Book Reviews

Memoirs of Historic Accomplishments

All in Our Time. The Reminiscences of Twelve Nuclear Pioneers. JANE WILSON, Ed. Educational Foundation for Nuclear Science, Chicago, 1975. iv, 236 pp., illus. Paper, $3.45. Reprinted from *The Bulletin of the Atomic Scientists.*

Here are 12 scientists, today much honored by position and accomplishment, writing a few pages each on what was probably the most exciting thing that happened in their lives. Three of these scientists, Philip Abelson, Luis Alvarez, and Martin Kamen, write about the time of the rapid development of the cyclotrons at the University of California at Berkeley and their use to produce one new radioactive element after another. It was almost true that a successful experiment or new discovery was made each day. Kamen recalls, "For those few short years before Pearl Harbor the Radiation Laboratory was one of the great, uniquely happy, experiences in the history of natural science." Kamen and Samuel Ruben crowned their exciting experiences in the Radiation Laboratory with the discovery and identification of radiocarbon, ^{14}C, which was later to be shown by Willard Libby to be uniquely useful for dating prehistoric man and glacial advances and retreats.

Philip Abelson remembers his exciting days at the Radiation Laboratory to be especially centered on the discovery of fission of uranium, a discovery made in Germany by Hahn and Strassmann. Abelson had been seeking to identify transuranic x-rays by using an x-ray spectrometer which he had built himself, and it was one of the first. But he learned of the news that neutrons, captured by uranium, largely broke the uranium nucleus into fission fragments, and for him it was a great shock. Luis Alvarez read it in the newspaper and told Abelson, whereupon "I almost went numb as I realized that I had come close but had missed a great discovery.... By the end of [the next] day, I was able to identify the 'transuranic' x-ray as being a characteristic x-ray of iodine" (which is a fission product). Within about five days Abelson published this discovery in the *Physical Review* in a letter ten lines long, which he admires for its brevity. Abelson is now the editor of *Science* and has suffered through many submitted articles much lacking in brevity.

Luis Alvarez is now a Nobel Prize winner for his discoveries, with his collaborators, in which he used a boldly scaled-up giant bubble chamber to study interactions in beams from the high energy particle accelerators. He probably should have had the Nobel Prize long before for such discoveries as the radioactive hydrogen isotope of mass 3 and invention and construction of the linear accelerator with components left over from the wartime radar project, not to mention the universally used ground-controlled approach landing system for airplanes in conditions of low visibility.

Alvarez had been a graduate student of the Nobelist Arthur Compton's at the University of Chicago. He remembers the climate for research at Chicago in the mid-1930's as exceedingly individualistic. "There was... a feeling that each person's research problem was his own, and I can't ever remember giving or receiving advice on how an experiment could be done better or more efficiently." In contrast, what has remained impressive to him about the prewar Berkeley scene is that there the research work of almost everyone was discussed objectively in a friendly, nonpossessive spirit by everyone else. Alvarez realized "how much we were missing in Chicago by not having our work examined critically by our friends." He says of the cyclotron, "Important new physics was done there every day—physics that couldn't be duplicated anywhere else in the world." Alvarez moved to Berkeley as fast as he could and was there when he was awarded the Nobel Prize. He feels strongly that his early experiences at Berkeley were of prime importance in molding his lifelong characteristics as a scientist.

The other nine scientists whose writings are contained in this volume describe their work in helping to design, engineer, and test the first nuclear weapons. These nine include then-senior scientists such as Otto Frisch and Kenneth Bainbridge, juniors who had attained first appointments at universities, such as John Manley and R. R. Wilson, fresh postdocs like Herbert Anderson, Albert Wattenberg, and Boyce McDaniel, and finally two who did not yet have doctoral degrees, Frederic de Hoffmann and Val Fitch.

Through their eyes we have views of the pressures, stresses, attitudes, and gratitudes which concerned this broad spectrum of collaborators.

Bainbridge's ever-present concern was that if the nuclear device failed in its test at Alamagordo he would have to go up the tower and disarm it.

Boyce McDaniel describes the assembly of the bomb components at the tower, delayed by the apparent misfits caused by differences in temperature, to the final minutes when "with fear and trepidation I made the trip to the top and returned safely."

Frisch, a rather late comer to the laboratory from England, is certain that he has "never found such a concentration of interesting people in one place" as at Los Alamos. Frisch, together with Lise Meitner, first showed theoretically that fission was energetically possible and was the answer to the chemical puzzles demonstrated by Hahn and Strassmann. The chemical puzzles were of course caused by production of fission products instead of transuranic elements when uranium was bombarded by neutrons.

Manley remembers the many diverse jobs, the "jumble of excitement, long hours, and fatigue," but principally the "singleness of the technical objective comprised of many problems which could so easily be shared with sympathetic and understanding colleagues," and most strongly the "rapid transformation" of the theorist Robert Oppenheimer into "a most effective leader and administrator." Manley says of his association with his boss the new director of Los Alamos, "Thus began a long association and friendship from which I learned much and which had a deep and positive impact on my own career."

This admiration is shared by R. R. Wilson, who writes, "I was soon caught up by the Oppenheimer charisma, became a loyal and devoted lieutenant, a confidant, a friend.... In his presence, I became more intelligent, more vocal, more intense, more prescient," as was so for many others.

Oppenheimer is immortalized not only by his brilliant directorship of Los Alamos, where he undertook to understand and help solve the major technical problems of the laboratory and stimulate and also maintain the single-minded collaboration of scores of prima donna greats among the physicists and engineers of the laboratory. Previously at Berkeley and Pasadena he had assembled perhaps the largest school of young theoretical physicists the United States has ever had, stirring their curiosity, intelligence, and ambition to achievements that are now historic.

It was the youngest members that were most influenced by the years of the Manhattan District in the '40's. Like Alvarez at Berkeley in the '30's, they were then in formative stages of their education as scientists.

Wattenberg's detailed account of the early technical operation of the Manhattan District at Columbia, the transfer to Chicago, and some of the many experiments that were made at the rate of about one a day (as with the cyclotron in Alvarez's formative period), measuring physical constants vitally important to construction of the future nuclear reactors for plutonium production and engineering the nuclear weapons, is very clear. If you want to know something about how an experimental physicist lived then from day to day he tells you. He gives glimpses of Enrico Fermi's deep involvement and direction of this phase of the Manhattan District's task. "Fermi ... kept track how the pile was [building], and he optimized the placement of the highest quality [purest, most dense] material ... Fermi established that we had built a self-sustaining nuclear chain reaction and that it quantitatively responded to the control rod ... [He] had explained these things to us in lectures early in the fall. We now had the fun of seeing these things happen."

Fermi worked on whatever was going on that interested him. Every six weeks or so Fermi would call a meeting, at which he wrote on a blackboard a list of measurements and activities that needed attention. Everyone could choose what he would try to do. After a few of these meetings it became predictable that Fermi would collaborate with those who chose the most difficult and far out (possible negative or no-result) projects. The few who did had the most excitement.

One of the writers in this volume without a doctorate at that time, de Hoffmann, was plucked from his senior year in physical chemistry at Harvard and at Los Alamos was processed through a sort of "grand final exam day, with all the senior faculty members of all the U.S. and European physics faculties assembled to give ... that final exam." He remembers that "a sense of research excitement permeated all of Los Alamos—the like of which I have not known since ... a sense of common mission." He and Fitch are both grateful for the complete access all Los Alamos scientists had to the nuclear theory and technology as it developed. De Hoffmann says it was "remarkable" and that it was "such an ideal atmosphere ... that it has left an indelible impression." Fitch gratefully remembers the camaraderie with great scientists he, then an enlisted man assigned to the Special Engineer Detachment, enjoyed on the ski slopes above Los Alamos. He felt then and, now a much respected professor at Princeton, still feels gratitude that "the complete intellectual integrity required in the pursuit of physics carried over into the personal relationships of physics." This fundamental principle of sharing knowledge, of equality of intellectual access, was strongly supported by Edward Teller from the earliest days of the Manhattan District, and it undoubtedly contributed in a major way to the success of the wartime nuclear energy project. Characteristically Teller, for one, has expanded his earlier position of no secrecy between scientists to campaign for no scientific secrecy whatsoever, not even between nations. He argues that without secrecy national scientific development and innovation will progress more rapidly, to the national benefit.

The longest and most comprehensive account is that of Herbert Anderson. Within a few months of Fermi's arrival at Columbia in 1939 Anderson had begun to work with him. He gives warm insights into the thought processes of some of the great scientists of the Manhattan District. For example, "Pronouncements by experts, who claim that something cannot be done, can irritate a man like [Leo] Szilard, and a statement by Rutherford to the effect that atomic energy could never be released on a large scale set him to thinking how he could prove otherwise." And, for example, "Szilard liked to say ... that Fermi's idea of being conservative was to play down the possibility that the chain reaction would work; but that his idea of being conservative was to assume it would work and then take all the necessary precautions." With Fermi, Anderson found a closeness almost like that with an elder brother. He says "I was immensely drawn to Fermi. ... The fact that he could read me, and I him, made it easy and natural for us to work together. ... He wanted to wrestle with nature himself, with his own hands. He liked to have someone to work with ... we began a collaboration that continued happily ... until his death some 15 years later."

One hopes Jane Wilson will collect more reminiscences and publish them. One would like to hear from chemists (the writers in this volume are all physicists) and especially from the forgotten heroes, the engineers. For example, it is easier to conceive of an implosion than to make it occur, and to make it occur symmetrically and without instabilities. Very little publicity has been given to the magnificently successful engineering that characterized the achievements of the Manhattan District.

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Chemical Bonds

Hydrogen Bonding. MELVIN D. JOESTEN and L. J. SCHAAD. Dekker, New York, 1974. viii, 622 pp., illus. $45.

The purpose of this monograph is to provide a comprehensive survey of hydrogen-bonding studies subsequent to those reported and discussed in Pimentel and McClellan's book. A book that met this goal would be timely because the structural importance of hydrogen bonding is becoming ever more obvious. However, insofar as investigations in biological systems are an important component of the research carried out to elucidate the nature of hydrogen bonds, this book has failed in its primary objective. The authors have exhaustively reviewed studies in general organic and in inorganic species, but, with the exception of reporting some theoretical DNA base-pairing studies in chapter 2, they have neglected biological systems.

The book presents the general theory of the hydrogen bond well and also gives a useful brief survey of the techniques used to investigate the structural and thermodynamic properties of this type of interaction. In certain instances the emphasis given to subtopics is disproportionate. For example, experimental techniques for investigating the hydrogen bond might have been discussed in greater detail at the expense of the needlessly long introduction to the quantum mechanical theory of hydrogen bonding. Chapter 3, dealing with the thermodynamics and kinetics of hydrogen bonding, is probably the most informative and best-written portion of the book. The discussion of hydrogen bond energies is particularly interesting and useful. Unfortunately, in neither chapter 2 nor chapter 3 has attention been given to the various empirical hydrogen-bonding energy functions that have been developed to relate molecular geometry to hydrogen bond energy. A treatment of such functions might have served to relate the molecular and the macroscopic hydrogen-bond properties reported throughout the monograph. Correspondingly, the book would be more cohesive had the authors consistently attempted to rationalize observed thermodynamic and kinetic properties in terms of