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high-speed computing machines which enable us at long last to do something with nonlinear problems have led to a renewed and practical interest in what seemed 60 years ago to be an elegant intellectual exercise.

In view of the theory's increasing range of applications, there is little doubt that it is going to grow considerably. It is interesting to observe in it a clear example of how rigorous, pure mathematics can still profit from sound intuition, including physical intuition.

The Concept of the Positron. A Philosophical Analysis. By Norwood Russell Hanson. 236 pp. Cambridge Univ. Press, New York, 1963. \$5.95. Reviewed by Norman Feather, University of Edinburgh.

IT is perhaps surprising that this is the first book by an established philosopher of science devoted specifically to the problem of antimatter. One might have supposed that the glamor of the topic would have proved irresistible long ago.

To be precise, "The denouement of the book is its ninth chapter. There the intricate story of the discovery of anti-matter is set out" (p. 1), or, to be more precise, "Chapter IX is called *The Positron* . . . the positron is the first anti-particle" (p. 3). So, Professor Hanson has attempted to provide, in the first eight chapters (pp. 5-134), the prolegomena appropriate to the denouement of the ninth (pp. 135-165)—and in 18 pages of appendices, and 42 pages of notes, the critical apparatus for the program as a whole.

The reader who wishes to derive full value from these sixty pages of appendices and notes must be no mean scholar; he must be able to translate the Latin of Descartes, Grimaldi, and Newton, the French of Joliot and Curie, and the German of Skobel'tzyn; he must have had some grounding in quantum field theory—and his knowledge of the literature must be such that he is able to supply many missing or incomplete references out of his head (authors crop up, every now and again, previously unmentioned, with a mere *loc. cit.* to direct the searcher to his quest). If he is a conscientious reader, he will have been making regular reference to the notes as he followed the main text of book. There, too, not to be overwhelmed by the author's parade of learning, he will have had to call on his own knowledge of philosophy and history—and of physics itself—to a considerable degree. Too often, Professor Hanson introduces a character, for a moment, only to dismiss him again with equal celerity—Beeckman, the seventeenth-century Dutch geometer, for instance, or Benton, of whom all that we are told (and I have discovered no more) is that he wrote *unintelligibilia*. Too often, also, bits and pieces of the formalism of theoretical physics are encountered, with no guide to the uninitiated by way of a definition of symbols.

We wonder what reader is equal to the whole task. This reviewer, certainly, is not. He can evaluate Professor Hanson's account of the doings of experimenters, and he admits to some acquaintance with the history

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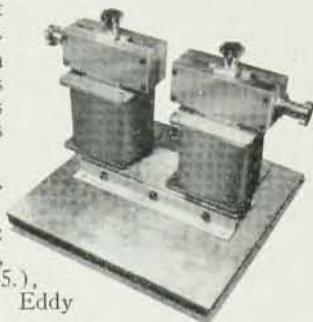
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of physics in its classical phase. But he cannot evaluate the argument which is advanced for the claim that "the 'proofs' of Eckart and Schrödinger, to the effect that the Wave Mechanics and Matrix Mechanics of 1926 are equivalent, are . . . faulty in their conclusions". He can note, possibly with amazement, special pleading in favor of Newton in relation to Grimaldi and diffraction (p. 21), and special condemnation of Bohr as an epistemologist ("the *ex-cathedra* utterances of the melancholy Dane", p. 96; "the strident statements of Bohr and Rosenfeld when they violate every lesson of the history of physics", p. 111), but, on the other side of the fence, he can do no more than accept uncritically Professor Hanson's discussions of noncommutativity.

Professor Hanson's thesis, in relation to experimenters, can be stated most succinctly in words which he quotes from a letter of Professor D. H. Wilkinson of 12th December 1955: "all sorts of people saw positrons well before Anderson." It is clear that the author believes this to be the case, and that the reason why these experimenters "saw, but did not observe" (p. 138), was that they were naturally conditioned (along with their theoretical brethren) to do anything rather than admit a third "fundamental" charged particle to the exclusive partnership of negative electron and positive proton which dominated their thinking at that time. Professor Hanson has obtained an admission (already available for historians generally in a letter in *Nature* in January 1934) from Professor Skobel'tzyn that "as early as in 1931 I observed some cases of positron-electron pairs but give them a wrong interpretation . . . as of an energy loss by radiation in a nuclear collision", but he is not satisfied with that. "It can confidently be reported," he writes (p. 138), "that the tell-tale tracks were 'encountered' by researchers like Orban, Rochester, Terroux, Feather, and certainly by Meitner, and the Joliot-Curies." Professor Skobel'tzyn will have none of this: "But no one else contrary to what you state did it at that time or earlier" (p. 183).

It is unfortunate that Professor Hanson sets so much store by his confident assertion: it involves the scrupulous reviewer in so many words to rebut it, for rebuttal there must be. Professor Hanson nowhere acknowledges that the recognition of pair-production in an expansion chamber requires either that the chamber be operated at high pressure (so that both positron and electron tracks end in the chamber), or that the chamber be operated in a magnetic field of suitable strength. It can confidently be reported (nay, more strongly!) that none of the experimenters in Professor Hanson's list had operated an expansion chamber at high pressure before 1930, and that certainly not all of them had operated with a magnetic field at that time (this reviewer had not, for one). So, some names, at least, must be struck off the list. Secondly, and at least as importantly, Professor Hanson altogether fails to admit—or even to record, except obliquely in Professor Skobel'tzyn's letter, which he quotes—that precise simula-

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Magnetism

A Treatise on Modern Theory and Materials

 Edited by **GEORGE T. RADO**
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Volume 1, December 1963, 688 pp., \$19.00

Volume 2, in preparation

Volume 3, 1963, 623 pp., \$18.00

This three-volume treatise provides an up-to-date and concise summary of our understanding of magnetically ordered materials. It deals with ferromagnetism, ferrimagnetism, and anti-ferromagnetism, i.e., with phenomena characterized by ordered arrangements of magnetic moments subject to strong mutual interactions. The broad scope of the work includes the most diverse aspects of these phenomena in insulators as well as in metals.

The Direct Observation of Dislocations

 By **S. AMELINCKX**

Supplement 6 of Solid State Physics

 Edited by **FREDERICK SEITZ**
and **DAVID TURNBULL**

Spring 1964, in preparation

In this book, for the first time, a complete review is presented covering all known methods for detecting individual dislocations. For each method the theoretical background is developed. Its use is illustrated by specific, representative examples, and the advantages and limitations are discussed.

Physical Acoustics

 Edited by **WARREN P. MASON**

Volume 1, Part A—Properties of Gases, Liquids and Solutions

 Winter 1963–64, about 525 pp.,
approx. \$17.50

Volume 1, Part B—Properties of Polymers and Nonlinear Acoustics

Winter 1963–64, about 375 pp.

Physical Acoustics describes high frequency sound waves in gases, liquids, and solids, in their uses as tools for analyzing the molecular, defect, domain wall, and other types of motions that can occur in these media.

Vapour Pressure of the Elements

 By **AN. N. NESMEYANOV**

 Translated and edited by **J. I. Carasso**

1963, 469 pp., \$14.50

A critical survey of published data on the vapour pressure of the chemical elements, the experimental results of many authors are presented in tabular form and as plots of the logarithm of the vapour pressure against reciprocal absolute temperature. To help the reader develop the valuable faculty of critically appraising published data, methods of measuring vapour pressures are discussed systematically in Chapter 1.

Ergodic Theory

Proceedings of an International Symposium held at Tulane University, New Orleans, 1961

 Edited by **FRED B. WRIGHT**

December 1963, 316 pp., \$8.00

This series of papers deals with current aspects of research in ergodic theory. A broad range of topics is discussed including random, abelian, and adjoint ergodic theorems, random series, and minimal sets. Extension and generalizations of classical results, entropy, applications to Markov chains and processes are also covered.

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tion of the tracks of a positron-electron pair is provided (when the ends of the tracks cannot be seen) by the inelastic, large-angle scattering of an "ordinary" electron, that this (quasi-ambiguous) phenomenon is to be seen whenever electron tracks of whatever origin are photographed (with a magnetic field), and that even when a pair-producing source is being employed, it is of much more frequent occurrence than genuine pair-production in the gas of the chamber (unless very high pressures are used).

Professor Wilkinson's characteristically light-hearted remark "all sorts of people saw positrons . . ." must be judged correct, in so far as cosmic-ray positrons (and others) must certainly have produced tracks in expansion chambers, ever since C. T. R. Wilson, in 1923, showed how these could be operated at full sensitivity. But, in Professor Hanson's context, this statement is trivial, and beside the point. So far as the argument of this book is concerned, the author has entirely failed to convince. Professor Skobel'tzyn should have the last word: "No one else contrary to what you state did it at that time."

That should be the last word of this review, but it must also be stated that the experimenter will raise his eyebrows many times, in other places, whilst reading this book. Let him refer to pp. 6, 31, 33, 46, 56, and 140 for examples of some remarkable confusions.

Professor Hanson produces some telling epigrams: "As historians know, the primary-secondary distinction dissolved in George Berkeley's inkwell" (p. 50), "diffuse hunches that God isn't a dice-player" (p. 196), "Logicians can talk science to death"—but more is required here than an epigram, however much truth it may enshrine.

Physical Adsorption of Gases. By D. M. Young and A. D. Crowell. 426 pp. Butterworths, Washington, D. C., 1962. \$13.00. *Reviewed by Stuart A. Rice, University of Chicago.*

ALTHOUGH all bodies are necessarily bounded by surfaces, our knowledge of the surface state is meager. Of the many possible methods of studying surfaces, the adsorption of gases has been the most popular for many years.

Physical Adsorption of Gases is an exhaustive survey of adsorption phenomena of gases on solids with extensive references to the literature and discussion of data. Except for the first chapter, the level of the book is uniform and suitable for use by graduate students. The first chapter is somewhat out of place in that it contains much elementary material which would presumably be known by those likely to use the rest of the book. The book contains very good discussions of experimental methods (but no mention of low-angle x-ray scattering techniques), a good discussion of the thermodynamic description of adsorption, and an adequate but sketchy treatment of the statistical theory of adsorption. The book would be improved if the treatment of the statistical theory were more detailed and if more