Book Reviews

Shifting Views of Science

Social Processes of Scientific Development. Papers from a conference, London, Sept. 1972. RICHARD WHITLEY, Ed. Routledge and Kegan Paul, Boston, 1974. x, 286 pp., illus. $19.95.

By now it is the most tedious of banalities that the place of the natural sciences in our culture is problematic, uncertain and shifting. Even five years ago it was possible to dismiss this uncomfortable perception by reference to how the times themselves were out of joint. Such an escape is no longer plausible. Instead the transformation of our cultural valuations of science continues apace, even as it slowly becomes possible to identify some parameters of our intellectual malaise and some possible directions of resolution. The papers contained in *Social Processes of Scientific Development* exaggerate rather than mirror the confused state of present debate but are interesting for the more general questions their appearance provokes. In order to broach those questions a historical excursus is necessary.

A powerful vision of rationality, and of mathematical astronomy, theoretical natural philosophy, and experimental physics as its repository and guarantor, has molded Western civilization over the past two centuries. This vision found its evidential base in the cumulating achievements of Copernicus, Kepler, Galileo, and Newton. Its philosophical articulation, begun in the Enlightenment and carried on by philosophers as varied as Auguste Comte and John Stuart Mill, found its own culmination in the formulations of logical positivism associated with such names as Bertrand Russell, Rudolf Carnap, and A. J. Ayer. The social utilities of the vision lay in its subtle resonances with the transformations inherent in the processes of industrialization. Those processes are now essentially complete, within the English-speaking world. The ordering, intellectual, and religious purposes served by the vision exhibit corresponding decay. Nonetheless, the associated philosophical formulations live on to enchain contemporary thought in many areas.

One form in which the philosophy endures is as the underpinning to a major tradition of historical thinking about science. That tradition emphasizes the similarities, indeed the unity, of the various natural sciences, the reality of the scientific method, the coherence of scientific theory, the supremacy of mathematical modes, and the uniquely cumulative character of scientific knowledge. Though individual historians have varied as widely as humanists will, it is this tradition that has nourished the best work of the last two generations—work of thinkers as diverse as George Sarton, Alexandre Koyré, and Herbert Butterfield, and of most current holders of major chairs in Britain, North America, and Australia. Not surprisingly, the years from 1945 to 1970, in which the natural sciences saw rapid growth in funding and acclaim, were years that served to reinforce to both scientists and laymen this picture of a past where scientific ideas, scientific procedures, and scientific debate, free of social gravity, led inevitably to truth and progress. The social sciences, enjoying their own growth in audience and respectability, yet aware of their cadet status, were content to accept this picture and bask in a reflected glory.

Meanwhile, by an unplanned division of labor which was yet pregnant with implications, the fledgling discipline of the sociology of science took up precisely those questions of social organization that the historians of science chose to ignore. Accepting the same rationalistic vision, sociologists too saw the substance of natural science as unaffected by the force of social gravity. They therefore focused on the structures and procedures by which the scientific community combined the potentially disruptive personal quest for recognition ("priority") with the social pursuit of validated knowledge. In this they have been brilliantly successful, thanks primarily to the masterly work of Robert K. Merton and the students he has trained and influenced over the last 40 years. Their researches have unequivocally established what many natural scientists doubted, namely, the fruitfulness of viewing science from a sociological perspective. One indicator of the suspicion with which the very possibility of sociological study was still regarded in the postwar years is the recent confession in this journal (10 May, p. 656) of so influential and acute a historian of science as Charles C. Gillispie that for several years he dismissed as "a bit trivial" what he later came to see as "the most eye-opening single piece" Merton has written.

But while historians and sociologists of natural science have been belatedly discovering the extent of their common ground, that ground has—as so often—been shifting under their feet. More scrupulous attention to the actions, as opposed to the post hoc rationalizations, of the scientists they study has led historians of science to question the very philosophic assumptions on which their own work was based. Most incisive in his questioning and most cogent in his reformulations is Thomas S. Kuhn. In his *Structure of Scientific Revolutions* (University of Chicago Press, 1962; new edition, 1970) Kuhn was led to portray science as an evolutionary enterprise lacking explicit goals and routinely transformed by collective gestalt switches rather than by rational debate. Although Kuhn's reformulations have provoked more debate than agreement among his colleagues, the thrust of his argument well represents the general direction of movement.

Kuhn's work has also influenced every field of social science by making apparent the fictive nature of that "proper" scientific methodology which some social scientists had sought to assimilate from such highly regarded fields as experimental physics. In recent years social scientists have found quite other reasons for staging a revolt against value-free, scientistic norms for their own inquiries. The causes of that revolt are too complex for discussion here. One result of these twin currents, however, is an unfamiliar sense of confusion and crisis about how we are to understand Western science as a culturally embedded historical phenomenon. There is obvious agreement that such an understanding must draw on the insights and canons of explanation of anthropology and sociology as well as on those of intellectual history.

Equally, that understanding will transform by feedback the ways in which we perceive the very social sciences to which we turn for aid.

This of course is a broad and lofty prescription. Whitley's symposiasts are well aware of the problems (one of them, Stuart Blume, has broached a particular subset of the issues in his own *Toward a Political Sociology of Science,* recently reviewed here: 12 July, p. 137). However, with one or two notable exceptions (Cornelius Lammers on the social implications of diversity in the social sciences, Dorothy Zinberg on the social perceptions of chemistry students), their contributions fail to advance our understanding in significant ways. Indeed, in its diversity of topics, in its occasional shrill dismissal of the major insights that Merton's work has afforded into the social system of science, in its narrowness of historical sympathies, and in the sketchy remedies it proposes, *The Social Processes of Scientific Development* speaks eloquently of our current confusions. To see those confusions spoken *to* rather than simply displayed, the reader should turn instead to such recent works as Mary Douglas's *Natural Symbols: Explorations in Cosmology* (Pantheon, 1970), Alvin Gouldner's *The Coming Crisis of Western Sociology* (Basic Books, 1970), the first volume of Stephen Toulmin's *Human Understanding* (Princeton University Press, 1972), the studies of Western science and non-Western tradition associated with the name of Robin Horton (for example, *Modes of Thought*, Humanities Press, 1974), and the growing range of new studies both in the sociology of knowledge and in the cultural history of the natural sciences. What these works reveal, when taken severally and together, is that a veritable revolution of consciousness is now under way.

As yet we still wait for a new consensus on the shape of our scientific past. It seems reasonable to suppose that any such refashioned understanding will be pluralistic in its stress, concerned with the diversity of the various sciences, and distinctly cautious over the autonomy of the intellect and the hegemony of the Western tradition. One rewarding site for research directed toward creating elements for that consensus would seem to be the nodal points where interaction takes place between some or all of the cognitive levels embodied in the various sciences (for example, the experiments, theories, and laws of a particular science and the concepts, orientations, and presuppositions that it uniquely holds, those that it shares with other sciences, and those that it shares with wider social elements), the cultural patterns of norms, values, and beliefs in the larger society (whether philosophical, political, economic, or religious), the social arrangements of the various sciences (such as institutional groupings, patterns of recruitment, training, employment, and reward, and patterns of financing) and those social groupings, social interactions, and social realities (whether populational, technological, or positional) in the larger society which help create, confine, and shape the ways in which organized knowledge evolves.

The challenge now is to provide new work that addresses these issues in significant ways, while also measuring up to the standards of rigor, clarity, and persuasiveness apparent long ago in, say, G. N. Clark's *Science and Social Welfare in the Age of Newton* (Oxford University Press, 1937; second edition, 1949; reprinted 1970). On this count *The Social Processes of Scientific Development* must be declared a failure. It is yet an interesting failure in the way it seeks to grapple with a major intellectual question of our day.

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Bioinorganic Chemistry

Metal Ions in Biological Systems. HELMUT SIGEL, Ed. Dekker, New York. Vol. 1, Simple Complexes. 1974. xviii, 268 pp., illus. $21.75. Vol. 2, Mixed-Ligand Complexes. 1973. xvi, 294 pp., illus. $25.25. Vol. 3, High Molecular Complexes. 1974. xiv, 290 pp., illus. $22.75.

Inorganic Biochemistry. GUNTHER L. EICHORN, Ed. Elsevier, New York, 1973. In two volumes. xxxviii, 1264 pp., illus. $110.

Both these books are collections of papers by various authors, and both are readable and well put together. There is little direct overlap in content. In general, the first three volumes of *Metal Ions in Biological Systems* deal with interactions between metal ions and proteins and their subsequent effect on protein or enzyme functions, whereas *Inorganic Biochemistry* includes many chapters on the role metal ions play in proteins and metal enzymes themselves. Future volumes of *Metal Ions in Biological Systems* will contain several chapters with titles similar to those in *Inorganic Biochemistry,* but by the time they appear new results will have necessitated further review.

Inorganic Biochemistry is a two-volume work that does an excellent job of introducing the reader to basics of coordination chemistry and of showing how metal ions function in complex biological systems. It provides an excellent pathway by which an inorganic chemist or a biochemist can enter the field of bioinorganic chemistry.

Part 1 of the work provides a brief but not entirely superficial review of relevant basic coordination chemistry. In part 2 the interaction of metal ions with amino acids, peptides, and proteins is reviewed. An excellent discussion of some metalloproteins involved in the storage and transfer of iron and copper is contained in part 3. A chapter on hemocyanin is also .included. Part 4 contains a chapter on the activation of small molecules by means of coordination. The next chapter reviews how metal ions participate in enzymatic activity, and other chapters review the structure and function of carboxypeptidase A and carbonic anhydrase. Discussions of phosphate transfer and its activation by metal ions and kinases are also included. All the reviews emphasize spectroscopic techniques. Part 5 contains a rather thorough but not completely up to date review of enzymatic oxidation-reduction systems. Heavy use is made of model systems for metal enzymes that catalyze reactions with molecular oxygen and that fix molecular nitrogen. A chapter on electron transfer, which is heavy on theory, and reviews of the ferredoxins and other iron-sulfur proteins and copper-containing oxidases are included. These chapters provide numerous suggestions for possible inorganic model systems. Part 6 is concerned primarily with the porphyrin prosthetic group and its properties in and out of proteins. Chapters are included on iron-porphyrin compounds, including detailed discussions of myoglobin, hemoglobin, cytochromes b and c, cytochrome oxidase, peroxidases, and catalases. There are also excellent chapters on chlorophyll and corrinoids. Again, work with inorganic model systems is emphasized. Metal complexes of vitamin B_6 that serve as models for B_6-catalyzed enzymatic processes and metal complexes of flavins are discussed in part 7. Finally, in part