Thoughts on Science Policy

Priorities in Research. JOHN KENDREW, symposium chairman. JULIAN H. SHELLEY, editor. Excerpta Medica, Amsterdam, 1983 (U.S. distributor, Elsevier, New York). xxvi, 191 pp. $54. From a symposium, Kronberg, Taunus, West Germany, May 1982.

Science policy is about establishing priorities for research. Which of the many projects that might be undertaken should be supported? Some of the most experienced academic, governmental, and industrial scientific managers in the world discussed this question for three days at the fourth Boehringer Ingelheim symposium. These proceedings of the symposium bring to the surface the issues that really trouble the policy-makers, even if they do not tell us how those issues should be resolved. Many themes are taken up, although none is examined very deeply, for the most instructive thoughts often occur as remarks in the wide-ranging discussions of the relatively brief papers introducing each session. But the participants are among the wisest and wiliest old birds in the business, and what they sing in chorus is worth listening to.

They have little time for highly rational plans for science at the national level, where politics and pragmatism rule. They do not know any formula for deciding between, say, biotechnology and high energy physics and are skeptical of the capacity of even the most intelligent of expert committees to do so. But they are very conscious of the sensitivity of such decisions to the administrative framework in which they are made. Among the most interesting passages in this book are the brief summaries of different ways of organizing the financial support of research in different countries. In Germany, for example, there is a policy of giving at least token support to a large proportion of grant applicants and defending small projects from the insatiability of big science installations and elaborate programs. Even the French contributor played down the role of bureaucratic structures, and there was general agreement that it is best not to have budgets for individual projects within a single institution but to rely upon managerial skill to achieve a fair and flexible distribution of resources.

Can an interdisciplinary committee arrive at a good overall plan for research? Alas, each specialist is too deeply entrenched in his or her own neck of the woods and cannot appreciate the shape of the whole. Panel discussions round the table are more reliable than opinions collected by correspondence, but only the experience of working closely together toward a larger goal can make scientific specialists see each other's point of view.

Science policy thus comes to depend heavily on the decisions that scientists take concerning the direction of their own research. But the motives of individuals are diverse, and they do not necessarily add up to the greatest good for science or for society. Different sectors of the research system may need different priorities and strategies, even where the objective is simply basic understanding. Every research program involves personal and institutional interests that may not be apparent in its outward formulation. In fact, there is practical wisdom in the policy of not questioning too closely the motives of individual scientists, provided that the projects they undertake for career motives are reasonably advantageous to the nation or the firm.

Is sheer individualism still the name of the scientific game? Certainly it is essential to identify the talented players early in their careers and to give them openings to display their skills to the full. That could mean a ruthless policy of pruning out the mediocre long before the normal age of retirement: should they be simply made redundant, to shift for themselves, or can they be redeployed within the scientific enterprise? As it happened, there didn't seem to be any mediocre scientists at this symposium to advise on this particular point.

But there were a few historians of science and other social scientists present, and they remarked most forcibly on the asymmetry between the elaborate prior assessment that research projects now have to undergo and the perfunctory evaluation of their outcomes. They could have said more. They could have asked these practitioners of science policy whether they had really convincing grounds for many of their assertions. They could have asked how it is that research systems in different countries, with very different administrative and cultural backgrounds, often get just as good results. They could have asked—indeed, a distinguished scientist did ask—whether the notion of success in research is not somehow circular, with nothing except itself to measure up against. They could have asked, as R. K. Merton did many years ago, whether it is true that some scientists are vastly more talented than others by their very nature, or whether a high reputation could be accumulated unduly after an early lucky break.

Or, rather, more humbly, they might simply ask themselves whether the notions of these very experienced and perceptive practitioners concerning the nature of their craft may not suggest even deeper ideas that are not far from the truth after all. A regrettable stance among social scientists is to distance themselves from such "folk" knowledge and thus to cut themselves off from a potential source of inspiration. That is not to say that all the opinions of the notables are sound; they are often naïve and self-serving. But they are founded on many episodes in the real world that are difficult to observe and impossible to reproduce in the laboratory.

This book does not, therefore, convey any distinct message to anyone who wants to assign priorities to some research that happens to be around the place; that is a craft that he or she will simply have to learn by experience. But it could be a valuable resource for the sociologist of science on the lookout for evidence on how the research process actually works or what questions might throw some further light on this complex subject if they were investigated more fully.

There are also a couple of pellucid thoughts, worth quoting:

> I always say that applied research does not exist and that it is sloppy thinking and sloppy grammar. I can distil whisky, I can drink whisky. I can distil drinkable whisky. I cannot distil consumed whisky. And so I cannot do applied research either. I can apply research, certainly. I can do applicable research. I cannot occupy myself with applied research.—H. B. G. CASIMIR

> The R&D expenditure in companies is not an investment . . . in which you are entitled to expect a return. It has more the dimensions of gambling than investment. And like any other kind of gambling it should be money which you can afford to lose.—JAMES BLACK

JOHN M. ZIMAN
Department of Social and Economic Studies, Imperial College of Science and Technology, London SW7 2PG