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.

LEONA WOODS MARSHALL LIBBY
University of California, Los Angeles, and R & D Associates, Santa Monica

Chemical Bonds

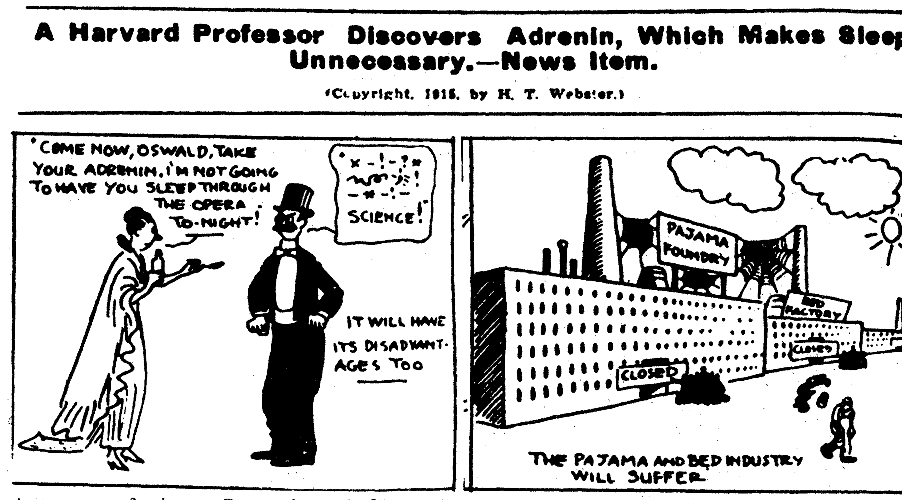
Hydrogen Bonding. MELVIN D. JOESTEN and L. J. SCHAAD. Dekker, New York, 1974. viii, 622 pp., illus. $45.

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 molecular geometry and stereochemistry.

The book should be useful to structural, physical, and organic chemists as a general reference. Classification by subject of the entries in the bibliography would have facilitated its use for this purpose. Workers in the biological sciences will find little information directly applicable to their fields, even though the title might imply otherwise.

A. J. HOPFINGER
Department of Macromolecular Science, Case Western Reserve University, Cleveland, Ohio



A cartoon referring to Cannon's work. [Reproduced from a Dallas newspaper in *The Life and Contributions of Walter Bradford Cannon, 1871–1945*]

Centennial in Physiology

The Life and Contributions of Walter Bradford Cannon, 1871-1945. His influence on the Development of Physiology in the Twentieth Century. Papers from a symposium, Brooklyn, N.Y., May 1972. CHANDLER McC. BROOKS, KIYOMI KOIZUMI, and JAMES O. PINKSTON, Eds. State University of New York Downstate Medical Center, Brooklyn, 1975 (distributor, State University of New York Press, Albany). xxii, 264 pp., illus. $20.

This volume is the proceedings of a symposium held at the centennial of the birth of Walter B. Cannon, summarizing his influence on the development of physiology in the 20th century. Three types of material are presented: summaries of important discoveries made by Cannon, reviews of current knowledge of some topics the study of which he initiated, and reminiscences and evaluation of him as a person. A list of Cannon's publications from 1897 to 1945 is appended.

A striking characteristic of Cannon's career was his logical transition from one research topic to another. He pioneered in the development of diagnostic roentgenography (radiation he received on his hands ultimately caused his death). He used x-rays to solve questions of esophageal and gastrointestinal motility, and for this he compared many species. He then spent years on the autonomic nervous system and demonstrated humoral transmission at sympathetic endings; his postulate of two types of "sympathin" was later modified but was empirically correct. During World War I he devoted himself to studies of shock. His next logical step was to study the central nervous basis for emotions, and in doing so he opened a large and continuing area of research. Related to this was analysis of thirst and hunger. Cannon was persistently holistic in his approach, and this culminated in enunciation of the principles of homeostasis, a concept that now permeates much of biological theory. He emphasized the role of sympathetic nerves and the adrenal medulla in responses to stress well before the role of steroids was suggested. His wide-ranging curiosity, industry, and research drive are clearly illustrated in numerous chapters in this book. He was a master at taking advantage of an unexpected turn of an experiment—serendipity. It is suggested that the reason he did not win the Nobel Prize may have been the diversity of his contributions.

The accounts of Cannon's contributions to roentgenology, to gastroenterology, and to the study of chemical transmission by sympathetic nerves are valuable contributions to the history of physiology. Some of the accounts of the present status of research, for example, on autonomic function, central representation of emotions, and the stress syndrome, diverge from the central theme of the book and seem somewhat forced. Even the final summary on "heroes in this age" is more Gerard than Cannon.

For many physiologists who, like myself, spent time in Cannon's department, the most interesting parts of the book are the accounts of Cannon as a man, as a leader of American science, as an international figure in physiology and medicine, and as a mentor. Chapters by Hallowell Davis, Bradford Cannon, and the editors reveal Walter Cannon's liberal character, his willingness to battle for good causes (exemplified by his continued opposition to the antivivisectionists), his leadership qualities. I experienced nostalgia in reading accounts of research done in the '20's and '30's and wished for more insight into how Cannon managed to maintain such quality in his staff. He insisted that medical students have a rigorous background in basic science before entering the clinics. His dogged emphasis on the international unity of science is well illustrated by his friendship with Pavlov, by his support of Spanish physiologists during the revolution, by his willingness to speak bluntly of matters of principle at international congresses. One must agree with Gerard in doubting that the next generation of physiologists will have such heroes as Walter Cannon.

C. LADD PROSSER
Department of Physiology and Biophysics, University of Illinois, Urbana

Radiation Chemistry

EPR of Free Radicals in Radiation Chemistry. S. YA. PSHEZHETSKII, A. G. KOTOV, V. K. MILINCHUK, V. A. ROGINSKII, and V. I. TUPIKOV. Translated from the Russian edition (Moscow, 1972) by P. Shelnitz. T. Pick, Transl. Ed. Halsted (Wiley), New York, and Israel Program for Scientific Translations, Jerusalem, 1974. viii, 446 pp., illus. $45.75.

Both neutral and charged free radicals are pervasive and important intermediates in the action of ionizing radiation on matter. Electron paramagnetic resonance (EPR) is generally the technique of choice for detecting, identifying, and monitoring them. Reactive free radicals can be most conveniently studied after stabilization in solid matrices. The spectra of radicals stabilized in single crystals can often be analyzed in considerable detail to obtain both isotropic and anisotropic hyperfine constants, to deduce the radical structure, and to identify radical-matrix interactions. In polycrystalline, amorphous, or glassy solids, however, the anisotropic information is partially or wholly lost and only gross isotropic features of the spectra are readily discernible. Nevertheless, analysis of spectra in such disordered media is of great value because single-crystal systems can-