is there any consistency of theme or approach in the four articles on this topic published to date? (ii) Where the different authors appear at cross-purposes, can we do anything—*e.g.*, draw any necessary distinctions—to resolve these conflicts, and so put our problems in truer proportion? (iii) If so, are these distinctions already reflected in administrative practice and in the structure of our scientific institutions, or can some corresponding element of cross-purposes be recognised in current procedures also?

II

To start with, we must pick out from the four articles in question the key ideas put forward about scientific choice—even at the cost of simplifying rich and suggestive arguments. (i) Polanyi's central doctrine is that " the body of scientists, as a whole ", constitutes a " Society of Explorers ", which he names " the Republic of Science ".

> Such a society strives towards an unknown future, which it believes to be accessible and worth achieving. In the case of scientists, the explorers

[2] *Minerva*, I (Autumn, 1962), 1, p. 5.
[3] *Minerva*, I (Autumn, 1962), 1, pp. 54–73.
[4] *Minerva*, I (Winter, 1963), 2, pp. 159–171.
[5] *Minerva*, I (Winter, 1963), 2, pp. 172–181.
[6] *Minerva*, II (Winter, 1964), 2, pp. 141–159.
[7] See, *e.g.*, Rt. Hon. Aubrey Jones, M.P., " Some Comments on Trend ", in *The Technologist*, I (January, 1964), 1, pp. 19–24, referred to below.
[8] Gilpin, Robert, & Wright, Christopher (ed.), *Scientists and National Policy-Making* (New York and London: Columbia University Press, 1964).

> strive towards a hidden reality, for the sake of intellectual satisfaction. And as they satisfy themselves, they enlighten all men and are thus helping society to fulfil its obligation towards intellectual self-improvement.[9]

Internally, the scientific community has a structure by which its intellectual values are continually cross-checked and maintained. Each scientist is " a member of a group of overlapping competences ", while " the whole of science " is " covered by chains and networks of overlapping neighbourhoods ". As a result,

> Scientific opinion is an opinion not held by any single human mind, but one which, split into thousands of fragments, is held by a multitude of individuals, each of whom endorses the other's opinion at second hand, by relying on the consensual chains which link him to all the others through a sequence of overlapping neighbourhoods.[10]

Questions about " scientific merit ", Polanyi argues, must be judged by the tribunal of working scientists forming the relevant " neighbourhood ". Science can accordingly advance only if the republic of science is left free to determine research priorities by what one may perhaps call its own " syndicalist " procedures. Only so can " this vast domain of collective creativity " be " effectively promoted and coordinated ".[11] Despite " the generous sentiments which actuate the aspiration of guiding the progress of science into socially beneficent channels ", this aspiration has been proved, in actual experience, to be " impossible and nonsensical ". Indeed, " any attempt at guiding scientific research towards a purpose other than its own is an attempt to deflect it from the advancement of science "—and a self-frustrating attempt at that, since science " can advance only by essentially unpredictable steps, pursuing problems of its own, and the practical benefits of these advances will be incidental and hence doubly unpredictable ".[12]

(ii) Maddox agrees with Polanyi that " decisions on scientific policy " should be " determined by the long-term interests of science itself ",[13] but he adds some interesting points. First, he rejects the idea that science must inevitably be fragmented into small, overlapping sub-disciplines:

> Antecedents suggest that there is a structure to the pattern of advance in the whole field of science which, though less definite than what seems to be the natural path of advance in a narrow field of science, does imply that there are advantages to be won when different branches of science march forward together and not independently . . . In other words, it is possible to think of planning what may be described as a balanced strategy for the encouragement of science.[14]

[9] *Minerva*, I (Autumn, 1962), 1, p. 72.
[10] *Ibid.*, pp. 59–60.
[11] *Ibid.*, p. 61.
[12] *Ibid.*, p. 62.
[13] II (Winter, 1964), 2, p. 142.
[14] *Ibid.*, p. 145.

Secondly, he sees that Polanyi's analysis has left one major question unanswered: *viz.*, how we are to extend into the technological field the kind of debate by which research priorities are argued out within pure science. Evidently, " it is not feasible to divide the scientific community into two parts, one of which is concerned with pure science and the other with its application " [15]: yet how—bearing in mind the commercial considerations which in this case are unavoidable—are decisions about technological merit to be arrived at? Maddox recognises only one sure method. Wise decisions, in technology as in pure science, depend on " intellectual confrontations " like those by which pure scientists argue out the merits of their projects within the academic world [16]:

> Ideally, there would be a great many more *ad hoc* committees like that appointed by the Royal Society to examine the needs of the biological sciences. This is *the only way* [my italics, S.E.T.] of reaching a balanced judgement on questions such as the importance that should be attached, in a country like Britain, to high-energy nuclear physics, space research, or oceanography . . . It is precisely by allowing to go unchallenged lines of technical development whose promise is unlikely to be fulfilled that the scientific community invites the scorn (and the parsimony) of the rest of society.[17]

True, Maddox concedes, the Royal Society's *ad hoc* Biological Research Committee is not an entirely happy precedent. It did its work 10 years too late, and even now the universities are " slow to accept the implications of the new molecular biology "; but he sees no alternative—" That, too, is a defect that might have been avoided if only there had been more open discussions of the new scientific opportunities." [18]

(iii) Weinberg adds some important refinements to a similar general view. The difficult choices (he sees) are not those within any single branch of science, but those which pit different fields of work against each other:

> The five fields I choose [for comparison] are molecular biology, high-energy physics, nuclear energy, manned-space exploration, and the behavioural sciences . . . [These fields] are incommensurable: how can one measure the merit of behavioural sciences and nuclear energy on the same scale of values? Yet the choices between scientific fields will eventually have to be made whether we like it or not. Criteria for scientific choice will be most useful only if they can be applied to seemingly incommensurable situations.[19]

To justify these invidious decisions in the public domain, Weinberg proposes three groups of criteria, which he refers to as " technological merit ", " scientific merit " and " social merit " respectively. Technological merit is an uncontroversial idea: it represents the normal balance between

[15] *Ibid.*, p. 143.
[16] *Ibid.*, p. 150.
[17] *Ibid.*, pp. 155, 158.
[18] *Ibid.*, pp. 153–154.
[19] I (Winter, 1963), p. 167.

research costs and prospective return with which the directors of all science-based industries are familiar. Scientific merit, for Weinberg, is to be measured as much by indirect repercussions as by direct promise: "that field has the most scientific merit which contributes most heavily to and illuminates most brightly its neighbouring scientific disciplines".[20] Questions of social merit, however, thrust scientific choice into the arena of politics and human values: they have to do with such things as health, food production, defence and prestige. To deserve really massive public support, a research field should rate highly on more than one of these three scales. Molecular biology (in his opinion) has all the merits, high-energy physics is currently somewhat overrated, while space-research is only masquerading as science: "If we do space-research because of prestige, then we should ask whether we get more prestige from a man on the moon than from successful control of the waterlogging problem in Pakistan's Indus Valley Basin. If we do space-research because of its military implications, we ought to say so. . ." [21] To sum up:

> As much out of prudent concern for their own survival as for any loftier motive, . . . scientists must acquire the habit of scrutinising what they do from a broader point of view than has been their custom . . . For scientists as a class to imply that science can, at this stage in human development, be made the main business of humanity is irresponsible—and, from the scientist's point of view, highly dangerous.[22]

(iv) By this point, we have already moved some way from Polanyi's uncompromising "syndicalism"; but with Carter's paper we plunge into another world. For his approach is deliberately utilitarian: "The aggregate scientific effort of a country is a function of the number of scientists and technologists (of various kinds) which it contains, and of the effectiveness of their use . . . I will assume that it is national policy to have full employment of its scientists, and to use their skill as fully as our present understanding of the 'use of skill' allows." [23] Any nation is of course at liberty—as a matter of deliberate policy—"to undertake pure research beyond what can be given a broad economic justification by its ultimate application", and it would so demonstrate that it "regarded intellectual discovery as more important than an increment of wealth. . . . In point of fact, however, it would be difficult to prove from the British record that the total scale of pure research exceeds what can be given an economic justification." [24] So Carter makes the economic criterion his starting point:

> Though this is not entirely an economic matter, it is only from economics that any guidance will at present be obtained. In other words, it is possible

[20] *Ibid.*, p. 166 (Italicised in original).
[21] *Ibid.*, p. 170.
[22] *Ibid.*, p. 171.
[23] *Ibid.*, p. 174.
[24] *Ibid.*, p. 175.

> to give some sort of an answer to the question: What kind of distribution of scientific effort will most effectively increase the flow of wealth? [25]

Applying general economic principles to the present situation of Britain (as contrasted with—say—China), Carter concludes that the country is moving inexorably into a "Swiss" or "Irish" position:

> Unless we can embody, in a substantial range of exports of goods (and services), technical qualities or designs which make them desirable . . . we shall have continually to interrupt our economic progress because of balance of payments crises.
> A starting-point for scientific policy, therefore, is the examination of what we know about the pattern of exports ten or twenty years ahead.[26]

How will such an examination work out in practice? "By asking these questions . . . it would, I think, be possible to reach some conclusions, first about applied research and development, then about the pure research which feeds into it, and finally about the forms of training needed to support this research." [27]

The general moral of Carter's discussion is thus the reverse of Polanyi's: whereas Polanyi roundly declares that "any authority which would undertake to direct the work of the scientist centrally would bring the progress of science virtually to a standstill",[28] Carter subordinates major decisions about scientific policy to a glorified N.E.D.C. or *Commissariat-Général du Plan*:

> This is an act of long-term planning . . . The plan thus drawn up is only a starting-point, for the country may wish to decide that, at the expense of the flow of material wealth, it will divert resources to satisfy pure intellectual curiosity . . . Yet a few tentative moves towards rationality may be better than leaving the disposal of a valuable resource to random and undirected influences. One hopes that this is why we have in Britain a Minister for Science.[29]

III

Thus my first question answers itself. In his approach to questions about scientific policy, an economist like Carter shares almost no common ground with a scientist like Polanyi. For him, the fundamental significance of scientific research lies in its long-term contribution to the stock of productive knowledge: the "intellectual curiosity" of the natural scientist is an object of disinterested patronage alone, a sideline into which—if the nation so decides—surplus resources may be diverted "at the expense of the flow of material wealth". From this standpoint, Polanyi's conception of a self-governing republic of science, "helping society towards intellectual

[25] *Ibid.*
[26] *Ibid.*, p. 178.
[27] *Ibid.*, p. 179.
[28] I (Autumn 1962), 1, p. 56.
[29] I (Winter 1963), 2, p. 181.

self-improvement", must appear little more than a visionary ideal, out of touch with economic realities. From Polanyi's point of view, in its turn, Carter's own account must at first glance look like a piece of short-sighted materialism, which threatens to destroy the social fabric of science, and so bring it to a halt. Admittedly, Weinberg and Maddox go a little further than Polanyi towards conceding the claims of technological merit and commercial demand; but all three scientists would—it is clear—prefer to see research priorities based very largely on "scientific merit" and the technological dividends of scientific discovery treated as uncovenanted mercies.

We have, then, an economist's view, according to which science is basically deserving of support because it is the handmaid of industrial growth; and a scientist's view, representing technology as a kind of scientific roulette in which those who plunge deepest tend to win the biggest prizes. Taking either view at its face value, one could perhaps grade scientific projects in an "order of priorities"; but the resulting orders of merit—and the science policies based on them—would presumably be quite different. What, if anything, can be done to moderate the apparent opposition between them?

The crucial step, I believe, is to recognise that *there is no single problem of scientific choice*. Questions about research priorities arise for government (to say nothing of industry) at many different points and on many different levels and there is inevitably, as a result, *a plurality both of "criteria of choice" and of "orders of merit"*. Unless some particular administrative context is specified, questions about "scientific choice" become essentially indeterminate, for it is the exigencies of the specific context which impose particular criteria of choice. (The National Economic Development Council and the Medical Research Council adopt, naturally and properly, different standpoints and different standards.) Conversely, it is the very demand for a single *overall* "order of merit" that lands discussions of science policy in confusion. When members of the Advisory Council on Scientific Policy ask themselves whether they should recommend allocating the next £10 million to C.E.R.N., to cancer research or to space studies, they are understandably flummoxed. But this (I shall argue) is not their fault. In a well-ordered administration, problems that involved "measuring the merit of incommensurable . . . scientific fields . . . on the same scale of values" [30] ought not to arise; and if they do, this is a failure of government. For the "cardinal choices" [31] in national policy are—not

[30] *Cf.* Weinberg, A. M., "Criteria for Scientific Choice", *Minerva*, I (Winter 1963), 2, p. 167.

[31] This phrase, introduced by C. P. Snow in his Godkin Lectures—*Science and Government* (Harvard University Press, 1961), p. 1—is in course of being transformed into a useful "term of art": see, *e.g.*, the Columbia symposium, *Scientists and National Policy-Making* (footnote 8 above), especially the essay by Albert Wohlstetter, pp. 174 *et seq.*

surprisingly—*political* ones, to be made at cabinet (or presidential) level; and, by the time they reach that exalted status, the points for decision will have had to be restated, so ceasing to be technical questions about scientific importance, and becoming political questions about the rival claims of health, defence, intellectual patronage, higher education and the rest.

IV

Let me argue this case concisely, and so in broad terms. (a) First, I shall criticise the false unity suggested by Maddox's phrase " the scientific community " and by Polanyi's corresponding phrase " the body of scientists, as a whole ": these exaggerate the contrast, both in qualifications and in interests, between " scientists " and " the rest ", and so help to conceal the plurality of ways in which " research " (to use a less loaded word than " science ") impinges on the national life. (b) Next, I shall contrast a few of the different ways, and administrative contexts, in which questions about research priorities and choices arise: in some contexts (as we shall see) there is great virtue in Polanyi's insistence that the republic of science must be autonomous, but in others any claim that " scientists " should be the sole judges is unsustainable. (c) Finally, I shall take up some questions about administration and organisation which arise at the points where technical issues abut on, and shade into, political ones.

How, then, are we to define " the scientific community "? There are two separate difficulties. (i) This " community " has highly blurred edges: it is (as Professor Wallace S. Sayre puts it in the Columbia symposium) " a world of uncertain boundaries "—

> Who are the members of the scientific community? Is it an open community, hospitable to all who desire to enter, or is it open only to those who meet severe tests of eligibility? More specifically, are there " hard scientists ", whose membership is taken for granted, and " soft scientists ", whose credentials are dubious? Are physicists and chemists members of the scientific community by right, while other natural scientists must submit additional claims for admission? Do all engineers qualify, or only certain types of engineers? Do doctors of medicine qualify, or only research scientists in medicine? Are social scientists full members of the scientific community? . . .[32]

Sayre himself hints that the phrase is inevitably tendentious—

> [It] may thus belong in that class of invocations, so familiar to the political process, which summon up numbers and legitimacy for a point of view by asserting that " the American people ", or " the public ", or " all informed observers ", or " the experts " demand this or that. There is nothing especially astonishing about this, since all participants in the political process indulge in the stratagem . . . but there may be grounds

[32] Gilpin, Robert, & Wright, Christopher (ed.), *op. cit.*, p. 98.

> for mild surprise that the code of science permits its extensive use by scientists either as deliberate strategy or in genuine innocence.[33]

Elsewhere in the symposium, the size of the American " scientific community " is estimated—using different indices—at anything from 1,200,000 (the totality of professional groups) down to 200 (the effective political lobby).[34] Could we do any better in Britain? I think not. For the same quandaries apply here. We need not question the " good standing " as scientists of Fellows of the Royal Society—leaving aside honorary fellows like Sir Winston Churchill; but it is notorious that " the Royal " pays too little attention to technology,[35] while positively excluding workers in the human, economic and social sciences, even from those branches which have long since earned their admission into the scientific academies of other countries.

(ii) Further, " the scientific community "—however it is defined—has an internal structure which can sometimes be of great significance: notably, an age-and-status structure. This creates practical difficulties, which Polanyi and Maddox both underestimate, when it comes to assessing " scientific opinion ". The republic of science is not, in practice, a full democracy: in its external affairs especially, it is a *gerontocracy*, and this fact causes me to wonder whether " open intellectual confrontations " and " many more *ad hoc* [Royal Society] committees " really represent (as Maddox declares) " the only way " of reaching balanced judgements about scientific priorities. It should be illuminating, for instance, to experiment with the " Delphi Method ",[36] developed by the Rand Corporation to deal with just such problems. In this technique, a panel of specialists participates in a sequence of questionnaires, through which they compare and contrast their own opinions against those of other panel-members, *without knowing their identities*: so a consensus of opinions can be achieved which is undistorted by considerations of seniority and status. Suppose, for example, separate " profiles " of expectations and priorities had been established in this way in 1952, among biologists aged from 25 to 35, and among biologists aged from 50 up. Might this not have helped to short-circuit or accelerate the procedures of the Royal Society Committee on Biological Research? Might not the cardinal importance of molecular biology have, as a result, been publicly established rather earlier than 1960?

[33] *Ibid.*, pp. 98–99.

[34] *Cf.*, *ibid.* the essays by Robert C. Wood and Christopher Wright, especially pp. 48, 273.

[35] *Cf.*, Sir Gordon Sutherland's proposal for a separate Royal Society for technology on the lines of the Swedish *Ingeniörs Vetenskaps Akademien*, reprinted from the *Guardian* in *The Technologist*, I (January 1964), 1, pp. 39–40.

[36] See, *e.g.*, Dalkey, Norman, & Helmer, Olaf, " An Experimental Application of the Delphi Method to the Use of Experts ", *Rand Corporation Memorandum*, RM-727-PR (abridged), July 1962.

V

The " scientific community " (I imply) cannot be isolated, as a single, coherent body of opinions and interests, clearly separable from those of, say, hospital nurses, architects and town-planners, industrial managers and citizens-at-large. Rather, we must begin by classifying the different ways in which research can enter into and affect the national life, as a preparation for understanding the variety of ways in which such research can interact with and depend on government. For if science is inevitably brought into contact with government at a variety of points and in a variety of ways, then " scientists " can have no *unique* standpoint *vis-à-vis* government: there will be many legitimate modes of interaction, in each of which scientists apply their minds to a different group of problems, and the needs of each such partnership will impose their own pattern of research priorities and criteria of choice.

Evidently enough, one can distinguish at least four distinct types of research: (i) pure research in the natural sciences, engaged in with no eye to utility or material productivity; (ii) speculative technology, designed to broaden the general base of man's practical capacities; (iii) product-oriented research, aimed at developing a material, machine or medicine, say, to meet a given specification; and (iv) problem-oriented research, aimed similarly at finding an acceptable solution to some practical problem in the field of, say, transport, public health or defence. Different government agencies have correspondingly varied reasons for being interested in research and for underwriting its costs. It is a general interest of any country, for instance, (i) that its intellectual and artistic institutions should maintain a healthy level of activity, through either public or private patronage. (In Britain at present, the fiscal structure provides little incentive for the " patronage of the intellect " by corporations or individuals —the " seven-year-covenant " rule is an obstacle unknown in, say, the U.S.A.—so this function falls largely on government.) Similarly, government has proper parts to play, by (ii) helping to broaden the country's base of technological skills, as an essential element in guaranteeing economic development, (iii) paying the cost of developing certain types of product—*e.g.*, aircraft—and (iv) underwriting research on ways of overcoming the major problems of modern communal life: the Buchanan inquiry into the future shape of towns [37] is a classic example of this last class.

All this may seem very obvious, if stated baldly; but it is directly relevant to the *Minerva* debate on scientific choice. For some of the contributions to that debate manifestly concentrate too exclusively on

[37] *Traffic in Towns. A Study of the Long Term Problems of Traffic in Urban Areas. Reports of the Steering Group and Working Group appointed by the Minister of Transport* (London: H.M. Stationery Office, 1963).

one particular relation between science and government. Suppose, for instance, that all government support for research properly fell under the heading of " patronage of the intellect " (category (i) above), then indeed Polanyi's doctrine might be the whole answer. Certainly, within the limits of this category, the intellectual promise of a scientific project and the significance of its outcome are matters, above all, for the scientific guild. Even there, history suggests qualifications: a few exceptional scientific discoveries have had repercussions spreading indirectly to other intellectual disciplines, and beyond—Darwin's theory of natural selection is a clear illustration. To concede Polanyi's essential point, however, it is no more the business of government to pronounce on these wider implications than on the narrower scientific merits of an inquiry. They are a matter for the broader academic confederation, of which the republic of science represents, so to speak, one " canton ".

Again, Carter's criteria of choice become relevant only when we leave category (i) behind and move into categories (ii) and (iii). (Does not Carter explicitly set aside " the satisfaction of pure intellectual curiosity " [38] as irrelevant to his discussion?) From the point of view of the National Economic Development Council, the crucial question is, indeed, into which sectors of technology Britain—with its special position and problems—should plunge most deeply, as promising the highest economic return on investment in research and development. In this discussion, the scientific guild can again make an indispensable contribution, since judgements of *feasibility* are an integral part of the task; but scientists cannot have the entire say, since the final criteria of choice must here be based on a balance of *economic* considerations, in addition to intellectual ones. As for categories (iii) and (iv): there the " customer " too must have a say, along with the scientist and the economist. Criteria of merit in pharmaceutical development, for instance, involve—or *should* involve—something more than estimates of feasibility and financial return: the interests of the citizen-at-large (*qua* potential patient) must be explicitly represented also. Finally, research into a social problem like the impact of road traffic on city life calls for wider representation still. The fruits of research in this case will be far more than merely intellectual: what is at issue is the whole environment in which our children and grandchildren will live. In the post-Buchanan debate, accordingly, architects, town-planners and citizens-at-large are all entitled to intervene; and the creation of Professor Buchanan's Department of Transport at Imperial College indicates that—at last—the solution of *social* problems is becoming recognised in this country as being a species of science or technology, as

[38] *Minerva*, I (Winter, 1963), 2, p. 181.

truly as the refinement of quantum electrodynamics or the development of thermoplastics.

So, in real life, the republic of science cannot stand apart from the general commonwealth. Back in the 1930s, Polanyi's campaign to defend the autonomy of science against projects for a Nosey-Parkerish state centralism had a real point. By the 1960s, the need for academic science to be self-governing seems to be being conceded even in Russia and Polanyi's protestations are—surely—more insistent than they need be. As the social sciences too approach their coming-of-age, his distinction between the republic of science and the rest of the community becomes excessively disjunctive. The urgent question to-day is, rather, how the self-governing republic of science is to be integrated, not only into the broader academic confederation, but into the whole community of citizens. For it is on the answer to this question that our broader criteria of scientific choice ultimately depend.

VI

If carried only so far, this analysis may read like an exercise in pure philosophy. But, if one further premise is taken into account, it begins to have practical implications in the field of public administration. For there is a general principle of organisation which holds in the administration of scientific affairs as forcibly as it does in the rest of the public service. This is the Chalk-and-Cheese Principle: namely, that the structure of departments and advisory committees should be so ordered that, at each point, decisions have to be taken *between commensurable alternatives*. In a well-structured administration, choice always involves, say, signing a contract for one fighter-bomber rather than another fighter-bomber, allowing a pension to one ex-serviceman rather than another ex-serviceman, laying down one set of guide-rules for tax exemption rather than another alternative set of guide-rules—comparing, in each case, chalk with chalk and cheese with cheese. Administration can, in fact, be efficient and equitable only if its organisation is—in this sense—*functional*: when departments or committees are regularly required to choose between chalk and cheese, their decisions are inevitably more difficult and more political.[39]

In most areas of public administration this general principle is well recognised, and organisational structures have been developed which

[39] Let me digress at this point. It is often questioned whether an undergraduate training in Oxford philosophy is a useful preparation for the civil service higher administrative grades; yet the young assistant principals who took part in a recent discussion which I led at the Treasury Centre for Administrative Studies found it entirely natural to adapt the techniques of analytical philosophy to the discussion of public administration. What I here call respecting the Chalk-and-Cheese Principle, for instance, they called—following Gilbert Ryle—"avoiding category-mistakes", and the sophistication of Oxford philosophical debate served them well in the discussion.

faithfully reflect, and discriminate, the different governmental functions to be performed. Yet there is reason to think that, in the case of scientific affairs, such an equilibium has not yet been reached. This can be illustrated by referring, briefly, to the Atomic Energy Authority in Britain and to the National Aeronautics and Space Administration in the U.S.A. To begin with the A.E.A.: from the outset its functions have been ambiguous and the activities of its stations have contributed to the nation's power, health, defence and pure science programmes—to mention only the most obvious applications. In the first wave of enthusiasm for atomic technology, it was understandable that the A.E.A. should be set up as a unitary agency with a single central budget and parliamentary vote. But the time seems now to have come when government-sponsored atomic research might alternatively be budgeted for through a number of different channels, according to function. All these activities pre-empt large funds and highly-qualified staff, which might be being employed in other directions instead. It is, accordingly, arguable that the pure physics done under the A.E.A. wing stand—first and foremost—to be weighed in a scale against the pure physics done at universities, the atomic power programme against other work on the generation of power, the work on medical isotopes against other medical research, etc. For each of these sectors of work must, in the last instance, be judged *politically* as elements in the overall governmental outlay on pure science, fuel and power, etc.

Similar comments apply to N.A.S.A. On the one hand, N.A.S.A. initiates and supports a complex of activities which impinge on the national life of the U.S.A. in a great variety of ways: it plays a part in weapons development, engages in meteorological surveys, finances research both in universities and in its own scientific laboratories, fosters much more-or-less speculative technology, and maintains the newly-renamed "aerospace" industry in the manner of life to which it wishes to remain accustomed. N.A.S.A.'s functions, that is to say, are *multiple*. Yet it is funded by a *unitary* appropriation and, despite Congressional scrutiny, the final decision how this fund shall be divided between the different functions is—inevitably—taken within N.A.S.A. itself. In this way, N.A.S.A. has been given both the authority and the means to alter substantially the national division of effort as between (say) industrial development, defence and scientific research. Instead of being a purely administrative agency, it is to that extent a political agency taking political decisions: a "state within the state", to which Congress has delegated some of its powers under the Constitution.

To say this is not, of course, to *attack* N.A.S.A. The point at issue is not the manner in which the agency has in fact exercised these delegated

powers: it is the constitutional question, whether such a delegation of political authority by Congress to a largely autonomous agency best serves the national interest. Or would it be better if the multiplicity of N.A.S.A.'s functions were paralleled by a multiplicity in the sources of its funds? Is there not a case, as with the British A.E.A., for balancing each of N.A.S.A.'s major programmes against similar programmes maintained elsewhere from other sources as parts of the overall federal effort in the fields of (say) defence or technology or pure science?

Corresponding ambiguities arise, in Britain, over the present status of the Advisory Council on Scientific Policy. Members of the council speak with feeling about the " incommensurable alternatives " between which they are required to choose. Should they, for instance, recommend spending more on particle physics at the expense of cancer research, or *vice versa*? The Chalk-and-Cheese Principle prompts one to ask whether the A.C.S.P. is in fact being *asked the right questions*. True: a phrase like " cancer research " may cover anything from clinical trials to basic cell-physiology and this kind of ambiguity helps to conceal the problem. For the A.C.S.P. may quite fairly be asked to say whether, as a matter of pure intellectual patronage, there is a fair balance in the scale of government support as between (*e.g.*) particle physics and basic cell-physiology. But if " cancer research " is taken in a wider sense, it becomes a genuinely *medical* matter which should be weighed, not against the government's support for pure science, but as part of its overall provision for expenditure on health. So understood, the choice between particle physics and cancer research becomes a decision whether to allocate more funds (a) to the patronage of the intellect or (b) to improving the nation's health. *This is not a technical choice, but a political one.* As such, it should be taken by the cabinet on its own responsibility, not referred to a scientific committee like the A.C.S.P. The most any advisory council can do in this case is to appraise the likelihood of getting tangible results in the cancer field by a given technique and for a given outlay—after that, the choice is up to the government itself.

In this respect, the positions in the Washington hierarchy of the President's Science Advisory Committee and the Office of Science and Technology are less ambiguous. Instead of being attached to one of many executive departments, P.S.A.C. and O.S.T. are White House bodies, responsible directly to the Chief Executive and his Cabinet.[40] They are

[40] There has been some sharp criticism recently of the science advisory machinery in Washington—see, for instance, Abelson, P. H., " A Critical Appraisal of Government Research Policy ", *Robert A. Welch Foundation Research Bulletin No. 14*: Houston, Texas (November, 1963). However, in considering this debate, one must take care to distinguish questions about the personal position of Dr. Jerome Wiesner in the Kennedy administration from questions about the institutional merits and defects of

accordingly in a position to answer the technical questions bearing on all issues of national policy, whether these fall within the area of defence, industrial development, transport, education, intellectual patronage, or anything else; and their advice serves, not to *supplant* the ultimate political decisions, but to make them *better-informed*.

Unfortunately, the British A.C.S.P. has been answerable, not to the Cabinet, but to the Lord President of the Council in his role as Minister for Science. Its brief has been to watch over the functioning of the research councils and grant-giving agencies through which the government exercises its function as patron of scientific research, so as to ensure that " science-as-a-whole " is getting the right kind of support. Yet, on the argument of the present paper, the idea of " science-as-a-whole " has the same defects as the idea of " the scientific community ": one can no more separate off " science " from the rest of the national life than one can separate " scientists " from the rest of the population. More and more, the contributions of research to the welfare of the community are overlapping into spheres of the national life far removed from the pure patronage of intellectual inquiry. This fact is *both* the justification for the vast increase in public funds being devoted to research today *and* the reason why the older systems of organisation and budgeting—dating back to the Haldane Report of 1918[41]—are breaking down. Research on health, housing, transport, criminology and the rest can today be regarded as integral parts of the nation's overall activities in the fields of medicine, housing, transport, crime-prevention, etc.: the pre-1939 conception of the Lord President's Office, as the principal channel of patronage for research of all kinds, has outlived its usefulness.

VII

Though I started from a very different point, I end up in a position very close to that expressed in a recent article by Aubrey Jones:

> The Haldane Committee was perfectly right to point to the desirability of research both within administrative departments and outside them. I incline to the view, however, that the Trend Report exaggerated when it says that the Haldane Committee laid down a principle about the doing of research outside administrative departments. I question whether there is any principle involved. The doing of research by external agencies is a

the P.S.A.C. system itself. Evidently, the longstanding and extremely close personal relationship betwen Wiesner and John F. Kennedy gave rise among other scientists to tensions and suspicions resembling those provoked in Britain by the somewhat similar relationship between Churchill and Cherwell. It is interesting, as a result, to compare Dr. Abelson's attack on Wiesner's position with C. P. Snow's famous polemic against Cherwell, in his Godkin lectures on *Science and Government* (Harvard University Press, 1961).

[41] *Report of the Machinery of Government Committee.* Chairman: Lord Haldane. Cd. 9230 (London: H.M. Stationery Office, 1918).

> convenient administrative device for certain purposes at certain moments of time. But the greater the use of external agencies the more certain it is that the research will be distant from the purposes of the departments affected. . . I believe that some forms of research, and notably industrial research, should be moved closer to the departments affected.[42]

Still, this statement of the problem perhaps needs to be qualified. The quandary left partly unresolved by the Trend Committee[43] is, whether research laboratories wholly supported by government (such as the Road Research Laboratory) should be " under " the Department of Scientific and Industrial Research or " under " the department most closely affected (the Ministry of Transport, say). British traditions being what they are, it is commonly taken for granted that the department on whose parliamentary vote such a laboratory is carried must be given administrative control over it: " Who pays the piper calls the tune." Yet here again Britain may perhaps learn from American experience. For this administrative quandary reflects a novel conflict of considerations, of a kind which is likely to become more frequent rather than less. In the organisation of scientific research, the natural unit is the *technique*—nuclear reactors, rockets, lasers, computers, games-theory or whatever. One and the same technique may well contribute to the national life in half-a-dozen ways and so interest several government departments. The examples of the A.E.A. and N.A.S.A. posed the question, whether this unity of technique requires a unitary *budget*: in the present case the question is, rather, whether a unitary budget requires *administrative* integration into the user-department. Must a fully-supported research organisation always be administratively " under " some particular ministry?

The problem of combining scientific techniques, administrative flexibility and financial responsibility is solved in a new way in the " research contract " system, which has been exploited far more fully in the U.S.A. than in Britain. Research organisations such as the Rand Corporation and the Stanford Research Institute could scarcely survive without government finance; yet administratively they are independent entities, free to develop the techniques around which their activities revolve as they see fit, and able to " sell " their services to different agencies within government proper. While preserving the unity of technique which was the original *raison d'être* of the A.E.A. and N.A.S.A., they differ from those agencies in drawing their funds from a plurality of sources.[44] Finance reflects function,

[42] Jones, Rt. Hon. Aubrey, P.C., M.P., " Some Comments on Trend ", *The Technologist*, I (January 1964), 1, p. 24.

[43] *Committee of Enquiry into the Organisation of Civil Science*. Chairman: Sir Burke Trend. Cmnd. 2171. (London: H.M.S.O., 1963).

[44] On the development of the research contract system, and related project and grant systems, much fascinating material is to be found in the recent Kistiakowsky Report: *Federal Support of Basic Research in Institutions of Higher Learning* (Washington: National Academy of Sciences, 1964).

organisation reflects technique: while serving many departments, they are integral parts of none. This pattern has, up to now, been followed in Britain scarcely at all.

No doubt the research contract system would need some modification before it could successfully be transplanted into the British environment. Yet, at the very least, its merits and shortcomings deserve to be considered with some care. For this is one of the many new models of organisation which must be borne in mind, as the administration of Britain progressively adapts itself to the task of supervising all those complex and manifold functions which research—physical, biological, technological and social research alike—performs in a modern community.