Book Reviews

Life and Work of Ronald Fisher

R. A. Fisher. The Life of a Scientist. JOAN FISHER BOX. Wiley, New York, 1978. xiv, 512 pp. + plates. $24.95.

Joan Fisher Box has written a finely crafted and richly detailed account of her father's life that will be of great value to scholars seeking to understand the rapid growth of the fields of statistics and genetics. Fisher was a major contributor from 1912 to 1962, with three innovative books in statistics, two in genetics, and about 300 papers. Much of Box's book is devoted to clear and semitechnical exposition of the scientific issues Fisher addressed and to accounts of how Fisher was led to them, first as a student and later as the first statistician at Rothamsted Experimental Station (1919–1933), Galton Professor of Eugenics at University College, London (1933–1943), and Arthur Balfour Professor of Genetics at Cambridge (1943–1957). Box presents a balanced view of Fisher's character, showing that he was often unfairly quick to take offense with his family and colleagues, but also documenting his personal vitality, loyalty, and love of stimulating intellectual discourse.

Fisher was born in 1890 to a well-off London family. His extraordinary mathematical abilities were recognized early and were fostered through excellent schooling at Stanmore Park and Harrow. Proceeding to Cambridge, shortly after his mother's death and his father's financial ruin, Fisher obtained a first-class degree in mathematics while starting to develop his ideas relating Mendelian genetics and biometrics. Unable to join the armed forces, he spent the war years supporting himself by schoolteaching and farming, before formally starting his professional career at Rothamsted in 1919.

During the war years, Fisher managed several publications, including important papers on the sampling distribution of the correlation coefficient (1915) and on the implications of Mendelian genetics for empirical correlations between relatives (1919). The Rothamsted years saw an astonishing outpouring of highly original work. Using his deep understanding of n-dimensional geometry, he derived many small sample distributions, which are the bread and butter of contemporary statistics. He also founded modern mathematical statistics with an important series of papers on foundations that introduced such basic concepts as likelihood, efficiency, and sufficiency. Box reports that this work gained him election to the Royal Society in 1929 as a mathematician. The mainspring of his work was, however, the deep logical insight he brought to bear on real scientific questions, mainly in the biological sciences, from which the papers in pure theory were simply a spin-off. On the applied side, he devised the basic statistical tool called the analysis of variance, and he introduced randomization and factorial experimentation, thus founding modern statistical design and analysis of experimental data. Along with all this, he found time to introduce important mathematical techniques as well as revolutionary scientific concepts into genetics. In his subsequent professorships in London and Cambridge, and in retirement in Australia until his death in 1962, Fisher kept up a steady output in the many fields where he established leadership.

Fisher was an early and major actor in the controversies that have rocked statistics as the field has struggled to assimilate the major theoretical advances of the 20th century. Fisher demanded a great deal of his listeners and readers. Deep and subtle ideas were so plain to him that he often failed to create the explanations required by less able minds, who were therefore often reluctant to embrace important contributions. Also, Fisher disdained mathematics for its own sake and opposed teaching of statistics in an environment separated from direct contact with ongoing applications. He fought hard against the Neyman-Pearson school of statistical theory, which developed and achieved dominance during his working life, because he perceived that the frequency theory of statistical inference was too enamored of abstraction and too distantly related to practice. Fisher apparently lost most of his academic battles, and although his ideas permeate statistical science they are rarely taught in the form in which he understood them. He has left many tracks, however, especially in the English-speaking world, since he traveled widely in North America, India, and Australia. The tendency of mathematical sciences to diverge from the real sciences that spawn them is a major problem, not, as Fisher misguidedly thought, because these mathematical sciences do not deserve a life of their own but rather because the resources of mathematical talent that are brought to bear on the interfaces with real sciences are all too skimpy. Fisher will repay much study, not only for his ideas but also as a shining example of how to bridge the gap between mathematics and science.

Box tells her father's life sympathetically but fairly and very much as he saw it. As a consequence, her biography does little to set his work in context. Fisher himself was not much of a historian and was largely unaware of important antecedents of his own work, such as the work of Edgeworth, even quite close to his own time. And he saw his differences with others to be so great that he did not see how important his own ideas were to his opponents. The time will soon be ripe for further works on Fisher that will clarify the place of this undoubted giant in intellectual history.

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A Public Institution

Social Change and Scientific Organization. The Royal Institution, 1799–1844. MORRIS BERMAN. Cornell University Press, Ithaca, N.Y., 1978. xxviii, 224 pp., illus. $17.50.

In the spring of 1799 a group of wealthy Englishmen agreed to back Count Rumford's proposals for a "Public Institution for diffusing the knowledge and facilitating the general introduction of useful mechanical inventions and improvements, and for teaching by courses of philosophical lectures and experiments the application of science to the common purposes of life." The target and beneficiary of this Royal Institution were to be the laboring classes. They did not long enjoy its solicitude. In place of educator to the masses, the RI became entertainer to the fashionable and consultant to the interests that supported it.

Berman has determined these interests by compiling biographies of the RI's patrons and officers. His painstaking prosopography shows that the original supporters were predominantly improving agriculturalists. As their influence waned, physicians and especially lawyers came forward, men who expected that the social problems of industrializing England could be solved by the applied science, or rational method, that the RI represented. A high proportion of its officers—some 40 percent in 1840—supported Utilitarian projects such as the University of London, the Statistical Society, and the Society for the Diffusion of Useful Knowledge. Berman infers that during the 1830's the RI was governed by Benthamite social engineers.

The interests of the RI's officers affected, and sometimes directed, the work of its staff. According to Berman, Humphry Davy succeeded by placing himself at the service of the agriculturalists. His *Agricultural Chemistry*, his studies of tanning, his analysis of feed grains established his position: first a reputation for analyzing manures, then glory via electrolysis. In 1809 he lobbied for the building of a large new battery on the ground that electricity was a factor in soil fertility.

The work of Thomas Brande, professor of chemistry from 1813 to 1831, and of his successor, Michael Faraday, reflects the interests of the professional men. Rather than agricultural materials, Brande and Faraday tested metals, glass, materia medica, and illuminating gas. They acted as expert witnesses in legal proceedings involving technological questions. They taught medical students and lectured to young lawyers. They helped to reproduce the class they served.

Berman sets forth these patterns clearly and persuasively. His is a solid accomplishment. And it is the more to be praised, or wondered at, because the interpretation he places on his findings suffers from blurred distinctions, bad history, and old new-left melodrama.

Much of Berman's interpretation turns on a conflation of science, technology, data collection, and rational problem-solving. His definition of science runs from poor relief to natural philosophy; it includes cut-and-try methods, collection of statistics, and economic policy. The conflation serves the thesis that the RI's chief work, and its world-historical significance, were to act as carrier of a new "ideology of science." First the improving landlords, then the social engineers, subverted the "amateur tradition" of Enlightenment science. The RI became a "crucial precedent": in it science changed from "avocation" to "enterprise." Such reasoning fetches the grandiose conclusion that the RI and institutions like it determined "the direction of scientific activity."

This is bad history. The organization of the study of natural philosophy owed little if anything to the RI and its descendants, and no major technological innovation came from them. The RI was not the seed of modern science or its ideology but a sprout from institutions characteristic of the 18th century. Berman does not mention the Society of Arts, founded before 1750 to encourage the application of experimental philosophy to trades and manufactures, or the emphasis on applied science in Enlightenment encyclopedias, or the technical curricula in continental mining and military schools. He appears not to know of the accomplishments of the Réaumurs, Wedgewoods, Watts, and Achards. He writes that Bacon's "idea of a marriage between science and industry [was] almost totally submerged in the course of the 18th century."

Ignorance of the 18th century also infects Berman's definition of science. He allows only two alternatives: either amateur, the admiring of "some shells or curiosities," or entrepreneurial, the ideology of the RI's officers. Into which camp shall we put Aepinus, Cavendish, Coulomb, Volta, Lichtenberg, and their like? And what can we make of Berman's assertion that, before the foundation of the RI, lectures on science were given only at philosophical societies? His own data expose this blunder. One of the RI's first professors proposed to pattern his "scientific course of experimental philosophy on the plan generally adopted in the universities."

Berman's bad history and over-broad conception of science serve a neo-Marxist analysis of modernization. "Science" is a tool of the oppressors of the working class. They placate the hungry oppressed with bones from their scientific soup kitchens; they control the belligerent oppressed by gas lighting and bureaucrats. One of their favorite ploys is the cover-up. They collect statistics, employ social engineers and hygienists, hire experts, all to mystify the common person, to imply that the ills of society are curable "technical difficulties" rather than diseases "endemic to the structure of industrial society."

Science is not responsible for belief in the expert or for the rise of modern capitalism or for the industrial revolution. It did not bring record-keeping, rational problem-solving, or bureaucrats. And it did not become an effective social force until after the time of which Berman writes. The RI was one of many premature attempts to promote the application of science to technology. It claims attention for the scope of its efforts and the quality of its staff, not as the governor of science or the yoke of the working class.

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Life in Thermal Habitats

Thermophilic Microorganisms and Life at High Temperature. THOMAS D. BROCK. Springer-Verlag, New York, 1978. xii, 468 pp., illus. \$25.80. Springer Series in Microbiology.

This book is a remarkable document. It is primarily a synthesis of the 90 or so papers that came of the ten years that Brock spent studying life at high temperatures, particularly in Yellowstone National Park. The book also incorporates the work of others. Its scope is great, ranging from molecular biology to ecology to geochemistry. It is a book that flatters the ecological approach to microbiology and shows where it can lead.

The chapters "The habitats" and "The organisms" are general overviews. Chapters on the key genera (*Thermus, Thermoplasma, Sulfolobus, Chloroflexus, Cyanidium*) and on the thermophilic blue-green prokaryotes are well illustrated and contain detailed descriptions of the ecology, physiology, and biochemistry of the organisms. The chapter "Life in boiling waters" is an informative and provocative account of the bacteria that Brock proved were thriving in waters over 90°C but that so far have not been cultured or characterized. Brock includes a chapter on the formation of hot spring prokaryote mats, which appear to be analogous to Precambrian stromatolites. There are also chapters on the microbial world at low *p*H and on the calefaction of the Firehole River of Yellowstone, a case of natural thermal pollution.

It is the highly personal style of this account that makes the book remarkable. It is a readable and satisfying narrative of how ideas were generated, how experiments were done, and how mistakes were made. Brock takes time to give historical perspective and to cover many unusual but interesting aspects of his work. An example is a detailed, 2½-page history of the reviews and revisions of his paper first describing *Sulfolobus*