A Managerial View of Research

Scientific Productivity. The Effectiveness of Research Groups in Six Countries. FRANK M. ANDREWS, Ed. Cambridge University Press, New York, and Unesco, Paris, 1979. xxxiv, 470 pp., illus. $24.95.

This book reports the results of an international comparative study, sponsored and coordinated by Unesco, of the performance effectiveness of scientific research. Six European countries (Austria, Belgium, Finland, Hungary, Poland, and Sweden) took part, each establishing its own research team to carry out the study. In each country a sample of research units was selected (a research unit being defined as "a cluster of scientists and technical support personnel working under single leadership, sometimes as a team, on a specific research or experimental development project") covering the academic, private industrial, and public sectors, as well as a broad range of natural and social sciences. Questionnaires were then sent to all unit heads as well as to a sample of staff scientists and technicians in each: over 11,000 individuals in some 1222 research units. The analysis of this somewhat formidable collection of data is intended to provide guidelines for better science policy and research management as well as, as a subsidiary objective, to advance methodology in the study of research effectiveness. To this end contributions to the book are organized not by country but by topic: chapters on the effects of various organizational variables upon research performance are followed by a series of chapters on methodological matters.

Social scientists are mostly well aware of the pitfalls involved in cross-country comparative work, and many would hesitate before participating in a study involving six countries with as many languages. Under surefooted methodological guidance provided (I take it) by Frank Andrews (also the editor of the book) of the University of Michigan's Institute for Social Research, the study seems to avoid the common pitfalls. Indeed, at one level it could be called a model of its kind. Certainly also, in terms of its subsidiary objective—advancing methodology—it can claim some success, providing a most sophisticated account of the relationships among and advantages and disadvantages of various measures of research performance. The range of statistical techniques deployed in the analysis is bewildering to one not expert in such matters.

But I have to judge the work as a contribution to research policy or management, bearing in mind also the possibility of value for theoretical understanding of the working of the research process. From both of these perspectives it has to be adjudged a failure. It is so at two levels. First, with only one or two exceptions it actually has very little that is new and significant to say about the determinants of research effectiveness. Second, it seems to me to have failed because of the basic viewpoint adopted from the very beginning and the sociological assumptions made. Let me discuss these two lines of criticism in turn.

To be sure, most of the aspects of research organization chosen for analysis were, on the face of it, important. There could have been no reason to imagine that so many of them would turn out (at least on the basis of these data) to be unrelated to research performance. For example, an analysis of the value of methods of research planning, and of the advantages of formal methods, would have been of use to industrial research management and to those governments which attempt research planning. But Haraszthy and Szántó's report on the subject is inconclusive. Stolte-Heiskanen looks at the relationships between availability of resources and performance, surely the most central of issues in science policy. It would be extremely valuable to be able to predict the effects of the decline in the resources (particularly for capital equipment) available to most European scientists. ("Resources" in this study is interestingly defined, in terms not of cash but of work space, equipment, support staff, and the like and of subjective satisfaction with each of these. Unsurprisingly, objective measures of provision and subjective measures of satisfaction are not related.) But it turns out that "relationships of objective *material* resources to effectiveness are generally minimal and in the case of some items even consistently negative." Even the author is surprised at this and recommends "caution" in the interpretation of her findings. Admittedly the fact that the results of a piece of sophisticated social research conflict with conventional wisdom is not necessarily a criticism of the research, and even negative results can have major significance. The trouble is that in the study as a whole there are just too many negative findings for comfort. Kowalewska, for example, inspired by Tannenbaum's earlier work on the effects of patterns of influence and control on the effectiveness of industrial organizations, finds no such relationships in the case of R & D units. Some of the authors are more fortunate. Knorr and her associates, in one of two contributions, look at the effects of "quality of leadership" and "group climate" (defined to express extent of dedication, cooperation, and innovativeness in a group). Beginning with ideas developed by organizational theorists, Knorr *et al.* do indeed find associations of each of these variables with research performance, the association being larger for the technological sciences than for basic sciences. And Andrews is able to confirm the results of earlier work by himself and Pelz showing that extent of motivation matters and that diversity of activities is a good thing for scientists.

To be more positive, much the most interesting contribution for my money is that of Stankiewicz, dealing with the effects of size and age of research groups on their performance in research. At a practical level it is interesting because there is a widespread belief among science policy makers that large research groups are necessarily a good idea and that therefore resources should be concentrated in a small number of large groups. A few investigators (including myself) have wondered at this and sought to test the relationship between research group size and effectiveness. Stankiewicz uses only data relating to university research groups in Sweden, but to good effect. On the basis of previous work he hypothesizes that the effectiveness of research groups should increase with size (since large groups can tackle multiple aspects of complex problems, pursue alternative research strategies in parallel, enjoy fruitful interactions between colleagues, and so on) but that beyond a certain size group dynamics begin to prove counterproductive. This hypothesis is supported by the data presented. There is an increase in effectiveness up to 3 to 5 or 5 to 7 scientists, depending upon which of the performance measures is used, and then decline. The

study goes on to consider some of the factors mediating between size and performance. One of these is the research experience of the research leader: highly experienced scientists profit more from large research groups than do the less experienced (at least in Sweden). Such findings certainly have important implications for science policy. Stankiewicz notes that another potentially mediating variable is the research field in which a group works. In my own work of a few years ago limited to chemistry (and to the United Kingdom) we found that the benefits of large research groups were highly dependent upon the area within chemistry in which the group worked and went on to argue that this had to be explained in terms of the kind of research typical within each. Differences of this kind are likely to be much greater when a wide range of disciplines is included, as here. This aspect of the problem, though noted by Stankiewicz, is not developed. The fact that it is not leads me to my second line of criticism.

Most of the contributions explicitly or implicitly adopt a perspective derived in some way from organizational theory and tend to look at research groups as more or less isolated formal organizations. The sociology of science, also concerned with the production (and validation) of scientific knowledge, some years ago discarded an earlier concentration on the "work group" as its major focus of interest. The rather different ties binding the scientist into the community of his or her peers seemed to be of greater importance. It is noteworthy, then, that Kowalewska, faced with her conclusion that patterns of influence within research units seemed of little importance, ruefully admits, "It may be that the functioning hierarchies that matter for R & D are not primarily defined in terms of roles within a single organization." The point is that in this study the whole notion of scientific community has been sacrificed to the hope of "scientific management" of the process of knowledge production. But work within the sociology of science has already suggested that organizational factors, resources, and division of labor (the parameters with which managers can operate) actually vary in their effects from one specialty to another. The problem then is to make sense of this in terms of the cognitive structures of the sciences. Though this line of sociological study as yet lacks immediate applicability it does suggest that policies and managerial practices to be adopted in furthering research in one specialty will not necessarily be the same as those appropriate to another and that the crucial factor is the set of "objects" and theories with which a specialty is concerned. Admittedly all this relates to the basic sciences. The idea of scientific management certainly has more validity in the case of the applied and technological sciences. If this study had been restricted to those it might have been more successful.

The sociology of science has also, simultaneously, developed along another track. Many studies have shown how social, political, and economic factors within a society influence the working of its scientific institutions. This social context of research has also been lost in this study—a great pity, because there is a great deal to be done in the comparative analysis of scientific communities. It is sincerely to be hoped that sociologists will return to this collection of data with the intention of trying to understand the workings of science rather than, unrealistically, trying to formulate principles of scientific management of universal applicability. The particular countries studied would provide a fascinating comparison from this point of view. I personally would be delighted if some of the contributors to the present volume, who know the data, who know the countries, and to whom my remarks on the sociology of science will be no revelation, would set about producing a second volume. It could be a major contribution to sociology of science.

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Geology in Retrospect

Two Hundred Years of Geology in America. Proceedings of a conference, Durham, N.H., Oct. 1976. CECIL J. SCHNEER, Ed. Published for the University of New Hampshire by the University Press of New England, Hanover, N.H., 1979. xiv, 386 pp., illus. $20.

You may dimly recall from your high school lessons in Western civilization that the Whigs were an 18th-century political party in England who championed the cause of popular rights and the democratization of government. The tendency of overzealous Whig historians to reconstruct pre-Whig history in ways most flattering to their reform movement has given rise to an expression sometimes used among historians of science today. To be charged with "whiggery" implies that one has made the mistake of evaluating past scientific ideas on the basis of their resemblance to current scientific thought. Could, for example, the 17th- and 18th-century natural philosophers who refined the concept of the Scala Naturae be considered pre-Darwinian evolutionists? Prior to the publication of A. O. Lovejoy's *The Great Chain of Being* (1936) some biologists who dabbled in the history of science believed so. Taken within the context of its own time, however, the Scala Naturae may be interpreted for what it actually was: an elaboration on religious doctrine immune from the question of speciation. Does the actualist approach to historical inquiry demand anything more than a high standard of scholarship? What is unpardonable in seeking out the true roots of a scientific idea? The conflict between the whig and actualist viewpoints is but one theme that threads its way through the present collection of papers.

Two Hundred Years of Geology in America is a welcome addition to the earlier collection edited by Schneer, *Toward a History of Geology* (M.I.T. Press, 1969). The authors of the 27 papers that make up the book are a mixed group of 32 geologists and historians who met at the University of New Hampshire in 1976 to celebrate both the nation's bicentennial and the long profession of geology in America. Unlike the proceedings of most symposia, the papers were not read at the meetings but were circulated among the conferees ahead of time in order to provide additional time for structured discussion. It is not clear to what extent, if any, the various authors subsequently revised their contributions, but the excitement of dialogue does manage to come across in a surprising number of papers.

In his paper "Geology in 1776," historian Kenneth L. Taylor forces the whig-versus-actualist issue by demonstrating the incipient nature of the science at the time of the American Revolution (the invention of the word "geology" by a European was still two years away). Paleontologist Stephen J. Gould presents an intriguing twist on whiggery in his study "Agassiz' later, private thoughts on evolution," based on the marginalia found in Louis Agassiz's own copy of Haeckel's *Natürliche Schöpfungsgeschichte* (1868). The detailed commentary scattered through the entire 568 pages of Haeckel's book suggests that Agassiz did not retreat blindly from "rational science" in his later years, as is often charged by 20th-century historians. Three significant papers in the collection relate to the topics of continental drift and plate tectonics, and