Book Reviews

Man, Science, and Society

The Broken Image: Man, Science, and Society. Floyd W. Matson. Braziller, New York, 1964. 355 pp. $6.95.

This book will satisfy few but infuriate many, including even those who (like myself) are fundamentally in sympathy with it. But it will also stimulate. It does not succeed, as the author claims, in laying a truly scientific foundation for the study of man and his society. But its very shortcomings raise important and searching questions.

There are three parts to the book. In the first and longest part, "The Great Machine," Matson surveys the main trends in contemporary American behavioral science and finds them sadly wanting. Indeed he concludes that behavioral science starts out by denying man and ends up by destroying him—by making him the "broken image" of the title. In the second part (under a rather unfortunate and ugly heading—"Humanization"), Matson argues that the dominant schools of behavioral science actually apply an outmoded concept of 19th-century mechanics, and that the main trends in today's natural sciences lead to approaches entirely different from those considered scientific by the behavioral scientist. In the very short conclusion, actually only a few pages, the author then tries to set forth what he considers to be the proper foundation of a science of man and society.

Of the three parts, the first is the most brilliant and the most successful. The first 126 pages of this book are a short but comprehensive survey of the main schools of thought in American behavioral science. Although the author's bias is obvious and open, he documents his statements by comprehensive and well-chosen excerpts from leading authorities. Above all, he puts American behavioral science and its European parallels into historical and philosophical perspective. Even those behavioral scientists who find his conclusions least acceptable would agree, I imagine, that he states fairly the assumptions and the methods of today's behavioral science. They should even agree with his conclusions that the empirical behavioral scientist is not primarily interested in understanding man as such but in manipulating him, and that he denies, if only by implication, that a unique being, called Man, exists or that there could be any scientific concern with human and political values and goals.

The second part, however, is much less convincing and is, in fact, altogether inconsistent. The argument that 20th-century physical science has completely changed approach, direction, and method, and that it leads to philosophical conclusions totally at variance with the scientific method of yesterday, is one few scientists would be willing to accept. At least, judging from the correspondence columns of *Science*, I believe few biologists would accept the author's statement (on p. 161) that "the recovery of purpose in biology . . . would seem by now to be firmly established." Few physicists, to my knowledge, would go along with Matson's interpretation of Heisenberg's uncertainty principle as restoring "indeterminacy" to "nature"—in the sense that indeterminacy means control by purpose rather than by cause. A probability statement is not, as Matson maintains, indeterminate, nor is it, as he seems to believe, nonquantitative. It is merely not relevant to any one element within the universe but completely determined and rigorously defined for the universe as a whole.

But worse still is the internal contradiction and inconsistency of the author's argument. In the first part he argues that behavioral science is pseudoscience because it applies the concepts of physical science to man, who is not in his entirety part of the physical universe, and to man's society, which is altogether a social rather than a physical phenomenon. What Matson asks for in the first part are concepts for the study of man developed from definitions of man's special and distinct nature and existence. Then, suddenly, 20th-century physical science is considered appropriate to the study of man, even though its concepts, are fully as much derived from the physical universe as were those of Newtonian mechanics.

And, frankly, the third part is completely unsatisfactory. The foundation that Matson puts forth for the study of man turns out to be a mixture of neo-Freudian psychoanalysis, existentialism, and the old German distinction between *"Naturwissenschaft"* and *"Geisteswissenschaft."* These may be important, but their inability to give us anything that could be called a methodology for a science of man, or for any real knowledge of man and of his society, has been established. It is precisely because we got nowhere by using these foundations that we have swung over towards the purely empirical, the quantitative and manipulative study of man and society. And reluctant though I am to admit it, whatever results we have had have come through this approach.

Of course, Matson is not alone in this failure to provide an alternative, and human, approach to the science of man. He shares it with all those who have tried to develop an adequate modern concept for the study of man (and I am included among those who have failed). What Matson does, however, is to point up the fundamental questions that confront the student of man and of society today. That the empirical, purely quantitative approach towards man and society as "Great Machines" is inadequate is fully proven. The political process and pruning on a minor piece of legislation can be expressed in a vectorial analysis. But the emergence of new leaders or misleaders—whether a Hitler, a Roosevelt or a Castro—or the transformation of goals that underlies the current Negro revolt in America defy behavioral analysis. But this does not mean that behavioral science is altogether invalid, only that there are limitations to it. To a substantial extent man—and his society—are mechanical and can be studied by mechanical means. But where is the line between those areas that are accessible to the approach of the behavioral scientist and those that are not?

The basic assumptions for the study of man and society, as specifically human and social, are also reasonably

clear. The study of man differs from the study of physical phenomena in that the aim must not be manipulation but self-control. It is concerned not merely with knowledge, which is external, but with "understanding" as well. But is this altogether capable of being organized scientifically, indeed can it ever be so organized? Or, as a good many of the most able physical scientists today would probably surmise, are these areas of understanding and of self-control essentially areas of experience rather than of knowledge —that is, are they the domains of poet, artist, and saint rather than of researcher and professor? Matson does not raise these questions, but he makes it clear that these are the real issues and that so far no scientific approach has been found to the understanding and self-control of man, of his society, his behavior, and his values.

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Crystallography

Crystallography and Crystal Perfection. Proceedings of a symposium held in Madras, 14–18 January 1963, and organized by the University of Madras. G. N. Ramachandran, Ed. Academic Press, New York, 1963. x + 374 pp. $12.

It is obvious that the editor must have been hard put to find a suitable title: a glance at this book will show the magnitude of his predicament. Thirty mostly unrelated papers, by 55 authors representing about 30 institutions, were presented at a symposium on crystallography. (Another symposium—on protein structure—held at the same time, is presented in a companion volume.) These activities testify to the vitality of Indian crystallographers: about one-third of the contributions come from Indian research centers. The list of authors contains many names famous among crystallographers. In Section 1, Phase Problem, are papers by M. J. Buerger (image functions), Dan McLachlan, Jr., (optical devices for phase determination), G. N. Ramachandran and R. Ramachandra Ayyar (Fourier series for isomorphous crystals and anomalous dispersion), I. Nitta *et al.* (sign determination by statistical method), W. Hoppe *et al.* (the "shift product method" and structure-factor signs by Sayre's relations), S. Raman and W. N. Lipscomb (the Patterson approach), R. Srinivasan *et al.* (tests for isomorphism), and W. Cochran *et al.* (EDSAC program).

Section 2, Crystal Perfection, contains papers by G. Borrmann, L. V. Azároff, S. Chandrasekhar, R. Parthasarathy *et al.*, M. Renninger, and N. Kato, who contributes a unified treatment entitled "Wave-optical theory of diffraction in single crystals." Section 3, Crystal Disorder, contains four papers: H. Jagodzinski (disorder phenomena), I. Waller (effect of impurities on neutron scattering), P. Krishna and A. R. Verma (silicon-carbon polytypes), J. I. Langford and A. J. C. Wilson (variance and line broadening). In section 4, Anomalous Dispersion, J. M. Bijvoet *et al.* take a second look at sodium chlorate and sodium bromate antipodes of the same sign and find that they have opposite configurations; D. Dale, D. C. Hodgkin, and K. Venkatesan report the structure of Factor V 1a, which is an aquocyanide of natural vitamin B_{12} nucleus (containing Co); S. N. Vaidya and S. Ramaseshan comment on procedures used in the Bijvoet method. A paper on electron diffraction by S. Miyake *et al.* and two papers on neutron diffraction, by J. Shankar and V. M. Padmanabhan and by P. K. Iyengar (from the Atomic Energy Establishment at Trombay, India), constitute section 5.

The Wooster family describe their automatic x-ray diffractometer in section 6, Instrumentation; an integrating Weissenberg camera for low and high temperatures is also described, by A. K. Singh and S. Ramaseshan, in this section. Four miscellaneous papers are collected in a last section: elasticity of cubic crystals (by J. Laval); infrared and Raman spectra of glycine and its addition compounds (R. S. Krishnan and P. S. Narayanan); infrared absorption in ionic crystals (S. S. Mitra); antitensors of first and second kinds and their symmetries—ten groups for each kind (I. S. Zheludev). Each contribution is accompanied by a summary of the oral discussion that followed it. A 5-page index lists the names of over 400 authors whose works are cited. The volume is beautifully printed, illustrated, and bound. A crystallographer can hardly afford not to buy it.

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Russian Translation

The Theory of Mathematical Machines. Yu. Ya. Bazilevskii, Ed. Translated from the Russian edition (Moscow, 1958) by C. A. Hoare. J. M. Jackson, Ed. Pergamon, London; Macmillan, New York, 1963. xii + 264 pp. Illus. $10.

This is a translation of a book published in Russia in 1958. It is comprised of a preface and eight papers. The principal contributors are the editor who wrote two papers and participated in one other, I. Ya. Akushskii who wrote two papers, and Yu. A. Schreider who wrote one paper and shared one with the editor. As is customary with symposia, the papers are to some extent expository. It is my impression that the last four papers are mainly of the expository category. The first four, however, have more claims to originality.

In the preface the editor presents an outline of the theory of mathematical machines. According to him this theory consists of three parts. The first part consists of the study of the logical structure of mathematical machines. The second is the theory of programming, and the third is the study of the means of physical implementation of a given logical structure. He says that "The first and second parts may be considered as a branch of cybernetics which deals with methods of implementing algorisms as distinct from such branches as, for instance, mathematical linguistics and biology, which are concerned with the creation or the discovery of algorisms. The content of the third part lies outside the framework of cybernetics." This statement, granted accurate translation, is rather stunning, but perhaps this is because I have been much more concerned with numerical methods than with logic. It seems to me that an almost automatic consequence of implementing algorisms is the creation of new ones. Moreover, in this we see that the interpretation given cybernetics in Russia is broader than that accorded it in the United States.

The distinctive features of the first paper, "The theory of sequential logical functions," by Bazilevskii, seem to be in the methods of introducing sequences (that is, time lags and feedback) and in the study of reductions of sequential logical functions through use of periodicities as well as other devices. Although I have not attempted to con-