Book Reviews

Physics as a Social Model

Social Stratification in Science. JONATHAN R. COLE and STEPHEN COLE. University of Chicago Press, Chicago, 1973. xiv, 284 pp., illus. $12.50.

Originality and Competition in Science. A Study of the British High Energy Physics Community. JERRY GASTON. University of Chicago Press, Chicago, 1973. xx, 210 pp. $10.95.

It is often suggested that the institutions, norms, or social processes of science illustrate something important about how larger segments of society operate, or about how we ought to try to make them operate. Sociological studies of science are bringing a good many data to bear on such suggestions, and one of the books under review, the Coles', does this for an issue that is central for all social organization: To what extent are elites—strata holding special powers and privileges over other men—justified and necessary?

When this question is raised about society in general, its interest to observers on either the political right or the political left is obvious. When it is raised about science, its interest stems from a tendency to see that social institution as an especially pure example supporting one or another political view. For instance, certain political radicals have come to look upon science as the archetypal case of the elitism they oppose, and thus to condemn it totally (see the works of Theodore Roszak). The Coles, on the other hand, would argue that science decisively supports the claims of "functionalists"—those who view stratification as just and necessary. Their book contains the most extensive data available in support of certain aspects of this view, although, it seems to me, they miss entirely the essential character of science's stratification system and do so by making the same erroneous assumption that antiscience radicals make.

The Coles argue their case through some 20 of their own studies, involving more than five years of work, plus a good many other sociological investigations. These studies deal with various sciences, but a large percentage, including most of the Coles', focus on the American physics community. Most of their own studies also rely in whole or in part on the *Science Citation Index*. They take the total number of citations to a given paper (or to some set of papers such as a particular author's life work) as an operational definition of its quality, and thereby have a measure of the quality of a scientist's work that they can compare with indicators of his rank in a field—the number or type of honorary awards he has received, the prestige of the department where he is located, or how well known other scientists, in interviews, report his work to be. Further, they can determine how much of the variance of these indicators of rank can be explained by, on the one hand, number of citations to a man's work and, on the other, more extraneous variables such as the sheer number of papers he has published, the rank of the university where he received his degree, and his age, race, sex, or socioeconomic background. If more of the variance of scientific rank is explainable by number of citations than by these other variables, the Coles argue, then to that extent science's stratification system is universalistic, advancement in it being based on the quality of one's work rather than on extraneous criteria.

By methodically exploring a sizable number of these questions, a substantial case is amassed that, at least among physicists, rank is indeed universalistic. For example, although the Coles find a fairly high correlation between the quality of a scientist's work as defined by citations and the sheer quantity of papers he has produced, they do not accept the implication of most prior studies that quality and quantity of scientific production are almost synonymous. By partialing out the effects on various criteria of scientific status, they build a well-supported case that "the sheer volume of scientific output has little influence on building a scientific reputation" (p. 102), that instead "the quality of a physicist's work, as evaluated by his colleagues, is the single most important determinant of whether he rises to a position of eminence or remains obscure" (p. 122). This they say strikes a strong blow against the popular view that "the scientist who has published only a few solid papers will typically be passed over [for promotion or honors] in favor of the mass producer of trivia" (p. 96). Similarly, they show that a scientist's "reputational success" is not influenced by the standing of the department in which he earned his Ph.D. "Good work is recognized no matter where its producers come from" (p. 95). They find also that there is little discrimination among physicists against Jewish and women scientists in their field (there were too few blacks in physics to be represented in the study).

In the use and awareness of research the authors again find physics to be universalistic:

> Location in the stratification system had little influence on either what work a scientist utilizes or who utilizes his work. Perhaps most important about this part of the analysis was that the data offer little support for an "in-group" interpretation of citation practices. Elites cite the work of non-elites as frequently as [do] non-elites themselves [p. 190].

In another chapter, they take physics papers that were cited frequently several years after publication and check on how frequently they were cited in the first year after publication, to see what sort of authors received "early recognition." (If authors who have, say, received high awards prior to such a paper's publication receive earlier recognition for it than do other authors, this might be taken as a deviation from universalistic standards of evaluation.) They find that although two attributes—having high repute (as measured by total citings to one's other papers) and being at an especially prestigious institution—each had a very slight independent effect ($r = .18$) on early recognition, other variables one might expect to be important—membership in the American Physical Society, number of honorific awards, prestige of highest award, and age—had no significant effect at all in producing early recognition of this sort of paper.

However, while all this does indeed seem to add up to a strong case that rank in physics is based on merit, does it establish that science presents an instance in which, as functionalists would argue for society in general, stratification must be accepted rather than opposed? The Coles seem to assume that it does, or at least that only one further point need be made to defeat opponents of stratification—that ranking in science, besides being based on merit, also is *necessary* to science as an institution. They attempt to do this by describing the role that high-ranking scientists play in making science's necessary decisions:

> The stars in a particular field determine which ideas are acceptable and which are not. Abandoning the principle of authority would eliminate a rational basis for discarding poor or irrelevant work [p. 78].

> Given the role that stars play in exercising authority, establishing and maintaining consensus, serving as gatekeepers for scarce resources and referees for journals, consider what would happen to a field without stars. It is unlikely that a modern science could function at all. Scientists must be found to fill these important positions [p. 79].

and thus,

> The institution of science is a highly stratified one. There is at least as much inequality in science as there is in other social institutions [p. 89].

Now, few would deny that science must indeed decide issues, or that the scientists who figure prominently in reaching its decisions tend to be those who have the most prestige. However, in the Coles' statement of these facts I think one can also see another assumption, one which is more crucial to the issue of stratification than those that they discuss explicitly. This is that decisions are made by scientists in important positions, and that it is as occupants of these positions that they discard poor or irrevelant work, exercise authority, and maintain consensus. To hold to this assumption the Coles have to lump together status based on administrative position and status based on substantive contributions to research or theory:

> There is substantial overlap in the groups having power and those having prestige. The two groups having the capability of wielding power are the same two groups which make up the prestige elite: those scientists who have earned recognition for their outstanding contributions to knowledge and those who hold key administrative positions. They comprise the relatively small number of scientists who largely control mobility within science [p. 81].

> The distinction between power and authority in science may not be a particularly useful one [p. 81].

In other words, the Coles conceive of rank and administrative power as essentially undifferentiated, and would interpret them together in terms of the standard functionalist interpretation of power in business or society in general —that for group decisions to be made power must be conferred on elite decision makers. This is exactly how those radicals who see science as evil think it operates. But what if there is another possibility? What if a group's key decisions can also be made through a system we might call "participatory"? By this I do not mean one in which everyone always participates, but simply one such that anyone who cares to speak on an issue is always able to do so, a system such that no one is able to obtain the power to *make* major decisions, but rather must be content with influencing them by submitting persuasive messages to some open forum. If one looks at science with this possibility in mind, one will see it rather differently from the way the Coles do.

For instance, where they see editors of scientific journals deciding "what should and should not be published" (p. 81), one will notice how much such journals rely on the judgments of separate referees. Of course, editors or their consultants choose these referees, and thus do have some degree of bureaucratic power. But does this mean that the editor of, say, the *Physical Review*, which employs something over a thousand referees a year, exercises power that is not to be distinguished from that which a company president exercises over his employees, or even that which a newspaper editor exercises over the information and opinions that his subordinates transmit?

In general, the result of the Coles' undifferentiated concept of group decision making is that they view scientific work itself as essentially a way of obtaining positions, positions that then confer the power to make decisions. One whose concept of decision making includes a participatory possibility will see the process of research, publishing, and so on as itself science's primary "decision maker," and any bureaucratic power that success in this realm confers as more of a side effect than a dominant aim. He will also see science's relationship to other endeavors differently. For instance, whereas the Coles proceed throughout as though scientific success differs from success in business or government only in being based on more universalistic and "clearcut" criteria of success (see especially pp. 254 ff), one with a differentiated view will find science interesting in the extent to which it keeps bureaucratic position subsidiary to prestige obtained through its more fundamental participatory process. In other words, where the Coles see only "overlap" between administrative positions and scientific prestige—since they are "functionally equivalent"—one with a differentiated view of stratification will see a fundamental conflict between the participatory realm (and prestige gained in it) and power based in a science's administrative realm. And he may suspect that the universalistic criteria the Coles find governing advancement in physics could not be maintained if participatory decision making did not dominate. As we have known since at least Michels, the first use an elite with unopposed bureaucratic power makes of it is to secure its own position, thereby ending universalistic criteria for achieving power. In other words, I am suggesting that science's participatory decision making process provides the necessary check over bureaucratic power that enables the sciences to operate as well as they do. This sort of view would lead one to ask what the balance between the two realms in fact is: How open are a particular field's journals? To what extent is its contract distribution done through peer review? Are bureaucrats gaining control over the field? Different sciences, or the same science at different periods, might thus be expected to differ, rather than, as the Coles would have it, to appear identical in a common functional necessity for an elite to make their decisions (*1*).

The Coles' general political assumptions also seem to me to underlie the results they obtain in considering the "Ortega hypothesis," the notion that "the majority of scientists help the general advance of science," which they believe their citation studies contradict. Their chapter dealing with this was published as an article in *Science* (**178**, 368 [1972]), and both the conclusion and the evidence were criticized in a later issue by several physicists, who pointed out some patent weaknesses of citation counts as indicators of quality or usefulness (*2*). The Coles are, however, aware of the imprecision of this kind of measure (see their chapter 2), and I think it is not the basic flaw of their work. Where they go astray is in failing to distinguish between scientists' opinions about what is promising or important in their field and the influences that bear on scientists in arriving at such opinions. The Coles assume that citations document these equally well.

This error can best be understood in relation to their general assumptions.

Note first how taking success in a business bureaucracy as their sole model of success in science leads them to interpret scientific recognition:

> Recognition in science is the functional equivalent to property; and the right to "recognition" is indeed an inalienable one for scientists [p. 45].
>
> The scientist who is located at Harvard or Brookhaven, who is the head of the National Science Foundation, who has won a Nobel prize, or whose life work has made a wide impact on his field, is a "rich" man in science. He has property. Correlatively, the scientist who has not been widely honored, who is located at a small, relatively unknown college, has little property [p. 46].

This sort of analogy may be useful for some purposes, but note its result when combined with a standard assumption of politically conservative functionalists—that property accumulation adequately indicates the value to a society of its various socioeconomic strata. Applied to science, this assumption allows one to conclude that citations, as the most tangible indicator of scientists' "property accumulation," adequately indicate the value to science of its various sorts of members. And, just as the most conservative political observers are ready to assume that social strata receiving little or no income are *ipso facto* of little or no value to their society (and hence deserving of little or no public support), conservatively oriented observers of science might conclude that scientists receiving very few citings are of little or no value to science. Whether in the Coles' case it is because of some political inclination or not, they reason in precisely this fashion, concluding that citation data make it reasonable to suppose that the somewhat over half of all research physicists who are rarely cited could be eliminated with no loss to the development of knowledge or opinion in physics.

An alternative model of how scientists' opinions are formed is well developed in the works of Kuhn and Ziman, and especially in the interesting sociological elaboration of Kuhn's work by Crane (*3*). These authors assume that influence in science is transmitted through something basically analogous to informal group interaction, but which includes remote contacts through publications and through other colleagues' conversation, as well as through face-to-face interaction. With this sort of model of scientific influence available as an alternative to "property accumulation," it seems to me that only a really doctrinaire sort of functionalism could ever lead one to reason as though citations tell the entire story of the influences on scientific opinion formation.

Despite the problems growing out of the Coles' sometimes rigid functionalism, however, it should be noted that their book contains a great deal of painstakingly collected data, and it will be unfortunate if its faults cause it to be ignored by social scientists concerned with understanding science's social organization or with extending key attributes of this organization to other parts of society.

Jerry Gaston's book is less ambitious than the Coles', in that it does not pursue any one aspect of science's organization quite so thoroughly. But to a nonphysicist it conveys a much stronger sense of what one community of physicists may actually be like. Gaston interviewed and surveyed a good sample of British high energy physicists, and rather artfully explains the many subtle forms of status, competition, and cooperation that obtain among them, separating and allying abstract, intermediate, and phenomenological theorists, bubble chamber and counterspark experimentalists, scientists located at Oxbridge, Redbrick, and Scottish universities, and so on. The book is exceptionally well written and contains good, refreshingly brief summaries of much of the preceding work in the field. Some of Gaston's most interesting analyses deal with the effect of the centralized British system of funding science, as compared with the more decentralized American system, and with the effects that experimentalists' need for large organizational efforts has on increasing the importance of administrators among them and in causing them to concentrate on "news" in scientific papers rather than on detailed analyses. Gaston reports a number of original survey results, but the book is more interesting for its explanations of what underlies these results. As Ziman's introduction indicates, Gaston's work is perhaps better seen as a kind of anthropology of physicists than as a purely sociological treatment.

In sum: two useful books—about physicists, and possibly about a good deal more.

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References and Notes

1. An example of the Coles' insensitivity to such questions shows in their several comparisons between physics and sociology. They are aware that physics journals accept approximately 85 percent of everything submitted to them, while sociology journals typically reject almost that percentage. But in several mentions of how little conflict there seems to be between established elites and other scientists in physics, the Coles never remark that this has not also been true of sociology in recent years, much less that there might be some connection between this difference and differing availability of journal space in the two sciences. For a suggestive study of journal acceptance in different scientific fields see H. A. Zuckerman and R. K. Merton, "Patterns of evaluation in science," *Minerva* **9**, 66 (1971).
2. See letters from S. A. Goudsmit, J. D. McGervey, and R. J. Yaes, *Science* **183**, 28 (1974), and the Coles' reply (*ibid.*, p. 32). See also E. Garfield, *ibid.* **182**, 1197 (1973).
3. T. S. Kuhn, *The Structure of Scientific Revolutions* (Univ. of Chicago Press, Chicago, ed. 2, 1970); J. Ziman, *Public Knowledge: The Social Dimension of Science* (Cambridge Univ. Press, Cambridge, 1968); D. Crane, *Invisible Colleges: Diffusion of Knowledge in Scientific Communities* (Univ. of Chicago Press, Chicago, 1972).

Lasers and Their Uses

Laser Handbook. F. T. Arecchi and E. O. Schulz-Dubois, Eds. North-Holland, Amsterdam, and Elsevier, New York, 1973. Two volumes. xlviii, 1948 pp., illus. $175.

These two volumes constitute a handbook in the *Handbuch* sense, "a scholarly book on a specific subject, often consisting of separate essays or articles," rather than a reference handbook of data and formulas (the latter need being met for the laser field by the Chemical Rubber Company's *Handbook of Lasers* edited by R. J. Pressley). This handbook contains 40 chapters by 53 authors, nearly half from outside the United States. The first volume covers theory and practice for lasers and nonlinear optical devices under four broad headings: basic theory and laser physics, classes of lasers (gas, solid, liquid, and semiconductor), laser devices and techniques, and materials for nonlinear optics and light modulation. The second volume reviews laser applications divided broadly into "physical" (that is, scientific) applications—light scattering (Rayleigh, Raman, and Brillouin), nonlinear and coherent optical phenomena, two-photon spectroscopy, and plasma formation—and "technical" (that is, practical or technological) applications—metrology, holography, optical information processing, photography, material processing, ranging, communications, and medicine.

It would be difficult to improve on a perceptive and detailed review by Peter F. Moulton, Helge Kildal, and Paul L.