Modes of Leadership

Contrasts in Scientific Style. Research Groups in the Chemical and Biochemical Sciences. JOSEPH S. FRUTON. American Philosophical Society, Philadelphia, PA, 1990. xii, 473 pp. $40.

In his Nobel Prize address in 1902, the great sugar, purine, and protein chemist Emil Fischer observed that the mass production methods that had come to dominate modern life had inevitably entered into scientific practice. Scientific progress, he commented, was no longer determined by brilliant personal achievements, but rather through planned collaboration with teams of workers. Today both scientists and historians of science commonly refer to collaborative research as emanating from "research schools," or, as Fruton prefers to call them in order to restrict meaning to particular institutions, "research groups." Such collaborative research programs in organic, agricultural, and physiological chemistry can be traced back to Justus von Liebig and his laboratory at Giessen between 1824 and 1852. This is Fruton's starting point, for Liebig's form of organization, institutionalization, and publication (through *Annalen der Chemie*) quickly became the hallmark of scientific teaching and research in German universities and served as a model for other countries to adopt and to adapt.

It is therefore of some interest to ask, as Fruton does in his valuable and enlightening monograph, how the post-Liebig research groups who exploited chemistry and physiology to elucidate problems in biology, medicine, and agriculture actually worked. What (if any) different styles of research leadership were chosen by the German pioneers of chemical physiology and biochemistry such as von Liebig, Felix Hoppe-Seyler, Willy Kühne, Adolf von Baeyer, Fischer, and Franz Hofmeister? How far are such differences perpetuated, and with what consequences, in more recent research schools in Europe and America?

Using published and unpublished reminiscences, biographies, autobiographies, obituary notices, university archives, and scientific publications, Fruton amasses a rich body of data which, besides their use in answering his own questions, will be of considerable value to historians of education and science who are engaged in different tasks from his. Nearly 150 pages of the monograph are taken up with prosopographical information on the students who took their degrees with the six leaders chosen for study, or who published papers from their laboratories, or who were simply acknowledged in their leaders' publications. (For the record, this gives the following numbers of names: Liebig 348; Hoppe-Seyler 135; Kühne 59; Baeyer 617; Fischer 354; Hofmeister 72.) Although Fruton will undoubtedly be criticized for his criteria of inclusion and exclusion in such lists, the data are a monument to his industry and scholarship and will insure the monograph's usefulness as a reference work.

Sociologically, it is clear that research leaders gain status from their students and research assistants in exchange for finding them jobs, and in the reflection of their later independent work in academia or industry. To be recognized as a "Fischer student" was apparently often more valuable as a career passport than research originality. Another important finding is that the tendency to see leaders like Liebig and Baeyer as lone investigators is corrected as the work of their contemporary junior investigators is fully exposed. For example, Fruton's research confirms my suspicion that the influence of Liebig's colleague Heinrich Will has been seriously underrated by historians of chemistry.

Fruton identifies a difference between leaders like Hoppe-Seyler, Baeyer, and Hofmeister, who were liberal and encouraging toward independent research in their laboratories, and those like Liebig, Kühne, and Fischer, who worked on a narrow front and were autocratic and given to seeing all students' work as their own. Fruton traces the same patterns of leadership in the pupils who founded their own research groups, though continuity of research topic was rarely maintained. For example, while Otto Warburg adopted Fischer's style of leadership, under the influence of the new physical chemistry he moved away from protein chemistry to cell respiration.

If Warburg is an extreme example of the despot (as Hans Krebs's memoir testifies), present-day laboratory power is more happily based on mutual respect and affection for the past achievements as a leader. Although today's successful leaders have often risen to public renown more for their entrepreneurial skills than for their scientific genius, history suggests that this business acumen needs tempering with the attitudes of Baeyer or F. M. Hopkins, who encouraged a broad front of research in their laboratories. The analogy with the experience of 20th-century chemical industry, where research, manufacture, and marketing on a broad front have led to competitive success, is striking. But this merely confirms Fischer's Nobel observation.

Fruton's volume, which adds an important social dimension to his *Molecules and Life: Historical Essays on the Interplay of Chemistry and Biology* (1972), has been given the 1990 John Frederick Lewis Award of the American Philosophical Society.

W. H. BROCK
Beckman Center for the History of Chemistry, Philadelphia, PA 19102

The Lesser-Known Bohr

Redirecting Science. Niels Bohr, Philanthropy, and the Rise of Nuclear Physics. FINN AASERUD. Cambridge University Press, New York, 1990. xiv, 356 pp., illus. $47.50.

This book is a professional historian's study of the happenings at the Niels Bohr Institute in the decisive years 1930 to 1940. The author has dug up all the relevant documents wherever they were located and gives us an easily readable account of his findings. In particular, the documents referring to the financial support of the Institute by Danish and other foundations, mainly the Rockefeller Foundation, are treated in great detail, revealing many interesting aspects of these relationships. We learn how the Rockefeller Foundation changed its policy around 1930 from support of successful scientists such as Bohr wherever their interest might lead them to support of special fields of science. This change was initiated by Warren Weaver, who wanted to support mainly biology. The book describes the uncanny talent of Bohr in obtaining funds also for physics from the Rockefeller Foundation—the main support of his Institute in spite of the change in policy. Bohr always expressed great interest in the fundamental philosophical questions of biology, such as compatibility or complementarity of life phenomena with physics and chemistry. But his main requests for funds from the Rockefeller Foundation in those years were based on the presence in Copenhagen of George Hevesy, who introduced the radioactive tracer method, a most useful tool for biology but very far removed from the fundamental biological problems that were on Bohr's mind. Bohr used Hevesy's need for cyclotrons and other accelerators as sources of radioactive tracers in order to get the means for also doing pure nuclear physics research with these instruments.