We find it hard to subdue our intuitions and "get a feel" for space-time, but the authors of the essays in this chapter have taken great pains to present their topics via phrases and analogies, which the layman can at least reach after. In here using ordinary language (instead of the technical language of theoretical physics) to describe either the submicroscopic or the pancosmic, to transcend the five senses, they are attempting something very difficult and attempting it with their hands voluntarily tied. The lay reader should not limply surrender but should respond with his own reciprocal effort, reading, skipping, turning back a page or two, and rereading until at last the flavor of the subject permeates. Thus he will learn what it entails to vanquish the preconceptions of human intuition, in which conquests lie Copernican revolutions.

It is, of course, not possible to review such a book. It is far too diverse. One of the contributors, J. N. Hammersley, remarks in his contribution, "A principal cause of indigestion is too many breakfasts with the same companion." This is a danger the reader will certainly be spared. Perhaps the 25 different companions would prove rather a strain if taken at the rate of one every successive day, but they write with such charm and good manners that most people will find themselves lured into disquisitions about subjects which they had previously thought themselves quite incapable of understanding.

Of course it might be difficult to claim that all the doctrines are truly of Copernican magnitude. Was the discovery that our sun is not in the center of our galaxy—the topic of the first major article in the book—comparable in importance to the discovery that the earth rotates round the sun rather than vice versa? Indeed, are any of the other extraordinary discoveries of the astronomers, the gigantic explosions on a galactic scale, the mysterious quasars, black holes, and so on, any more than signs that once Copernicus had displaced man from the center of the universe we have no rational grounds for finding anything more extraordinary than anything else? Yet there is no doubt that we do continue to find these things both fascinating and very, very queer. Again, once we had got rid of the idea of vitalism—that "the least imaginable part [of an animal] which we can separate is as much alive as the whole," in a formulation quoted by Robert L. Sinsheimer—we can continue to marvel, but no longer be surprised, at what chemistry can do. We find ourselves confronted by what Sinsheimer describes as "an extended series of major discoveries both expanding and complicating the concept of life on the one hand, and both expanding and deepening the concepts of chemistry on the other." There is no doubt that one of the major influences in displacing the classical types of vitalism was the Darwinian notion of evolution, followed by Mendel's discovery of the particulate nature of heredity. R. C. Lewontin has no difficulty in making a very good case that these two together constitute a "materialist revolution" of truly Copernican magnitude; though I am glad to see that he admits that "until that interaction of gene and environment in determining [the phenotype of the] organism is fully integrated into scientific and social thought, the Darwinian and Mendelian revolutions will remain incomplete." I would myself have thought that a demonstration that the gene is a material particle, by T. H. Morgan and his colleagues, was a more Copernican sequel to Mendel than the one chosen here, the elucidation of the structure of DNA and protein, which seems to me more Keplerian in character.

One could, of course, go on almost indefinitely arguing, interestingly if inconclusively, about the claims of the various topics to be classed with the heliocentric theory. However, there would be little point in doing that here. Like Little Jack Horner, I will just pull out one further plum: a complete specification for a thorough piece of operations research by Florence Nightingale in a letter addressed to Francis Galton in 1891, quoted here by Herbert Robbins in his piece on "The statistical mode of thought." There is plenty of other unexpected treasure to suit the taste of almost any reader in this very rich book.

C. H. WADDINGTON
Institute of Animal Genetics, University of Edinburgh, Edinburgh, Scotland

Fifty Years in Physics and Chemistry

Solid State and Molecular Theory. A Scientific Biography. JOHN C. SLATER. Wiley-Interscience, New York, 1975. x, 358 pp., illus. $18.95.

As one of the first and few American-trained physicists to participate significantly in the development of quantum theory and a leading contributor to solid state physics and molecular chemistry, J. C. Slater enjoys an eminent position in 20th-century physics. Therefore, the appearance of his "scientific biography" is an event of note. The subtitle of the book, however, is ambiguous. The author points out that because of the strong interplay "between autobiography and history of science and technology on the one hand, and pure science on the other . . . it seemed worthwhile to make this book something half-way in between." The book does indeed have something of all these qualities, but the compromise is not entirely successful. There is, for example, textbook material that is only partially relevant for the reader who is already acquainted with the subject matter and is probably not understandable to the reader who is not. Portions relevant to the history of science and technology suffer from the absence of references to original sources. As an autobiography, the book, though subjective, is curiously aloof and impersonal. It is nevertheless deliciously spiced with strong, no-nonsense statements about physics, scientists, politics, and economics.

Slater first introduces himself to the reader as a Ph.D. candidate at Harvard completing an experimental thesis with Bridgman. (There is no mention of his earlier life except for a passing reference later in the book to boyhood and undergraduate studies at Rochester.) The next two years, 1923–24, were spent in Copenhagen with Bohr and at the Cavendish on a traveling fellowship and marked a turn to theoretical physics. The important Bohr-Kramers-Slater paper on the interaction of the radiation field with atoms was written during this period. In its introduction of virtual oscillators, an idea due to Slater, it hinted at the probabilistic notions that were to be developed a few years later in Born's statistical interpretation of the Schrödinger equation. The relationship with Bohr, apparently, was not entirely happy, because of a strong disagreement about the photon concept. Slater believes his views concerning the relationship between photons and electromagnetic waves to have been the same as those of de Broglie and to have been arrived at practically simultaneously. He writes somewhat bitterly that since de Broglie "did not have the antagonism of Bohr to contend with, . . . he followed his ideas to their obvious conclusion." The conclusion in question evidently is the recognition that the particle-wave duality extends to electrons and other elementary particles.

Slater returned to Harvard. During 1926–27 his work on quantum electrodynamics closely overlapped that of Dirac. It must have been a frustrating time, since Dirac was always slightly ahead. This experience evidently marked an intellectual turning point: "It was obvious that I would never catch up with Dirac to the point of being clearly ahead of him. Thus at this point I shifted my interest to the helium atom." Except for a war-induced interlude of intense effort on radar and microwaves, the remainder of Slater's research activity has been devoted to the theoretical study of the electronic energy levels of atoms, molecules, and solids.

Although Slater's immense respect for Dirac's intellect is clearly evident from this account, so also is the ideological difference concerning the approach to theoretical problems. Slater differentiates between two sorts of theoretical physicist, the "pragmatic, matter-of-fact sort" on the one hand, and the "magician, [who] waves his hands as if he were drawing rabbits out of a hat and . . . is not satisfied unless he can mystify his readers or hearers" on the other. It is interesting that Van Vleck used the same language in referring to Dirac's quantum theory of the electron, "one of the most brilliant intellectual achievements of all times," but in an entirely complimentary way: "All the properties of electron spin came out automatically, and almost magically, like rabbits out of a magician's hat" (*Pure Appl. Chem.* **24**, 235 [1970]).

Slater identifies himself (as well as Schrödinger and Heisenberg) strongly with the former group and Dirac with the latter. He expresses strong distaste for the use of what in his view are needlessly formal and general theoretical tools. The indiscriminate application of group theory (the "Gruppenpest" of 1929), second quantization, and diagram techniques all fall under this rubric. In his famous 1930 paper, which was the first to make use of what were subsequently called "Slater determinants," he showed that complex atomic spectra could indeed be adequately explained by simple theoretical means. He regards this as his most universally popular work, in part because it exorcised the "Gruppenpest." Its importance was in fact recognized very rapidly by Heisenberg, Hund, Wigner, Pauli (who claimed characteristically that "he couldn't understand a word of it"), and others, as Slater discovered during a half-year's stay in Europe on a Guggenheim fellowship.

In 1930 Karl Compton, the newly chosen president of the Massachusetts Institute of Technology, asked Slater to join that institution as head of its physics department. He accepted with enthusiasm and, together with Compton, greatly expanded the physics effort during the decade by attracting such first-rate people as F. Bitter, M. S. Livingston, P. M. Morse, J. A. Stratton, and B. E. Warren. Slater's view that the German institute concept was perhaps emulated in the United States more than was realized during this period of scientific immigration is of particular interest in the context of the organizational structure then being developed at MIT.

In the meantime the growth of solid state physics had been proceeding rapidly. In 1933 Sommerfeld and Bethe published a comprehensive review of the subject. According to Slater, that year marked a turning point in solid state theory because of the development of the Wigner-Seitz method for calculating electronic energy levels (or band structures) of solids. This was the first method to break away from the Bloch-Hückel "tight-binding" approach on the one hand and the Bethe-Peierls "nearly-free-electron approximation" on the other.

A number of other, more sophisticated methods have been suggested since, one of the most important being the augmented plane wave approach that Slater developed in 1937 during a stay at the Institute for Advanced Study. The full implementation of these methods had to await the development of large-scale digital computers after World War II. The Solid State and Molecular Theory Group, which Slater organized at MIT in 1951 largely for this purpose, furnished a wealth of information about the electronic energy levels of a large variety of molecules and solids. This proved to be very important in the interpretation of accurate experimental data on the newly available high-purity single crystals that were produced with the use of techniques developed in the late '40's as part of the technological effort following the invention of the transistor. The MIT group, however, seemed less interested in the detailed interpretation of experimental results than in the development of new theoretical techniques, the explanation of systematic trends in energy levels of related solids (for example, the transition metals), the improvement of calculational procedures, and the problem of constructing better crystal potentials. Much of the work appeared only in the form of quarterly progress reports, which, however, were widely read and quoted.

The amount of ingenuity, effort, and organization required to produce these results and advances must not be underestimated. Slater devotes half the book to a description of these theoretical developments. Much of the account is very technical. Suffice it to say here that the work has culminated in the $X\alpha$-SCF (self-consistent field) method and that the characterization and intellectual history of the method represent its raison d'être. As Slater quite properly suggests, this method may be of considerable importance in leading to a better understanding of small-particle catalysts, molecules of biological importance, and surfaces.

In 1964 Slater left MIT to join the physics department at the University of Florida in Gainesville. He no longer felt comfortable at MIT: "During the 1960s so many faculty appointments were made in nuclear and high energy theory that the department became unbalanced, in a way I would never have allowed while I was department head." Times change.

The image of the 50 years in which Slater often has been a leading contributor to physics and chemistry that is given in this book is somewhat distorted. It cannot be otherwise because of the intensely personal, even self-centered, though always straightforwardly expressed, viewpoint. For example, the connection between the $X\alpha$ method and the more fundamental Landau theory of Fermi liquids is never alluded to, despite its scientific importance. In fact, there is no reference to Landau or to many other important scientific figures of the period in the index. There is also no glimpse given of other very important ways of getting at the electronic structure of solids besides those with which Slater has been connected at some point in his career. The pseudopotential method is the most notable of these. Its simplicity and close connection with experimental results have made it one of the most widely used approaches to band theory.

This book is the first extended essay on the history of solid state and molecular physics. Despite some flaws, this account by one of its founders and most notable contributors will be an invaluable resource to the historian and fascinating to anyone who has had any contact with the field or with some of the institutions where it developed.

HENRY EHRENREICH
Division of Engineering and Applied Physics, Harvard University, Cambridge, Massachusetts

Tests of Gravitation

Experimental Gravitation. Proceedings of a school, Varenna, Italy, July 1972. B. BERTOTTI, Ed. Academic Press, New York, 1974. xx, 576 pp., illus. $42.50. Proceedings of the International School of Physics "Enrico Fermi," Course 56.

Gravitation is a realm of science in which each new experimental result is eagerly awaited, because the weakness of the gravitational interaction makes the battle for each new bit of information long and arduous, and because most physicists and