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.

Valuable features not present in the original edition include an enlarged section of photographs, a "synoptic calendar" of events, and an index. But the bibliography of Hahn's papers and the section of biographical sketches of other scientists are defective. The former omits many papers published prior to the 1930's and leaves the reader uninformed of its incompleteness, and the latter contains far too many errors of fact and of omission. Additionally, one senses a lack of care in assembling the book: some "Germanisms" appear in the translation; there are several misprints; and some of the numerous references which Hahn placed in footnotes have been raised into the text in an incomplete form, while others have been omitted.

Nevertheless, we are indeed fortunate to have such an account of Hahn's work from his own pen. Its publication is particularly timely, for Hahn shared the 1966 Fermi Award with his two eminent colleagues Lise Meitner and Fritz Strassmann.

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Signals and Receivers

Information Theory and Esthetic Perception. ABRAHAM MOLES. Translated from the French edition (1958) by Joel E. Cohen. University of Illinois Press, Urbana, 1966. 227 pp., illus. $7.50.

It is an old idea to suppose that each department of sense—vision, hearing, touch, smell, and taste—is analogous to a telegraph line over which electrical signals can be sent and messages thereby transmitted. It is obvious to compare the sensory nerves (or their fibers) with wires, and there is a vague similarity between the impulses in neurones and the electrical pulses of the Morse code. The sense organ is analogous to the transmitting apparatus and the brain to the receiving apparatus. Sense-perception, then, is supposed to be a matter of decoding the nerve signals. Since the message comes from the environment, the *sender* of the message is comparable to an object in the world. What is comparable to the *receiver* of the message? In human communication there is always an operator who has learned the code or (with a teletypewriter) a person who reads the telegram. In sensory communication it would seem that a little man in the brain, a homunculus, is required by the logic of the analogy, but this is an unwelcome implication. Some psychologists believe that this difficulty destroys the whole analogy.

This way of thinking about perception was given a new impetus, however, when Shannon published his mathematical theory of communication some 20 years ago. He could define a channel in general terms and show how coded information could be treated as a quantity. It seemed that the analogy between a human channel of communication and a sensory channel could be tested. There were other applications of the theory of information transmission, but this was one.

Information Theory and Esthetic Perception is primarily a book about sense perception, as indeed it has to be if the author is going to discuss music and art, which he does in the second half. It was written in the full flush of enthusiasm for Shannon's mathematics by a man who had studied electrical engineering, physics, psychology, philosophy, and music. The author is an academic at the University of Strasbourg but, like the other standard-bearers of information theory, he wanted to cut across the academic disciplines and found a new branch of knowledge. There is boldness and imagination in this book but also much oversimplification and looseness of thought. The author was impatient with the existing theories of language, music, painting, and esthetics generally. He sat down to write a book, trusting to insight, intuition, and his explorations into "concrete music." This consists of experiments with recordings, electronic "clipping" of waves, tape-splicing, running a tape backwards, and the like.

The translation appears to have been carefully done, and there is an excellent translator's preface. This English edition is useful mostly in showing what a certain intellectual movement was like nine years ago, not what it is like today. Although communications engineering has made orderly progress, general information theory has not. Many of its pioneers have turned to other methods, and there is no assurance that Moles himself would subscribe now to what he wrote then.

The chapters on esthetic perception are plagued by the same obscurity that characterizes other writings on esthetics. Information theory has not made the subject any easier to understand.

The chapters on visual and auditory perception represent one form of the theory of the sense organs as transmitters of elementary sensations and the brain as a receiver of these signals. Moles thinks of the brain as storing memories, creating symbols, and having a priori knowledge. These concepts of what the brain does are not new. The newest way to think of the brain is as a computer, and this conception is not found in Moles's book. Computer models of perception are now proliferating.

So long, however, as the brain is likened to any instrument that simply receives coded signals the theorist is faced with a paradox. Optic or acoustic signals must be seen or heard. If what the brain gets is signals from its eyes and ears it must have internal eyes to see them with and internal ears to hear them with. So the theorist is right back where he started.

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Unifying Theory for Solids

Pseudopotentials in the Theory of Metals. WALTER A. HARRISON. Benjamin, New York, 1966. 352 pp., illus. Paper, $7.95; cloth, $13.

The general reader may well be surprised by the title of this book. In nuclear physics constructed potentials (pseudopotentials) are often used to describe the results of scattering experiments, because of our inability to calculate nucleon-nucleon forces. In metals, however, all the forces of interest are of Coulombic origin, and there appears to be no need for an artificial potential. Nevertheless a large technical literature concerning pseudopotentials in solids has grown up.

Pseudopotentials have proved useful in solids because they represent, as indicated by the author in his preface, "a single point of view from which virtually all the properties of simple metals [and, one may add, semiconductors also] may be studied." The pseudopotential represents an abstraction of the actual atomic potential which describes scattering of valence electrons near the Fermi energy. The pseudopotential is weak, so that it can be treated by perturbation theory, and the complicated behavior of the real