Lawrence S. Kubie, who has written frequently about man's utilization of his talents, here writes in a chapter on the ontogeny of the dropout problem: "We have always known that there is a purely accidental relationship between erudition and maturity, or between erudition and wisdom. Is it now time to consider how to produce the maturity and the wisdom which will make the erudition possible?" Kubie elaborates on several ideas which would contribute toward making such an education possible. He suggests, for example: "We may have to find ways to use living as a preparation for schooling—i.e., as a way of maturing the student to a point at which he can profit from education. . . . This is the exact reverse of what we have taken for granted in the past: namely, that school is a preparation for life."

The emphasis on what the requirements of the educational system do to the individual should not obscure another important perspective. Roy Schafer, in a paper titled "Talent as danger: Psychoanalytic observations on academic difficulty," asks: "What are the compelling subjective reasons to avoid using one's talents?" and suggests that the freedom to develop may be also put in jeopardy by the individual's need to balance between his desire to know and his desire to avoid the danger of the unknown or the unacceptable.

Many of the papers in this book pose a significant challenge to the educational establishment. There is evidence that our society has developed such a voracious appetite for trained manpower that it is willing to sacrifice the development and well-being of individuals in an effort to meet short-term goals. Such a policy, according to these authors, is misdirected, not only because it threatens individual freedom but also because it is in the long run self-defeating. The highest levels of talent, as Kubie suggests, depend on the fullest possible development of the individual as a whole.

This book can be read with profit by all who are interested in the problems of higher education. They may feel what this reviewer experiences as convention indigestion—too many ideas and too many levels of discussions too rapidly presented. Despite this drawback, the papers are provocative, and at least a selective sampling is recommended.

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Cosmological Enigmas

Worlds-Antiworlds. Antimatter in Cosmology. HANNES ALFVÉN. Translated from the Swedish edition (Stockholm, 1966). Freeman, San Francisco, 1966. 109 pp., illus. $3.50.

The universe contains many puzzles. It appears to be expanding in a state of approximate uniformity and isotropy. Why? We do not know. It is usually believed to contain only matter, not antimatter. But physical laws are symmetrical for matter and antimatter. According to the theoretical discussions of Hawking and Penrose, the universe develops out of a singularity. But physicists like to believe that physical laws can be continued indefinitely into the past.

Alfvén attacks with his characteristic originality two of these enigmas. He suggests that we may be mistaken, that the universe, even our own galaxy, may contain both matter and antimatter, in equal amounts. He proposes that an originally uniform and mixed ionized medium becomes separated into fragments of matter and antimatter through the combined influences of electric currents and gravitational fields. He then proposes that the matter and antimatter bodies remain thereafter substantially separated through the influence of a type of Leidenfrost phenomenon, the generation between the bodies of a relatively thin layer of high-energy electrons. The pressure of this thin high-temperature layer separates the matter from the antimatter.

Concerning the singularity, Alfvén suggests that the universe starts as a collapsing gaseous medium that ultimately fragments into galaxies. It is proposed that in this collapse there is enough nonradial motion of the galaxies to reverse the collapse and start the universe expanding before an irrevocable collapse can occur.

As might be expected in such a novel approach to the basic ills of cosmology, Alfvén does not address himself to all the possible questions and difficulties that his ideas might suggest. Thus, the origin of purely matter or antimatter interstellar clouds derived from the ejecta of a very large number of both matter and antimatter stars could present a problem. Also, a nonuniform universe capable of reexpansion without violent collapse may encounter serious observational difficulties. The large red shifts of the quasars and the microwave background radiation might be difficult to deal with in this model.

Despite the complexities of these difficult cosmological problems, Alfvén has successfully directed this book to the nonscientist. Devoid of equations, adequately illustrated, and interestingly written, with technical details discussed carefully, the book should be intelligible to the nonspecialist. But the specialist may also want to read it, for it is probably the most complete source of information on Alfvén's interesting and original ideas about these cosmological problems.

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Immunology

Foundations of Immunochemistry. EUGENE D. DAY. Williams and Wilkins, Baltimore, 1966. 223 pp., illus. $8.50.

In this book the author has attempted to set forth the information he believes to be fundamental to any effort employing immunochemical methods. The book is apparently not intended to, nor does it, fill the growing need for a more comprehensive book that presents the developing field of immunology from a molecular point of view. The foundations of immunochemistry, according to the author, consist of a knowledge of immunoglobulin structure and of the reactions between antigens and antibodies; among the latter, only hapten binding and the precipitin reaction are deemed relevant.

The opening chapter of the book presents a thesis which a reader with some background in physical biochemistry might regard as rank heresy, that the notorious heterogeneity of immunoglobulins is the rule, rather than the exception, in protein chemistry. The remaining chapters in this part of the book deal briefly with various aspects of immunoglobulin structure in a manner that suffers at times from oversimplification, not all of which can be ascribed to the brevity of the discussion.

A more critical approach is evident in the portion of the book dealing with antigen-antibody reactions. However, in section 5 of the chapter dealing with the precipitin reaction, the author presents what appears to be a novel treatment by means of phase diagrams. The thesis is that in the equivalence zone the antigen-antibody complexes undergo a change of phase as the antigen/

antibody ratio of the system increases. In order to substantiate this point, it is necessary to examine data on the composition of antigen-antibody complexes (both soluble and insoluble) over a wide range of antigen/antibody ratios. The author achieves this range by combining in one figure data for the same antigen-antibody system obtained by different groups of investigators 20 years apart. One of these studies dealt with precipitation from antibody excess into slight antigen excess, and the other examined the composition of soluble complexes in marked antigen excess. The failure of these two nonoverlapping sets of data to fall on the same line in a phase diagram is taken as evidence supporting the author's thesis; this ignores the strong possibility that the disagreement is due to antibody heterogeneity—for, as indeed the author himself points out earlier in the book, no two antiserums can be expected to react in the same manner. Unfortunately, considerable use is made of this concept of a phase change in subsequent discussion.

The book is exceedingly well written—so well that the clarity and elegance of the author's style may blind the unwary reader to some of its defects.

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A Pioneer's Account of His Research

Otto Hahn: A Scientific Autobiography. Translated from the German edition (Braunschweig, 1962) and edited by WILLY LEY. Introduction by GLENN T. SEABORG. Scribner, New York, 1966. 320 pp., illus. $7.95.

In the autumn of 1904, Otto Hahn went to London for a short period of work in the laboratory of Sir William Ramsay. His primary purpose was not to do research but to learn English in preparation for a post with a chemical firm. Ramsay, not deterred by the fact that Hahn was an organic chemist, handed him a bowl of active barium chloride and requested that Hahn separate the radium from it. In so doing, Hahn discovered a new radioelement, which he named "radiothorium." Ramsay was sufficiently impressed to urge Hahn to abandon thoughts of an industrial career and succeeded in gaining for him a place in Emil Fischer's Chemical Institute at the University of Berlin. But before heading home, Hahn decided that he needed to learn far more about radioactivity and recognized that the best opportunity for this would be with Ernest Rutherford, at McGill University. Rutherford, in September 1905, received the young German skeptically, for he had good reason to look askance at work in radioactivity done in Ramsay's laboratory, and his radiochemist friend at Yale, B. B. Boltwood, had suggested that radiothorium was merely a "compound of thorium X and stupidity." Within a short time, however, Hahn was able to convince the doubters of the existence of this radioelement, and indeed went on to discover a great many more.

Hahn is the last survivor of the group that pioneered in the study of radioactivity in the decade after its discovery. When he began untangling decay-series genetics in 1904, Henri Becquerel, Pierre and Marie Curie, Rutherford, and Frederick Soddy all were engaged in examining radiations or the substances emitting them. When with Strassmann in 1938 he discovered nuclear fission, none of these outstanding figures remained active in research.

In his autobiography this distinguished and beloved chemist tells the story of his contributions to the study of radioactivity. It is truly a scientific biography in which Hahn reviews his work in some detail, and he appends three interesting papers on fission. The discussions of key points leading to his many discoveries—and a few failures—are illuminating and valuable. The picture of Hahn that emerges is one of a superb experimeter, but (by his own admission) a not very daring or imaginative theoretician. In his short preface (not included in the present translation) to the German edition Hahn wrote that he hopes some day to record his personal reminiscences in greater detail. This explains why information about his career, in contrast to his research results, is at a minimum.

In discussing the apparent blindness of scientists to the possibility of fission at a time many were searching for "transuranium elements," Hahn notes that Ida Noddack alone maintained that *all* known elements would have to be eliminated before one could accept the existence of an element beyond uranium. He then continues that "her suggestion was so out of line with the then-accepted ideas about the atomic nucleus that it was never seriously discussed." One would have hoped that Hahn might elaborate here, especially since Robert Jungk, in *Brighter Than a Thousand Suns,* quotes Noddack to the effect that Hahn, wishing to shield her from ridicule, deliberately avoided referring to her idea in the mid-1930's and thus was instrumental in keeping it from being discussed.

Hahn says nothing about applied research during the second World War and the demands made on him by the German government for research with military goals. All that is mentioned as occurring between 1939 and 1945 is the identification of numerous fission fragments. Hahn has chosen to ignore this disagreeable chapter in his life, as he has also avoided any value-judgments on the atomic bomb, although he is widely reported as viewing his own unwitting role in its development with great distress.

There are, in addition, some surprising interpretations concerning the origin of the concept of isotopy. Hahn writes that Moseley's understanding of atomic number (which in fact came later), Rutherford's nuclear atom model, and the group displacement laws of Fajans, Soddy, and Fleck were needed before the idea of isotopes could occur to Soddy. I think it is clear, however, that the concept of isotopy could have emerged, and indeed did, from radiochemical considerations alone. Furthermore, the full understanding of isotopes came simultaneously with the group displacement laws in 1913, the first step of placing several radioelements in the same place in the periodic table having been taken in 1909 by Strömholm and Svedberg. And finally, Fajans, somewhat earlier than Soddy, published not only the correct displacement laws but also the application of the concept of isotopy to all natural radioelements. His term "pleiad" simply was less successful than Soddy's "isotope."

However, these and a few other imperfections do not detract seriously from the great value of the autobiography. Unfortunately, not all of the editor's additions are improvements.