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Using published and unpublished reminiscences, biographies, autobiographies, obituary notices, university archives, and scientific publications, Fruton amasses a rich body of data which,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.

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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.

Having spent two full years and made many visits in Copenhagen during the 1930s, I found the book most revealing, since Bohr did not tell much about his dealings with the sources of financial support to us young collaborators. Still, as an eyewitness of this period I feel that this book has the usual shortcomings of an account taken from written sources. On another occasion I wrote of Bohr "acting, talking, living as an equal in a group of young, optimistic, jocular, enthusiastic people, approaching the deepest riddles of nature with a spirit of attack, a spirit of freedom of conventional bonds . . . that can hardly be described." Here I miss a sense of that incredibly inspiring atmosphere at Copenhagen. For example, Aaserud does not mention at all the characteristic evenings of "comic physics" that took place at the yearly conferences.

I also think that the main title of the book is misleading. We did not feel at all that Bohr "redirected" science at Copenhagen. The shift that occurred in the period Aaserud is concerned with came as a natural development of fundamental physics, just as Bohr's interest in quantum electrodynamics around 1930 was a natural step forward from atomic quantum mechanics. That was no "redirection" either. True enough, Bohr undertook the natural expansion of quantum mechanics to nuclear problems later than it could have been done. His enthusiasm about the complementarity between the wave and the particle nature of the electron led him to believe that the riddles of nuclear structure and of biology should also be understood by broader complementarity relations. He thought that life phenomena were complementary to the laws of physics and chemistry, since any atomic analysis of life necessarily would destroy it, just as any attempt to localize an electron as particle destroys its wave properties. This idea turned out to be wrong, but it had decisive though indirect effects on life sciences that are not mentioned in the book; it brought Max Delbrück to switch from physics to biology, to become one of the founders of molecular biology. This influence is unconnected to the biological activities of Hevesy for which Rockefeller provided the funds.

Around 1930 Bohr had the strange idea of giving up the law of conservation of energy in order to resolve some of the nuclear problems. Pauli contemptuously attacked this as "the Copenhagen heresy." But it took only a few years for Bohr to recognize that the nucleus is an ordinary quantum mechanical system. No new complementarity was necessary for its understanding. Indeed, Bohr contributed much to this understanding—for example, the concept of compound nucleus and his analysis of fission with J. A. Wheeler. In my view these ideas and the experimental work of O. R. Frisch, H. Kopfermann, and others at Bohr's institute should not be considered a "redirection" of research but a logical continuation of the application of quantum mechanics to newly discovered phenomena.

Niels Bohr "takes a symbolic first step toward expanding the institute, 1935/36." [From *Redirecting Science*; Niels Bohr Archive, Copenhagen, courtesy of American Institute of Physics Niels Bohr Library]

George Hevesy, around 1935. [From *Redirecting Science*; Niels Bohr Archive, Copenhagen, courtesy of American Institute of Physics Niels Bohr Library]

The detailed accounts by Aaserud of Bohr's negotiations are a testimony to Bohr's uncanny ability to get what he wanted from the various foundations. We young collaborators admired his incredible ability to lead research and at the same time to provide the necessary funds—and, last but not least, to provide us Hitler refugees with jobs. Every year Bohr traveled to America and England to "sell his refugees."

Aaserud's book is an invaluable source of information and of documents that prove that Bohr was not only an inspiring physicist and philosopher but also a cunning negotiator who knew how to make use of his great reputation for the benefit of science.

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The High-Latitude Oceans

Polar Oceanography. Part A, Physical Science. WALKER O. SMITH, JR., Ed. Academic Press, San Diego, CA, 1990. xviii, 406 pp., illus., + plates. $69.50

Polar oceanography differs from the oceanography of lower latitudes in several significant respects. A fundamental physical difference is that the upper layers of the polar oceans are stratified by salinity rather than by temperature. The logistics of oceanographic measurement are also quite differ