Book Reviews

Radiation Theory Between 1896 and 1925

The Tiger and the Shark. Empirical Roots of Wave-Particle Dualism. BRUCE R. WHEATON. Cambridge University Press, New York, 1983. xxiv, 355 pp., illus. $39.50.

Bruce Wheaton describes his book as "the story of a radical change in man's concept of light." In 1896, when his story begins, physicists were convinced that light consists of electromagnetic waves, a conviction based on evidence accumulated over the course of the 19th century. During the next few decades that belief about the nature of light was put in doubt, along with so many other beliefs that had seemed indisputable in the 1890's, and physicists had to learn to live with a growing uncertainty about the adequacy of the wave theory of light. Electromagnetic radiation behaved in puzzling ways in a variety of experimental situations, showing "properties no wave has any business to have," as H. G. J. Moseley wrote in a passage quoted in this book. By the mid-1920's, when Wheaton ends his story, Arthur Compton's great discovery had "sounded the death knell" of the wave theory, in Arnold Sommerfeld's words, and physicists were prepared to accept far-reaching changes in the very basis of their science. These changes were expressed in the new quantum mechanics, which incorporated the wave-particle duality for radiation and for matter as well.

The first theoretical arguments that cast doubt on the wave theory and suggested particle-like behavior for radiation were put forward by Albert Einstein in 1905. A few years later he was pointing to the need for a new fundamental theory of radiation that would include both its wave and its particle aspects. Einstein's arguments fell on deaf ears for a long time. One of Wheaton's principal points is that other physicists, many of them unaffected by Einstein's arguments or even rejecting his proposal of light quanta, came to appreciate the need for a new approach to radiation on the basis of the paradoxical results of their own experiments, results that no wave theory could encompass. Two whose contributions Wheaton emphasizes were William Henry Bragg, father of Lawrence Bragg, and Maurice de Broglie, elder brother of Louis de Broglie.

Wheaton's story is no simple linear development; what history of human events ever is? It involves the complex interweaving of a number of strands, starting with the experiments initiated by Wilhelm Roentgen's discovery of his mysterious penetrating rays. Once these x-rays and the gamma rays, discovered in 1900 by Paul Villard in his experiments on radium, were identified as electromagnetic radiation, the evidence provided by their behavior had to be considered along with the results obtained with ordinary light. It was not easy to disentangle the sometimes contradictory conclusions drawn from absorption experiments, ionization studies, scattering studies, and investigations of secondary electrons (including the photoelectric effect). Wheaton has gone through an extensive literature, as indicated by his 35-page bibliography. In his discussion the reader will encounter such rare birds, now largely extinct, as the impulse theory of x-rays, the triggering hypothesis for the photoelectric effect, and W. H. Bragg's neutral pair interpretation of x-rays. Wheaton stresses two paradoxes, recognized very early by J. J. Thomson and Bragg, respectively, and forcefully restated in 1922 by Maurice de Broglie on the basis of much more solid and extensive experimental evidence: Why should the spherical wave or pulse that presumably constitutes an x-ray ionize only a very small fraction of the atoms over which it passes? Why should the energy of the electron set free in this ionization process be so much greater than the radiant energy in the small portion of the x-ray wave that it intercepts, be comparable in fact to the energy of the electron whose deceleration produced the x-ray in the first place?

Most of Wheaton's book is devoted to the period up to 1922. He emphasizes the experimental development, in welcome contrast to much writing about the history of science, but he also comments appropriately on the contributions made by theorists. His readers may be surprised to learn that the role of the early quantum theory in interpreting these experiments was less significant and less direct than textbook accounts would suggest. Wheaton argues effectively that the very success of the Bohr-Sommerfeld quantum theory of atomic structure and spectra helped to divert the attention of theorists away from the problems of radiation for a number of years. He also presents an interesting argument for the close connection between the experimental tradition described in his book and Louis de Broglie's proposal of matter waves. Louis de Broglie's discussions and collaboration with his older brother Maurice were an influence comparable in importance to his study of Einstein's papers.

One might have expected this account of the "empirical roots of wave-particle dualism" to give a significant place to Compton's work, but Wheaton has surprisingly little to say about it, devoting a mere three pages to the "so-called Compton effect." He assigns much less importance to Compton's results and their theoretical interpretation than has any previous writer on this subject. Wheaton describes Compton's explanation of his results by the use of the light quantum as part of a "reawakening of interest" in the quantum in the early 1920's. I must disagree with Wheaton's evaluation here. The reawakened interest in the light quantum was largely negative, and those who discussed it in 1922 were still rejecting Einstein's ideas. Compton's work did make a crucial difference. Wheaton also seems to ignore the detailed analysis of the development of Compton's thinking given by Roger Stuewer in his book *The Compton Effect* (1975), a book Wheaton refers to only in a rather cavalier way.

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Wallace and Darwinism

Just Before the Origin. Alfred Russel Wallace's Theory of Evolution. JOHN LANGDON BROOKS. Columbia University Press, New York, 1984. xvi, 284 pp., illus. $30.

John Langdon Brooks believes that Alfred Russel Wallace has not received sufficient attention from historians of evolution theory. This book surveys Wallace's career to 1858, when his paper on natural selection prompted the joint Darwin-Wallace publications on the theory and forced Darwin to begin writing the *Origin of Species*. Wallace spent the years 1848–1852 in South America collecting zoological specimens. Brooks argues that from the beginning his real purpose was to investigate the appearance of new species through a study of the geographical distribution of related forms. Wallace's first paper on this topic was written soon after he set out for Southeast Asia and appeared in 1855 under the title "On the law which has regulated the introduction of new species." Brooks describes at length the development of Wallace's ideas through to the writing of the 1855 and 1858 papers. In his conclusion, Brooks goes on to claim that Wallace's views on branching evolution played a key role in stimulating Darwin to develop his own principle of divergence. This is not a new idea. It was advanced by Arnold Brackman in 1980 (*A Delicate Arrangement: The Strange Case of Charles Darwin and Alfred Russel Wallace*, Times Books). David Kohn refuted Brackman's claim at some length in these columns (*Science* **213**, 1105–1108 [1981]), but Brooks believes that his own more sophisticated interpretation of Wallace's early views will allow it to be revived.

Brooks argues that the image of a "branching tree" of natural relationships in Wallace's 1855 paper forced Darwin to begin thinking about divergence. This ignores all the other influences that were driving Darwin in the same direction in the 1850's (see Dov Ospovat, *The Development of Darwin's Theory*, Cambridge University Press, 1981). According to Brooks, though, Darwin did not complete his theory of divergence through ecological specialization until Wallace's 1858 paper led him to reread the earlier one. A major addition to Darwin's "Natural Selection" manuscript, known to have been written in May or early June 1858, is seen as a new insight on divergence inspired by Wallace. To give Darwin time for this burst of activity, Wallace's 1858 paper must have arrived earlier than the normally accepted date of 12 June. Brackman suggested that the paper arrived on 3 June, but on the basis of a study of British and Dutch postal records Brooks argues that it could have been in Darwin's hands by 28 or 29 May. He finds, however, that an earlier letter from Wallace to another contact in Britain did not arrive until 3 June. The postal evidence is thus unreliable, and the case for Wallace's influence on Darwin must rest on a comparison of what the two men wrote.

Brooks acknowledges (p. 243) that Wallace did not have a theory of how divergence occurs, yet he insists that Darwin was not able to complete his own solution to this problem until he reread Wallace's 1855 suggestion that the gaps in the "tree" of relationships are caused by the extinction of parent forms. The plausibility of this claim is undermined by the fact that both the tree analogy and the idea that parent forms are exterminated by their more specialized descendants are contained in the 1857 letter to Asa Gray used by Darwin in the presentation to the Linnean Society. On this basis, most Darwin scholars see the 1858 material on divergence as a natural extension of Darwin's earlier ideas. I do not think they will be convinced by Brooks's assertion that this was a new initiative inspired by Wallace. Indeed, given the lack of attention paid to divergence in Wallace's 1858 paper, it is difficult to see why it should have prompted Darwin to check the very brief reference to the same topic in the 1855 paper.

The question of divergence may distract attention from Brooks's valid insistence that Darwin and Wallace had very different concepts of natural selection in 1858. He argues that Wallace did not believe that varieties within a species might occupy different ecological niches. They all get their living in the same way, although some will be more efficient than others. The struggle for existence ensures that less efficient varieties have a limited population size, but Wallace did not claim that they are driven to extinction by the superior variety. Only at a time of unusual environmental stress will the less efficient varieties become extinct, leaving the fittest one as the sole representative of the species. This is a plausible reading of the 1858 paper, which would imply that Wallace's original form of natural selection was much less ruthless than Darwin's. Curiously, Brooks asserts (p. 222) that Wallace explained the formation of varieties through the natural selection of individual differences. But if the less efficient varieties could survive except in a time of unusual stress, how could Wallace have supposed the struggle for existence to be powerful enough to act on mere individual differences? In fact, as Brooks's own summary of the 1858 paper reveals (pp. 189–190), Wallace simply assumes that a species will split into varieties and scarcely mentions the action of selection on individual differences. His real interest was the interaction between varieties, not between individuals (see P. J. Bowler, "Alfred Russel Wallace's concepts of variation," *J. Hist. Med.* **31**, 17–29 [1976]). The two men were certainly arguing along different lines: Wallace did not deal with selection of individual differences, postulated only an episodic selection of varieties, and had no concept of divergence through ecological specialization. One can only conclude that it was quite reasonable for Darwin's friends to give Wallace's paper a subordinate position in the joint presentation to the Linnean Society.

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Avian Population Biology

The Arctic Skua. A Study of the Ecology and Evolution of a Seabird. PETER O'DONALD. Illustrated by Robert Gillmor. Cambridge University Press, New York, 1983. xvi, 324 pp. $49.50.

In recent years it has been increasingly realized that for the study of wild populations of organisms in field conditions the skills and knowledge of the geneticist are just as important as those of the ecologist or ethologist. Peter O'Donald's latest book, *The Arctic Skua*, is an excellent example of the value of this approach. The author applies his expertise as a population geneticist to the long-term study of a wild population of birds. This monograph of the Arctic skua (parasitic jaeger in North America) is thus unlike most avian monographs in subject matter. In addition to distribution, feeding, and breeding ecology, O'Donald covers the topics of genetics, sexual selection, demography and selection, genetic models of sexual selection, and mating preference.

A unique feature of the Arctic skua is its plumage polymorphism. Like Kettlewell's famous peppered moth, the skuas may be melanic or non-melanic. The melanism has a genetic basis and appears to be a stable polymorphism with a clinal distribution. Although O'Donald's genetic analyses include measures of heritability of some continuously variable traits, his major concern is to understand the plumage polymorphism. The questions he poses are: What is the genetics of the polymorphism? Is the polymorphism stable? How are the gene frequencies spatially and temporally distributed? What selective forces are acting on the morphs, and are they sufficient to "protect" the polymorphism against extinction of alleles? The answers to these questions must be considered as the unique contribution of the book, and the quality of the book rests largely on the author's success in han-