Atomic Theory and the Description of Nature. NIELS BOHR. Pp. 119, Cambridge University Press, New York, 1961. Price $1.65.

Recently, a collection of popular lectures by Niels Bohr, delivered at various occasions in the period 1932–55, was published under the title *Atomic Physics and Human Knowledge* (John Wiley & Sons, Inc., New York, 1958). We mention this to avoid confusion with the book under review, which is a paperback reprint of an earlier series of essays dating from 1925–31. These earlier essays were first printed by the Cambridge University Press in 1934 under the same title, *Atomic Theory and the Description of Nature*, and the present reprint differs only by the addition of a short preface by Bohr, dated January 1961.

The first edition, which has been out of print for some time, served as a sort of Bible to an earlier generation of physicists interested in philosophical aspects of the "Copenhagen" interpretation of quantum theory. The modern student will recognize in this reprint several famous passages which he has probably seen before only in the form of quotations in the works of others.

As a statement on the back cover emphasizes, this book is not of mere historical interest. These issues still evoke controversy whenever physicists meet to discuss them. Indeed, debate has intensified recently, due in large part to the efforts of Professor David Bohm and his co-workers, to construct an alternative to the Copenhagen theory.

During the formative years of present quantum mechanics, which coincide with the dates of these essays, many different formulations and viewpoints on interpretation were advanced, notably by de Broglie, Schrödinger, Einstein, Heisenberg, Dirac, and Bohr. Due largely, we understand, to Bohr's persuasiveness in argumentation, the viewpoints of the latter three fused into a single one which is now identified by the name "Copenhagen." But de Broglie, Schrödinger, and Einstein never accepted it, and have continued to raise strong objections.

In reading these essays, it is therefore natural for us to ask how the principles of the Copenhagen school, and the arguments advanced against alternatives, have stood the test of time. The Copenhagen theory has grown and prospered to an amazing extent. It is probably safe to say that no living person is acquainted with even one percent of its quantitative successes; they are taken so much for granted that, unless an entirely new area of application is involved, one would no more report such a success than one would report another confirmation of Maxwell's equations. A recent example of such a new area is the successful prediction of Pais and Gell-Mann concerning the double decay mode of neutral K mesons, which has extended the proven range of validity of the superposition principle into an area undreamt of when these essays were written. No case of clear conflict between the theory and experiment has yet been found. In other words, *in its ability to predict experimental facts, no physical theory has ever met with greater success.*

How, then, is it possible that this theory could still be controversial? Here is the difficulty in a nutshell: the Copenhagen theory can predict the relative time of decay of two Co^{60} nuclei only with a probable error of about five years, but the experimentalist can measure this interval to a microsecond. Yet the Copenhagen school claims that its theory used a complete description of the physical states of the nuclei, and the particular number measured in the experiment corresponded to *nothing at all* in the initial conditions!

This is typical of all the troubles—whenever the Copenhagen theory makes a clear, unambiguous prediction, the prediction is always verified by experiment. But in every such experiment there is some other aspect of the data, which was measured in just as clear and definite a way, for which the theory was unable to give any definite predictions, only a set of probabilities.

For this reason, Einstein always maintained that the Copenhagen theory, while presumably correct as far as it goes, uses an incomplete description of physical states, just as statistical mechanics does (statistical mechanics being based on incomplete information about initial conditions), and that it is the job of theoretical physics to supply the missing parts. The Copenhagen school, using arguments which its opponents regard as a form of mysticism, denies that there are any missing parts, and claims that its opponents (Einstein and Schrödinger included) have not fully understood the situation. It is this issue, not the undeniable experimental success, which causes controversy now thirty-five years old and no nearer resolution than when it started. The essays under review represent the first comprehensive statement of the Copenhagen position.

Now let us consider the arguments against alternatives. Here we find more heat than light. Some physicists have carried the inevitable "bandwagon" psychology to such extremes that the Copenhagen interpretation has become a new theology, defended with a righteous fervor more appropriate to another age. Soon after the period of these essays there appeared much stronger claims, to the effect that it was impossible *in principle* to construct alternative theories of the type suggested by de Broglie and Schrödinger.

Recently, Bohm has dealt a fatal blow to such claims. He constructed, along just the lines originally suggested by de Broglie, a theory in which there is no uncertainty principle, no lack of causal determination, and in which the probability statements of the Copenhagen theory appear only as a consequence of incomplete specification of conditions on a finer level, just as Einstein had anticipated. This does not mean that Bohm's theory is right—it contains elements just as bizarre as any in the Copenhagen picture of things. What is important is that Bohm has done, by explicit construction, something which a whole generation of physicists has been taught is impossible. The field is now open for attempts to construct other theories along these lines.

In the period 1925–26, Schrödinger developed his "wave mechanics," in which the wave function ψ was interpreted in terms of charge density instead of probability density. He was well on the way to explaining the facts of spectroscopy by a very simple model which enabled one to trace all details of the emission and absorption of light without any discontinuous "quantum jumps," uncertainty principle, or other causal anomalies. But he visited Copenhagen in September 1926. Heisenberg reports, in the recent volume dedicated to Bohr on his seventieth birthday, and in his book *Physics and Philosophy* (Harper and Brothers, New York, 1958), that Bohr somehow convinced Schrödinger that it was impossible to obtain the Planck radiation law from this approach, and it was dropped.

Recently, this reviewer and his students took up the calculations where Schrödinger left off. The Planck law emerged in a perfectly natural way, along just the lines Schrödinger had anticipated.

In summary, the Copenhagen theory, whose principles were given their definitive expressions in these essays, is a success. There is no working alternative to it, no need for one in areas where it makes definite predictions, and little prospect that one will be developed in the near future. But there is a growing area in which the Copenhagen theory has not made any definite predictions. Nuclear and elementary-particle physics present us with volumes of experimental data which have not been explained from first principles. Every new particle discovered leads the theoretician to introduce still another quantized field. Any theory can survive for a long time on this kind of *ad hoc* patchwork, and perhaps all will be brought into order without departing from the Copenhagen principles. But the history of science teaches us that when a theoretical picture forces us to "add another epicycle" too many times, the solution is found by introducing entirely new physical ideas.

It is therefore of some importance to realize that the claims of completeness and uniqueness which have been made for the Copenhagen theory were too sweeping, and that alternative possibilities have been cut off before they were sufficiently developed. Attempts to develop alternatives which "supply the missing parts" are a legitimate, and potentially very important, part of theoretical physics.

Persons who dislike these attempts and do everything in their power to discourage them should recall the principle that Bohr used to defend the Copenhagen theory against its early attackers: *the test of any theory is not whether it contradicts preconceived philosophical notions, but only whether it contradicts experimental facts*. It was only on this basis that the Copenhagen interpretation could survive the Einstein-Podolsky-Rosen paradox. Today, the shoe is on the other foot; the Copenhagen interpretation has become the "preconceived philosophical notion," and persons who seek to modify it are entitled to demand that their efforts be judged according to the same rules of evidence that Bohr demanded for his.

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