BOOK REVIEWS

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SCIENCE, TIGHT BUDGETS, AND SOCIETY

Title Criteria for Scientific Development: Public Policy and National Goals

Editor Edward Shils

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Reviewer Paul P. Craig

In 1962 a new journal, *Minerva*, began publication in England. The expressed objective was, "by the improvement of understanding, . . . to make scientific and academic policy more reasonable and realistic." The development of big science, and with it the growth of our present technological way of life, has raised a new area of inquiry related to the manner in which scientific progress occurs, the way in which priorities are established, and the interaction of science with the world. The issues are complex, and their discussion is in an early stage.

Although *Minerva* is but little known in the general scientific community, its pages have served as the common meeting ground for much of the best discussion of these problems. The appearance of this volume, consisting of selections from the first half-decade of the magazine, is particularly appropriate at this time of present and impending financial stress within the scientific community. This stress arises in large measure from a national reappraisal of the role science is to play in society. The outcome of this reappraisal is as yet unknown, but will certainly be influenced by the role scientists and technologists play in clarifying their contributions to society. It is toward this general question that *Minerva* has addressed itself.

The scientific community made its first major contact with the external world through the development of the atomic bomb. Its first political action lead to the formation of the USAEC. There now exists a sizable group of scientists and technologists with considerable political awareness and expertise. Despite this reservoir of practical experience, the theory of science-society interaction is far behind its practice, and the extent of the disparity becomes abundantly clear upon reading this volume. No one really knows how national science priorities should be established. No one knows how, in times of limited budgets, one best decides how to apportion support between, say, biology and radio astronomy. The questions are with us, and somehow decisions are going to be made. The scientific community may involve itself in the establishment of priorities, or it may permit others to make decisions for it. It behooves the conscientious scientist to concern himself with these questions, and in these papers in *Minerva* the beginning of an approach to these problems is made. The discussion goes forward on a number of fronts. I will enumerate some of these, but elaborate upon only a few.

The individual research scientist must decide in which direction he will direct his effort. In the absence of financial pressures, his choice of research problems is usually based upon his evaluation of the interest of a particular problem to the scientific community, and his assessment of its relevance, as well as upon his particular areas of competence. The manner in which these choices are made is analyzed by Michael Polanyi. Having made his choices over a period of time, the scientist produces research, and develops a scientific reputation. This reputation is directly apparent only to his colleagues working in the same or closely related fields, but the scientific community has developed techniques by which reputations can be compared between widely disparate disciplines. Polanyi discusses the process by which this evaluation system operates in terms of a "neighborhood concept," by which each scientist is capable of evaluating not only his own field, but neighboring fields as well. By an analytic continuation process, eventually all of science can be intercompared. He speculates upon whether such a process could be developed to evaluate nonscientific areas, such as business or bureaucracy.

The success of such an evaluation system relates to the universality of scientific standards, and to the existence of an accepted body of knowledge at any given instant in time. Occasionally assertions are made which appear to the nonscientific community to be scientific hypotheses, but which the scientific community refuses to discuss. Polanyi discusses in this context the celebrated case of Emmanual Velikovsky whose book, *Worlds in Collision*, became a best seller, although the scientific community ignored it. The book interprets a wide range of natural phenomena and disasters over a period of many centuries as arising from the repeated passage of the earth through the tail of a comet. This comet is asserted to have eventually collided with Mars, its head becoming transformed into the planet Venus. Velikovsky predicted in 1950 that Venus was hot and had a hydrocarbon laden atmosphere. Even upon direct confirmation of this striking prediction in 1963 by the space probe Mariner II, the scientific community refused to take Velikovsky seriously. Wasn't the scientific community unobjective in this case, in refusing even to look at the evidence?

Apparently at each instant in time, there are areas of knowledge which are held sacrosanct and which are not considered suitable subjects for investigation. This often occurs when problems are too complex to be amenable to attack with the tools available. It can also occur as a result of undue acceptance of certain conventional wisdom. The research scientist chooses his problems with care, recognizing that his research must prove acceptable to his peers if his reputation is to be made or maintained. He is usually unwilling to spend his valuable and limited time on speculative projects. The criteria he uses have yet to be well defined, but it is at least certain that the successful scientist is able to choose problems just sufficiently in front of the present-day limitations of knowledge that he can obtain solutions, and that judgment in choosing relevant problems is at least as important to his success as his facility in solving the problems he has selected.

The scientist does not operate apart from his society. Society chooses to support or not to support scientific investigations and exerts strong influence upon the particular directions of scientific and technological effort. In cheap areas of research, these restraints are minimal. As science utilizes an increasing proportion of the national product, the restraints operate more directly. As the editor of *Minerva* phrases it, "Scarcity in an epoch in which rationality and efficiency have to a much greater extent than heretofore become the criteria for the assessment of policy and performance, imposes the notion of priority." How, then, are priorities to be established?

This problem is discussed from a very practical point of view, and with emphasis upon the particular problems faced by Britain today, by C. F. Carter. In his view, economic factors are controlling. Britain today has limited resources. It must support itself by exports, and science must be oriented toward competition. "Given that we are no longer willing to work harder than the people of other advanced nations, we can only hope to work more effectively." This view of science bears little relevance to the U.S.A., and the support of basic science is defended upon rather different grounds in articles by Alvin M. Weinberg, Simon Rottenberg, and Stephen Toulmin.

Rottenberg uses the language of economic theory to investigate basic research as a productive investment or, alternatively, as a consumption good. Weinberg argues in a similar vein. Basic science can be considered, on the one hand, as something of value in its own right—as a "branch of high culture" to be supported in the manner that a patron of the arts supports a composer—and, on the other hand, as a producer of technological fallout that demands support of basic science even when no direct applications are apparent. Toulmin feels science is emerging as a "tertiary industry," an industry whose purpose is to fill leisure time and provide for the good life. Since present-day society is unlikely to support basic science strictly for its cultural value, the pragmatic approach is to accept reality and emphasize its usefulness. Weinberg feels that the expense of much basic science makes it difficult to justify directly, and one does best by considering these expenses as being "an overhead charge on applied science and technology." This attitude is already embodied to a large degree in federal support for basic research. In 1954 in Executive Order 10521 relating to the extablishment of the National Science Foundation, President Eisenhower stated "the conduct and support by other federal agencies of basic research in areas which are closely related to their missions is recognized as important and desirable, especially in response to current national needs, and shall continue."

Regardless of the reasoning behind one's conclusion that society ought to support science, the fact of this conclusion begs the next question: How much science? B. R. Williams analyzes economic growth in several countries, and in various industries, and attempts to correlate this growth with investment in science. The analysis leads, as all scientists know it must, to the conclusion that investment in science pays off. However, when one attempts to become more specific and to ask how one best apportions his resources to maximize the payoff, hard conclusions become almost impossible. British research and development consumes $\sim 2\frac{1}{2}\%$ of the gross national product, compared to just over 1% in Germany. If one deducts expenditures for defense and adds investments in foreign technology, the investments become about equal. But Germany uses a higher proportion of its scientists and engineers outside of research and development than does Britain. Is this policy responsible for the rapid postwar economic growth of Germany and the slow growth in England? One cannot be sure. What guidelines are relevant to the U.S.A.? The problem of optimal distribution of scientific manpower remains a central one for the future.

Alvin Weinberg, the Director of Oak Ridge National Laboratory, has not been reluctant to deal with certain aspects of this question of allocation of resources. He has been an outspoken proponent of a policy of bringing major national facilities to bear upon major national problems. Having reached conclusions, he has not been hesitant to act. The results are apparent in the exciting new programs at Oak Ridge, among them the MAN program in molecular anatomy, the desalinization program utilizing nuclear reactors, and the extension of a fallout shelter design program into urban renewal and the design of cities. Some of his most penetrating thinking is included in three articles reprinted from *Minerva*. To me, these articles represent the high points of the book and offer sufficient reason for any person concerned with the development of science to have it on his bookshelf.

Weinberg identifies three factors that he feels must be weighed in deciding whether a particular area of research is to be pursued: scientific merit, technological merit, and social merit. In the case of basic science, "two internal criteria can be easily identified: 1) Is the field ready for exploitation? 2) Are the scientists in the field really competent?" But internal criteria alone are insufficient. Science does not exist in a vacuum. Large-scale support of a scientific program is justifiable only when external criteria for merit are also satisfied. "The *value* of science cannot be determined from within science. It is a venerable philosophic principle that the value of any universe of discourse must be judged from outside that universe of discourse. . . . The answer to the question: does this broad field of research have any scientific merit? cannot be answered from within the field." Weinberg proposes a criterion: "Other things being equal, that field has the most scientific merit which contributes most heavily to and illuminates most brightly neighboring scientific disciplines." Here is a principle with which many will take issue.

The technological factors are easier to deal with. One must assess the state of technology. Is it ripe for exploitation? Are the people in the area competent? Is the project likely to succeed? In this country we have considerable expertise in evaluating technological factors. The most difficult judgment is social merit. Social values are hard to define. They include national defense, health, national prestige, and food. Concensus on the relative weights to be assigned to various proposed projects is difficult to achieve. Nonetheless, Weinberg feels, some qualitative evaluations are possible.

In an effort to apply these criteria to the real world, Weinberg analyzes and compares five scientific and technical fields: molecular biology, high-energy physics, nuclear energy, manned space exploration, and the behavioral sciences. I will summarize his comments on two of these. High energy physics rates superbly on scientific merit. It has many interesting problems and the best people. Yet in its relationship to the development of technology it is mediocre, and on social merit its rating is poor. These two low grades would be acceptable if high energy physics were cheap. But it is not. However, if high energy phsics were to contribute to international cooperation, say, by a joint East-West accelerator, its overall rating could be greatly raised.

At an opposite extreme is molecular biology. Weinberg devotes an entire article to the support of his belief that "of all the sciences now supported by our society, biomedical science ought to stand first." Problems exist in abundance. They are being attacked by competent people, and are being solved. The prospects of social returns in the near future are excellent. The costs of biomedical science are spiraling as more and more sophisticated apparatus is developed. It is time for major investment in this area. The approach most likely to prove fruitful is that of the major research institute, in which interdisciplinary interaction is the rule rather than the exception. Weinberg approvingly quotes Professor Peter Rossi: "the social ecology of the university is not as well suited to a massive attack aimed at a single goal as is the ecology of the research institute" [Researchers, Scholars and Policy Makers: The Politics of Large Scale Research, *Daedalus*, LXLIII, 4, 1142 (1964)]. In a research institute the whole is much more than the sum of its parts, and a competent individual can exert far more influence than he can in an isolated environment. Molecular biology receives top marks for all of Weinberg's criteria.

The previous discussion has been devoted to the operation of science within the developed, industrialized countries. Far different problems exist in the underdeveloped nations. Several articles are devoted to the elucidation of the nature of these problems and to methods by which they might be alleviated. Of particular interest are a number of eminently practical suggestions proposed by Michael J. Moravcsic. He is concerned about the brain drain, the problem of admission of students from underdeveloped nations into graduate schools, and the maintenance of continuing productivity when communications are difficult. An international roving team of technicians is proposed as a means for keeping complex scientific gear operative. It is suggested that a team of interviewers travel through underdeveloped areas interviewing graduate school applicants in order to apply uniform standards and minimize the traditional problem that Eastern applicants arrive with superb letters of recommendation, regardless of their true competence. None of Moravcsic's proposals are expensive or impractical, and it is to be hoped that some of them will be put into practice.

Criteria for Scientific Development is rich in ideas. It is time that the dialog taking place in *Minerva* be brought to a wider audience. Publication of this reprint volume is a step in that direction. An index facilitates following concepts from one article to another and retrieving thoughts remembered only vaguely. The omission of biographical data on the authors is unfortunate, especially since many are British and their names are unfamiliar in this country.

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COMPREHENSIVE COMPOSITION ON COMPACTION

Title Vibratory Compacting—Principles and Methods

Editors H. H. Hausner, P. K. Johnson, and K. H. Roll

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Reviewer Harry M. Ferrari

Vibratory compaction has become an important commercial process for fabricating ceramic nuclear fuel and reactivity control elements. Although many articles have been written on this subject, the information is diffused over several hundred publications encompassing many diverse fields. This book, the first one entirely devoted to vibratory compaction, will serve as a useful depository of some of the more important information.

The book is basically a compilation of four loosely related studies by several contributors. Over two-thirds of the book is a translation of a detailed Russian study of the