Book Reviews

Copenhagen Interpretation

The Concept of the Positron. A philosophical analysis. Norwood Russell Hanson. Cambridge University Press, New York, March 1963. ix + 236 pp. $5.95.

There is no doubt that this is an outstanding book, both as philosophy and as history of science. It is somewhat misleading, however, to bill it as a book about the discovery of the positron. The fact is that the last chapter, although delightful reading, is almost entirely independent of the rest of the book. If Hanson had dropped the last chapter and titled the book "Philosophy of Quantum Mechanics," the real nature and importance of the book would have been clearer. Instead, such passages as "this has been a third long step towards the positron" have been put into each section of the book (quite untruthfully in my opinion), and the book nowhere mentions that many chapters have been previously published as journal articles.

This is unimportant, however. The fact remains that chapter 1, "Light," is brilliant history (how many people are aware that Newton believed in the *dual* nature of light—*both* waves and corpuscles—and on what grounds?) and genuinely relevant to the rest of the book, and that chapters 1 through 7 constitute the best sustained defense of the Copenhagen Interpretation of quantum mechanics in print. Chapter 8, "Equivalence," and the last chapter (chapter 9), on the positron, are related to the others through their common concern with quantum mechanics—thus the book might, I repeat, have been revealingly titled "Philosophy (and Some History) of Quantum Mechanics."

According to the Copenhagen Interpretation, a particle whose position is unmeasured has *no* "sharp" (numerical) position *at all* (compare chapter 7, "Uncertainty again"). Experiments have shown self-interference effects, in the case of the photon, at 20-foot separations. If we accept the Copenhagen Interpretation, we have to conceive of macro-objects (like tables and chairs) as somehow retaining "sharp" positions and outlines, although they consist of particles whose positions can be "unsharp," even by macroscopic standards. The charge against the Copenhagen Interpretation is that it provides no satisfactory explanation of this fact.

The "classical limit" theorems do explain it in some cases, but not in the perfectly possible case of a macro-object whose position is partly determined by the outcome of a quantum mechanically uncertain event (as in the famous "Schrödinger's Cat" *Gedankenexperiment*). Some of the proposed "solutions" to the difficulty are: *to say the observer throws the object observed into a sharp state* (von Neumann); or to say the macroscopic detector does this (Bohr). Of course, this last *assumes* just what is to be *explained.*

Hanson has two lines of argument:

1) He suggests that macro-objects fall outside the province of quantum mechanics anyway, and there is thus no problem. This seems untenable. It is true that we cannot determine the state function of, say, a rocking chair (just as we could not *really* have determined the position and momentum of each particle of the rocking chair *exactly,* even if classical physics had been true); but we can describe our knowledge of its state by a statistical mixture. Thus classical objects fall into the range of quantum mechanics just as surely as they fall into the range of classical particle mechanics. Hanson does Mehlberg an injustice by suggesting that Mehlberg was unaware of the fact that classical mechanics is not *literally* deducible from quantum mechanics, even for macro-objects (if it were, there would be no problem!); but Mehlberg was challenging the quantum mechanist to explain macroscopic *objects,* not classical macroscopic *theory*. This confusion runs throughout Hanson's book.

2) Hanson suggests that the wave function of an electron is really just a predictor of the electron's probable effects on macroscopic detectors. But in what language are these effects to be described? In the terminology of classical mechanics? That is, are they to be described in the terminology of an inadequate theory? Or are they to be described in terms of wave functions (which would then be probabilities of probabilities of probabilities of . . .)?

I have already said that this book is the best defense of the Copenhagen Interpretation I know of. Unfortunately, even the best defense does not make that interpretation seem both intelligible and plausible.

HILARY PUTNAM
Department of Humanities, Massachusetts Institute of Technology

A Geometric Approach

Elements of Linear Spaces. A. R. Amir-Moez and A. L. Fass. Pergamon, New York, 1962. ix + 149 pp. Illus. $5.50.

That many textbooks of linear algebra have been published in recent years is not surprising, since there is hardly a science—physical, biological, or social—which does not now need the techniques of this subject. It is surprising, however, that few introductory texts have attempted a consistent development of the ideas of the subject through the use of two- and three-dimensional euclidean geometry. Of course, little bits and pieces of elementary geometry appear in examples, but in most texts algebraic ideas are not developed first in a geometric setting.

The authors claim to have attempted such a geometric approach to linear algebra in this book. In the first five chapters two- and three-dimensional real euclidean spaces are treated as linear spaces, and the general ideas of linear dependence, inner product, linear transformation, and the like are introduced within this concrete geometric framework. In the second edition the ideas treated in these first chapters are extended to *n*-dimensional vector spaces