The Correspondence Principle Revived

Sources of Quantum Mechanics. B. L. VAN DER WAERDEN, Ed. North-Holland, Amsterdam, 1967. 442 pp., illus. $14.

Here are two excellent books in one, and yet a frustration to the reviewer. As a source book, this is an excellent collection of papers, most of them previously untranslated. As a history, it presents a new and highly interesting view of the rise of quantum theory, documented with new evidence which the editor has obtained by querying the principals, and with a new selection of papers to be viewed as prominent. It is frustrating because the author-editor imposes on the reader an agenda that is as good as a verdict. For the theory here presented, namely that in the transition from the old to the new quantum theory nothing really served as a guideline but Bohr's correspondence principle, cannot be examined without reference to other topics. But this volume is devoted strictly to one topic. Other topics—wave mechanics, Hilbert space, Zeeman effect, spin and statistics, quantum field theory—have been excluded on various editorial grounds or relegated to a projected second volume. All the struggle, then, is finished in the two-page preface in which these omissions are noted. The 59-page introduction that follows it is a brilliant historical study which throws new light on the selections presented in the volume. It should, sooner or later, be critically examined by other historians.

The first paper, Einstein, 1917, on absorption and emission coefficients, is seemingly the odd man out. But Jordan explains in a private communication (printed here, as are all communications to Van der Waerden or among the dramatis personae, in the original German) that "Bohr had judged these results of Einstein rather skeptically for a long time; but then Van Vleck showed that it was possible to justify also these Einsteinian laws through a sharpened version of the correspondence principle." And so Van Vleck has enabled us to view Einstein in retrospect as a Bohrian.

There is, perhaps, one allusion to a guideline other than the correspondence principle. In his introduction to his 1925 paper, Heisenberg inserted a cryptic parenthetical phrase, "from the viewpoint of the wave theory"; Van der Waerden asked him whether he had meant "light waves or . . . De Broglie waves," and Heisenberg now explains that what he really meant was that an aspect of the wave theory of light has to be preserved in the new theory—the correspondence principle, again. Thus Born, Jordan, and Heisenberg render Van der Waerden's interpretation the authorized one. But how reliable is it? Surely it is new even by comparison with previous comments of the same authors. Let us examine the correspondence principle, then.

According to a most general methodological principle, any new theory should incorporate its predecessor, and as a special and limiting case, so as to explain the predecessor's explanatory and predictive success and also to achieve further empirical success. We can see this principle clearly at work in Kepler, Newton, Young, Fresnel, Maxwell, Einstein, and Bohr, and it was clearly stated by Poincaré and Duhem, if not by Whewell. This, then, is merely background material; incredibly, though, Born considers this (in his *Natural Philosophy of Cause and Chance*) as a predecessor to Bohr's correspondence principle.

In 1913 Bohr declared that, although his new theory makes an electron transmit one frequency while in transition from a second to a third, in the limit of high orbits (free electron) the three unite as in the old Maxwellian theory. This is hardly even an adumbration of the principle of 1918, according to which even on low orbits an approximation should exist—somehow.

The 1918 version of the principle is vague. In retrospect Heisenberg's work may be viewed as saying that, in some sense, every electron, as a virtual oscillator, possesses the frequencies it emits. In Van der Waerden's view, then, the development of the new quantum theory should be described more explicitly as the development of "systematic guessing, based upon the Principle of Correspondence," while the principle itself was developed, by systematic guessing, into clearer and clearer versions; indeed, the rise of matrix mechanics is both the guessing of a new theory based on the principle, and the guessing of the principle in its final, clearest scientific version: matrix mechanics is the principle and the theory unified, the end of the road.

All this is very exciting; yet the new insight is dependent on hindsight, which is inadmissible evidence. In history, as in criminal investigations, inadmissible evidence may be used as a means of discovering admissible evidence. But at the very least we should ask, What problem was Bohr's correspondence principle formulated to solve? The answer is, in Bohr's words, that "many difficulties of fundamental nature remained unsolved, not only as regards . . . the frequencies . . ., but especially as regards . . . the polarization and intensity of the emitted spectral lines."

Putting problems first may help in various ways. For one, Einstein's 1917 paper concerning relative intensities of spectral lines becomes the genuine starting point, with no need for the subsequent work of Van Vleck to legitimize it. Additionally, this approach leaves the way open for us to examine the diverse attempts, such as the abortive effort of Bohr, Kramers, and Slater, to solve the given problem. Van der Waerden includes their paper because it contains one element that was pursued by Born *et al.*—the idea of virtual oscillators. It would have been better to introduce the Bohr-Kramers-Slater theory as a possible solution to the problem of the relative intensity of spectral lines.

Clearly, then, the correspondence principle was a regulative idea for solving the problems which Bohr declared prominent; as Bohr, Kramers, and Slater record, it "afforded a basis for an estimation of probabilities of transition," and it did so by the introduction of the idea of virtual oscillators. Yet the central problem was wider: What mechanics can replace classical mechanics and yield existing atomic spectra? And Bohr's suggestion was even more specific—indeed, startlingly accurate even as a general idea. Obviously what was required was a mechanics which would yield classical mechanics as an approximation. Bohr went further, saying we need a Hamiltonian with electrodynamic potentials, the solution to which for the coordinates will supply transition probabilities as coefficients in their Fourier expansion. Surely this substantiates Van der Waerden's view of the work of Born, Heisenberg, and Jordan as the end of the line, the execution of a program. But it does so equally for Schroedinger.

One must, then, view wave mechanics in a similar vein, as an alternative to matrix mechanics. But here Van der Waerden is reticent. There is only one reference, which I have already quoted, to De Broglie, and only three to Schroedinger, one peripheral, one a statement that Born's perturbation theory is equivalent to his (what hindsight!), and one to him as a precursor of von Neumann! I hope that Van der Waerden uses "quantum mechanics" as a synonym for "matrix mechanics" merely to stress that historically the term is Born's, not in order to prepare the ground for a new attempt to bypass Schroedinger. But we must wait for the second volume to see how wave mechanics is treated. Let us hope that it will include other neglected works, notably of Landé and of Milne, not to mention Heisenberg's 1927 paper on indeterminacy.

In this volume, Van der Waerden draws attention only to that part of Bohr's program which relates to the rise of matrix mechanics. And therefore, even though he tries hard to credit Bohr, he does not do justice to all of Bohr's remarkably detailed and suggestive ideas. He can still do so in his second volume.

Viewed as a program, Bohr's correspondence principle suffers from too much success. Viewed as a proposal for a solution to a problem, it must be seen as embedded in a wider problem, how to overcome the wave-particle duality. The duality problem was not solved but rather deepened by following the correspondence principle. Otherwise there would be no need for the uncertainty principle (nor would it be possible). Did that principle solve the problem satisfactorily? The answer to this question was given by Bohr in his discussions of complementarity: it was as satisfactory as possible. The complementarity principle thus came to replace the correspondence principle. Hence the lack of clarity about correspondence, and the injustice done to it, in subsequent literature, including Pasqual Jordan's *Physics of the Twentieth Century*.

Van der Waerden has rendered a valuable service by putting the correspondence principle back on the map, even though he appears thus far as an apologist of one party in the dispute—the majority—rather than as a dispassionate observer; however, he can still correct this impression. It would be easier for him to do so, I suggest, if he stressed more the role that problems play in the advancement of science.

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Continental Drift Revisited

The Origin of Continents and Oceans. ALFRED WEGENER. Translated from the fourth revised edition (Braunschweig, 1929) by John Biram. Dover, New York, 1967. 256 pp., illus. Paper, $2.

Debate about the Earth. Approach to Geophysics through Analysis of Continental Drift. H. TAKEUCHI, S. UYEDA, and H. KANAMORI. Translated from the Japanese by Keiko Kanamori. Freeman, Cooper, San Francisco, 1967. 253 pp., illus. $4.50.

The debate about continental drift has raged fiercely ever since Alfred Wegener advanced his concepts in 1912. Struck by the congruity of the South American east coast and the African west coast, Wegener had gathered relevant geological data which convinced him that indeed South America and Africa had once been together as a single continent. He further concluded that all the present land masses had once been united. Historically, Wegener's views received their widest distribution through translation into many languages of his third revised edition (1922) of *The Origin of Continents and Oceans* (English translation by J. G. A. Skerl, London, 1924). That edition has formed the basis for most English-language discussions. The fourth revised edition (1929), however, from which the present translation was made, presents Wegener's matured views and represents a revision over the third edition to the extent that 40 percent of the references and illustrations are new. This last edition is flawed by Wegener's justified but unfortunate acceptance of erroneous geodetic measurements which seemed proof of relative movements of Greenland and Europe. The revision is surprisingly modern and cogent in some sections but is archaic in others. The volume (despite the tedious prose) should be read by every serious student of continental drift.

H. Takeuchi, S. Uyeda, and H. Kanamori use the debate about continental drift to present a generalized discussion of the earth's structure and history. The geophysical principles are presented nonmathematically, but only magnetism and heat are treated in detail. The authors take the reader from A. Wegener's presentation (third edition) through the controversy and the critical American Association of Petroleum Geologists symposium on continental drift of 1928, critical because the embittered rejection of the theory of continental drift by the main body of American geologists of that time can in part be tied to that symposium. The authors carry the discussion through the development of paleomagnetism into the modern oceanographic work and set the stage for the latest advance of the continental-drift theory, the hypothesis of sea-floor spreading, which has come on the scene since the book was written. A well-balanced account of the debate, which has itself been anything but well balanced, is presented. The authors provide many pieces of personal information which could not be gotten from the scientific literature and which help to bring the debate into its true historical perspective. A very strong emphasis is justifiably placed on the post-World-War-II development of paleomagnetism in the modern rebirth of the concept of continental drift. In this, however, the authors reflect the Japanese and European history rather than the U.S. history of rebirth. The rebirth in the U.S. came about through geological and geophysical studies in western North America and, especially, studies of the deep-sea floor. The large-scale yet simple geologic structures and geophysical properties of western North America and of ocean basins could only be explained in terms of a mobile earth, rather than the static one so long favored by most American geologists. The authors' treatment can be easily understood by the intelligent layman. It brings the reader up to the current scientific frontier while providing him with a good background. The book can profitably be read by any scientist concerned with the problem, if he is not put off by the simple treatment.

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Physics without Words

Basic Data of Plasma Physics, 1966. SANBORN C. BROWN. M.I.T. Press, Cambridge, Mass., ed. 2, 1967. 330 pp., illus. $8.50.

This is a book essentially written by computer. It is a compilation of curves useful to those making calculations involving elastic and inelastic collision cross sections, charge exchange cross sections, mobility, diffusion, recombination, and secondary emission coefficients, as well as other atomic processes that occur in gaseous discharges. These data were searched out of the