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The Scientific Enterprise

Public Knowledge. An Essay Concerning the Social Dimension of Science. J. M. ZIMAN. Cambridge University Press, New York, 1968. xii + 154 pp. Cloth, $3.95; paper, $1.95.

"Science" is a dirty word, scientifically speaking—imprecise, vague, and only subjectively definable. Science is variously described as a body of knowledge, a methodology, a philosophy, an attitude, a mystique, even a religion. Its component disciplines and subdisciplines have different views of what constitutes "good" science, depending on their respective stages of development.

The vast majority of professional practitioners of science couldn't care less about delineating it specifically. Having been educated and trained in their particular fields, and being accepted as professional physicists, chemists, biologists, or what have you, they have a conviction that what they are doing in the practice of their profession is "science" and an intuitive feeling about what constitutes good, bad, or indifferent work in their fields. They consider the broader aspects of the "system" and their interaction with it only when they become indignant at some breakdown in it, such as a failure to accept them into the in-group, rejection of what they consider to be a good paper, or unnecessary difficulty in retrieval of information from the many sources normally available. But they regard introspection as to the nature of science and the structure of its institutions as a waste of their time—something to be left to the philosophers, sociologists, and historians.

This situation is now changing. The so-called publication explosion, the enormous expansion in the number of practicing scientists and in the total cost of scientific research, and the increasing involvement of science with public issues have placed great strains on the normal functioning of scientists —on their institutions, their techniques for formal and informal communication, their training, their relations to the general public. Confronted by a plethora of glib, half-baked, and often drastic solutions to these difficulties, scientists are being forced to examine their institutions and to ponder over what aspects of the established system are good and worth preserving, what aspects could stand improvement, and how these improvements could be effected.

For this reason Ziman's book is particularly timely. As a research physicist, with a direct involvement in the various phases of scientific work and with a long-standing interest in the nature of science, he has brought a great deal of insight to the study of the system. After some introductory chapters summarizing current views on what constitutes science (and nonscience), he sets forth and develops the point of view that science is "public knowledge" —that is, a body of knowledge (facts, techniques, and concepts) that has been generated by members of a scientific community and, through a process of exchange, criticism, refinement, filtration, distillation, become part of a "public consensus." In elaboration of this point of view, he studies the "social dimension" of this scientific community —the education and training of scientists, the relation of the individual scientist to the scientific community, the varieties of informal and formal communication within science, and the relation of scientists to the institutions which employ them.

What comes through is a sense of the orderliness in the accumulation of scientific knowledge, in spite of the complexity of the processes involved. One sees the extent to which a scientist is affected by the attitudes he absorbs in the educational and training period, the functions of the various kinds of informal communication, the refinements that take place in the transition from informal communication to the archival literature and eventual absorption into the "consensus."

The development of these topics in the book is clear and well organized. It reads very smoothly, and only the reader who has himself tried to put similar ideas into words will appreciate the difficulty of the undertaking and the thought and care that Ziman must have spent to achieve this well-balanced, orderly exposition. Scientists, particularly those in the physical sciences, will find few statements with which they will disagree. What is presented is, in a sense, a consensus of what the social structure of science is or should be. Exceptions, flaws, and problems are carefully noted, but the general structure is kept firmly in sight throughout.

Partly because of this, what one misses in reading the book is the sense of science and its social structure as a dynamic, evolving mechanism, with continuing ferment and change in its institutions and communication channels and even in its overall relative values. Ziman presents the social structure of science as it has been in recent years, not as it was even 30 years ago or as what it will become. There is an element of complacency in the discussion, in that what is good is played up and what is bad (and getting worse) is dismissed somewhat too lightly. Little note is taken of the acute indigestion afflicting many fields of science today. Little emphasis is placed on the revolutionary advances in computer technology, printing methods, and duplicating techniques, or on the growing interaction of science with the general community. There is no mention of the current trend, in science as elsewhere, to challenge the Establishment, to propose radical changes, and to demand validation of current traditions and procedures.

In the area of scientific communication, for example, questions and proposals of the following sort are being debated, with considerable heat:

1) Has the traditional archival medium, the research journal, outlived its usefulness? Will it (or should it) be replaced by a vast collection of documents (refereed or otherwise), with only titles and abstracts receiving wide circulation? Can the input to such a collection receive adequate quality control in the form of refereeing and rejection or forced revision, or will every user have to do his own filtering?

2) Will "hard copy" research journals be replaced by computer stores able, by virtue of detailed classification of subject matter, to retrieve and supply appropriate papers by matching individual readers' subject-interest profiles or in response to suitably framed questions? Or should the research journals be retained, and be supplemented with smaller packages of bound "instant reprints" dealing with closely related subject matter, to which individual readers could subscribe?

3) Should the "invisible colleges" be made visible by making available, as an

addition to the current formal literature, the outputs of conferences, symposia, lectures, research proposals, preliminary drafts of papers, and so on? Could this be done without destroying the very nature of the informal communications?

4) Will the blurring of the distinction between formal and informal scientific communication (for example, by worldwide distribution of preprints and other reports prior to refereeing or other evaluation) help solve the problem of excessive publication?

5) What is the relative importance of speed of communication as against quality control of the retrievable formal literature, such as is provided by the evaluation of manuscripts by referees and editors? The proposals being made range all the way from bypassing the control procedures entirely to an order-of-magnitude increase in the strictness of refereeing.

6) Should research results be deposited in the archival literature, not in the form of fragmentary articles as at present, but in predigested form by rapporteurs or "critical review" writers working from unpublished documents?

7) Should major changes in the system be arrived at through open debate in the scientific community, or through action by small groups on elements of the structure with which they are particularly involved?

Questions of similar generality and significance are being raised concerning the education and training of scientists, the character of scientific organizations, and the relation of science to society and to public policy. Ziman's book does not furnish definitive answers to such questions. But by its development of the thesis that science involves intrinsically the social structure of the scientific community with a built-in orderliness and logic, and in its cautious, even-tempered analysis of the elements of that structure and of their relevance to the whole enterprise, it provides a firm base from which to consider the many controversies that agitate science today.

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Hannah Smith's Son

A Portrait of Isaac Newton. FRANK E. MANUEL. Belknap Press of Harvard University Press, Cambridge, Mass., 1968. xviii + 478 pp., illus. $11.95.

The title of Manuel's book is somewhat misleading. A portrait of Isaac Newton it certainly is, but it is not merely another portrait. Working in a tradition that is well established though not, because of its extraordinary demands, widely followed, Manuel has exploited the vast body of Newtonian manuscripts to produce a historical psychoanalysis. "Should the unconscious perchance not exist," he remarks, with about the same degree of uncertainty that Newton felt when he asserted conclusions under the guise of queries, "one of the underpinnings of the book collapses." To which we might add that another underpinning begins at least to shake if the Freudian analysis of the unconscious turns out to be incorrect. It is one of the potential weaknesses of the work that it is thoroughly Freudian in approach at a time when Freud's authority is ever more challenged. Nevertheless, it is impossible to contend that Manuel's insights into Newton's character have validity only within a Freudian context. He has produced a stimulating and provocative book, which uses the devices of psychoanalysis to place the study of Newton the man on a new foundation.

The title of chapter 1 presents the central interpretative theme that Manuel develops—"Hannah and the fathers." "Hannah," of course, refers to Newton's mother, whom he possessed exclusively for a few years, whom he lost to her second husband at the age of three, whom he sought to rediscover and repossess for the rest of his life. "Fathers" is deliberately put in the plural, referring to the real father Newton never knew, to God the Father who merged with the real father in Newton's psychic life, and to the hated stepfather who at once violated the chastity of the real father's wife and deprived Newton of her presence.

> One is almost [sic] tempted to recognize in his genius a union of two experiences, his relations with the father whom he never saw and with the mother whom he possessed with such intense emotion, whom he saw with his own eyes and always longed to see again as he had in the early years of infancy—a fantasy he pursued in vain throughout his life. . . .

Manuel sees the major traits of Newton's character as products of the two basic experiences. From the "loss" of his mother derived his terrible insecurity and his sense of deprivation. Because of the latter, any attempt to seize and carry off a child of his brain aroused in Newton, as Hooke and Leibniz discovered, a frantic and enraged defence of what was his own. Manuel insists on the energizing role of the mother in Newton's career. The *annus mirabilis* of 1666 had its locus in Woolsthorpe, to which Newton returned because of the plague; and again in 1679, the discovery of the crucial theorem of the ellipse was connected with Woolsthorpe and the death of his mother. With Fatio de Duillier, whose enormous role in Newton's life Manuel is the first to insist on, he enacted again the scenes of affection and abandonment from his youth, and in his niece Catherine Barton, born in the year his mother died, he found her reincarnation.

In pursuit of the father he had never known, the earthly father who was also the Heavenly Father, Newton saw himself as the one chosen of God. Manuel wishes even to maintain that Newton's unitarianism derived from his conviction that he himself was the only begotten son—born, after all, on Christmas Day, and spared by divine grace from the early death his weakness led everyone to expect. But the chosen of God also stood under the judgment of God, bound to obey the law and all too aware of his lapses from it. Newton bore a terrible freight of guilt to the very grave, and much of his life was devoted to the search for other culprits on whom to project his guilt that he might punish them to demonstrate his obedience. The condemned of God was at once