Book Reviews

A Career in British Science

Cockcroft and the Atom. GUY HARTCUP and T. E. ALLIBONE. Hilger, Bristol, England, 1984 (U.S. distributor, Heyden, Philadelphia). xii, 320 pp., illus. $34.

The atomic disintegration of lithium by John Cockcroft and Ernest Walton at Cambridge in 1932 was hailed by the British weekly *Reynolds News* as entailing "nothing less than the complete abolition of irksome manual labour and a new era of prosperity for all." Nuclear energy would be the solution to all man's problems. For the scientists, too, it was a golden age. "What a wonderful time it was then at the Cavendish!" recalled the visiting Soviet physicist P. L. Kapitza of those days. "The science belonged to the scientists and not to the politicians. Nowadays in spite of the terrific amount of money which is available for science, we do not enjoy our work nearly so much." Cockcroft, after a decade as a research scientist with Kapitza and Rutherford, went on to become one of the elite government science administrators characterized by C. P. Snow as "new men," retiring from government service in the '60's to become master of the embryonic Churchill College, Cambridge, which was ostensibly modeled on the Massachusetts Institute of Technology.

In 1968, Churchill College asked Cockcroft's former Cavendish and Metropolitan-Vickers colleague T. E. Allibone to undertake the present semiofficial biography, which is based largely on private correspondence and papers held by Churchill and made available by the Cockcroft family. Hartcup, author of *The Challenge of War: Scientific and Engineering Contributions to World War Two,* was a natural person to bring in on the project, especially to deal with Cockcroft's many wartime contributions and their postwar repercussions. He was also allowed by the U.K. Atomic Energy Authority "to make use of Sir John's personal files" and other records.

Cockcroft's principal contribution to the Cavendish laboratory in the late '20's and in the '30's was not necessarily as either a researcher or a tutor but as a designer and builder of equipment, much of it of vital importance. He was able to synthesize the considerable knowledge of electrical engineering he had gained at Metro-Vickers (as the former British Westinghouse was then called) with the experimental needs of the major British university physics laboratory, which was just about to leave behind the era of "string and sealing wax" equipment. In addition to the Tesla discharge tube for his and Walton's atomic disintegration experiment, he designed the coil for the huge pulsed fields that were crucial for Kapitza's magnetic laboratory and contributed also to the building of the helium liquifier for the latter's Royal Society Mond Laboratory, which became the center of the embryonic discipline of solid-state physics at Cambridge. His work was thus crucial to the development of two principal subfields of 20th-century physics, nuclear and solid-state. Subsequently, when in 1934 Kapitza was forced to remain in the Soviet Union following a summer visit, Cockcroft assumed the acting directorship of the Mond—in the same period in which, as junior bursar, he was directing the restoration and extension of St. John's College.

Had the war not intervened Cockcroft would no doubt have stayed at Cambridge. But his considerable administrative abilities and tact were needed for the mobilization of British scientists, first for the radar program and later for atomic weapons research. Toward the end of the war he directed the building of the nuclear power establishment at Chalk River in Canada, and after the war he was selected for a similar role at the new U.K. Atomic Energy Research Establishment at Harwell. His main qualifications for the latter post, according to his former Cavendish colleague and fellow Nobel laureate James Chadwick, were, "His knowledge is wide but it is not at all profound; his views are of rather a dull everyday hue. On the other hand his temper is so equable and his patience and persistence so inexhaustible that we can put in lively and relatively irresponsible men who have the real feeling for research without fear of upsetting the balance."

Indeed, as Allibone and Hartcup put it, this was essentially the same role Cockcroft assumed in relation to Walton in their famous disintegration experiment back at the Cavendish. The completion of the work seems to have been accomplished mainly by Walton, guided by theoretical ideas put forth by the visiting Russian émigré George Gamow, with Cockcroft serving mainly as a coordinator and gatherer of equipment. The present authors go so far as to remark that "Cockcroft, with all his other commitments, could not have done the experiments on his own; temperamentally, he was not a good experimental physicist and tended to be slipshod and forgetful."

As a filtered distillation of the extensive Cockcroft correspondence, the biography provides a tasty brew of information about the comings and goings of its subject, largely as seen from his own immediate situation and vantage point. Appropriately, less space is spent on his years at the Cavendish than on his far longer period as a government science manager and major proponent of nuclear energy. The reliance on the private correspondence, an additional smattering of recently declassified Atomic Energy Authority and Public Record Office documents, and the tributes of surviving colleagues is both the book's strength and—to my mind—its major weakness. The documentation and recollections provide such a plethora of quotable material that the authors experience considerable difficulty in placing it in the wider social and political context which would make it fully meaningful. The book also contains little about its subject's political views, though reading between the lines one gets the impression that he was content to concentrate on administration, leaving the overall decisions about radar and nuclear weapons to others.

Nevertheless, when asked whether it was morally right to develop atomic weapons after World War II, Cockcroft replied that the U.S.S.R. "has taken over the practices of the Nazi regime—the concentration camps, slave labour, the watcher in the street and everything which goes with that; [so that] until there is a settlement I believe we are justified in arming ourselves as strongly as the Russians." Or again, his biographers state that in 1955 "Cockcroft sought to allay public anxiety about the biological effects of nuclear explosions and the hazards likely to be encountered from the large-scale development of nuclear power." As a record of his public assertions this is appropriate enough, but the authors do not address the considerations that underlay such statements or attempt to weigh their effects, if any, on the debate about the dangers of continued nuclear testing or the subsequent introduction of large-scale nuclear power. Of course Cockcroft had played a central role in British decisions leading to the first nuclear reactors and also in the British rejection of the American pressurized water reactor. Interestingly, according to the authors, even in the early '50's it was believed by Cockcroft and others in the British program "that a water-cooled reactor, similar to that which the Americans had designed at Hanford, would be prone to runaway instability if the coolant water flow failed; the neutron flux would increase rapidly, overheating might ensue particularly if the shutdown mechanism failed, and then the atmosphere would be polluted by radioactive products."

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Physics 1905–1939

Otto Hahn and the Rise of Nuclear Physics. WILLIAM R. SHEA, Ed. Reidel, Boston, 1983 (distributor, Kluwer Boston, Hingham, Mass.). x, 254 pp., illus. $49.95. University of Western Ontario Series in Philosophy of Science, vol. 22.

This book contains a variety of papers to do with radioactivity and nuclear physics before the Second World War. The papers cover a period that begins in 1905 with Hahn's discovery of radiothorium and his trip soon afterwards to work with Rutherford in Montreal. It ends with the painstaking experiments of Hahn, Strassmann, and Meitner leading to the discovery of nuclear fission in the winter of 1938–39.

A lengthy paper by Roger Stuewer traces views about the structure of the nucleus from 1911 when Rutherford suggested its existence. Stuewer shows the prevalence of the view in the 1920's that there were electrons contained inside the nucleus alongside protons, the only other particle then known. The mysteries of these nuclear electrons deepened with the development of quantum mechanics and as new experimental data became available. These electrons did not seem to have their expected spin or magnetic moment inside the nucleus, and, most puzzling of all, they could be emitted in beta decay with a continuous range of energies, unlike the products of other radioactive decays. The discovery of the neutron in 1932 seemed only to exacerbate some of these problems and led to a debate about the neutron and whether it was a simple or complex particle. This question was only resolved with the help of another particle, the neutrino, suggested by Pauli and named by Fermi.

Many of the twists and turns in this complex story are followed by Stuewer. However, as he himself points out, the hypothesis of nuclear electrons was only one of the ingredients of the history of nuclear physics in those years. Other factors, for example the introduction of particle accelerators—the first seeds of "big physics"—were important, particularly during the work on the first atomic weapons a few years later.

Some of the ramifications come into the papers by Spencer Weart and Fritz Krafft on the discovery of fission. Weart points out that, though fission did have important consequences for the human race, it had few for nuclear theory. This leads Weart to try to identify a paradigm (a word he uses very cautiously) for the study of nuclear physics, which he does by examining the work of the groups in Rome, Paris, and Berlin who were bombarding uranium with neutrons in the 1930's. Part of this paradigm was the idea of transmutation, a concept with a long history. Consequently, Weart argues that there is continuity between the expectations of Hahn and his contemporaries and the end results of their research today. Baldly stated that conclusion is hardly contentious, but Weart hopes that further research into this and other paradigms will illustrate the myriad connections between an individual scientist and the society that supports him or her.

By contrast, Krafft focuses more closely on the personal histories of Hahn, Meitner, and Strassmann to explain both the policies and the accidental factors that led to the Berlin group's discovery that barium, rather than nuclei close to uranium, was produced when uranium was bombarded with neutrons. In Krafft's view, the long collaboration of the Berlin group (despite Meitner's having to seek refuge in Sweden) and the contribution of the analytical chemist, Strassmann, were decisive in their success.

Altogether, this is an interesting collection of papers. Some of the shorter ones, though deserving, cannot be discussed here. The differences of approach used by the various authors suggest that there are many arguments still to come, especially with the large number of publications on the history of fission we are likely to see by the end of 1988, the 50th anniversary of its discovery.

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Climate

The Global Climate. JOHN T. HOUGHTON, Ed. Cambridge University Press, New York, 1984. vi, 233 pp., illus. $49.50.

Throughout the 1970's atmospheric scientists from many nations were heavily involved in the Global Atmospheric Research Program (GARP), whose primary practical objectives were the development of global systems for the acquisition and processing of atmospheric weather data as well as the improvement of global weather forecast models. As GARP has been phased out over the last few years, opportunity has been provided by the formation of the World Climate Program (WCP) for the scientific community to attempt a similar degree of international cooperation in the study of global climate.

The World Climate Research Program (WCRP) is the component of the WCP directed at promoting research on the physical processes of the climate system. *The Global Climate* contains comprehensive, and generally up-to-date and well-written, papers reviewing the research areas that make up the WCRP. The editor of the book is the chairman of the committee of scientists responsible for the design and oversight of the WCRP. The first chapter, by Houghton and Pierre Morel, gives an overview of the whole program, which has as its objectives to determine the extent to which climate can be predicted and the extent of human influence on climate. The program is divided into three "streams" of research according to time scale. The first "stream" is concerned with the physical basis for long-range weather forecasting, the second with interannual variability, and the third with long-term climatic trends and climate sensitivity.

Dividing the research according to time scale serves primarily to highlight the different degrees to which changes in ocean heat storage and transport may influence the rest of the climate system. Because the interior of the ocean has a