What Did Bohr Do?

Niels Bohr's Times. In Physics, Philosophy, and Polity. ABRAHAM PAIS. Clarendon (Oxford University Press), New York, 1991. xviii, 565 pp., illus., + plates. $35.

This is the third in a remarkable series of books on the history of modern science by Abraham Pais, the earlier two being *Subtle Is the Lord*, a biography of Einstein, and *Inward Bound*, a history of particle physics. Pais is himself a physicist of great distinction. He writes with authority and, in addition, with unfailing grace and considerable charm.

Pais knew Bohr and his family well and interacted with him scientifically over a span of 16 years, often daily. Pais states at the outset, "I loved Bohr. I have tried to exercise restraint in regard to these sentiments, which may or may not shine through." I believe they do. Some admirers of Pais's previous books may find themselves disappointed that the relevant science is generally treated in much less depth and detail in this one. In compensation, however, we receive a close-up portrait of a truly extraordinary, and extraordinarily appealing, personality. Niels Bohr would have been remarkable and fascinating even if he were not the author of truly monumental scientific achievements. But in Bohr's case the man and the science are inseparable.

Near the outset of the book Pais quotes three Nobel Prize–winning theoretical physicists of succeeding generations, regarding Bohr:

Born (1923):

> His influence on theoretical and experimental research of our time is greater than that of any other physicist.

Heisenberg (1963):

> Bohr's influence on the physics and the physicists of our century was stronger than that of anyone else, even Einstein.

Anonymous modern physicist:

> What did Bohr really do?

I may add that, in my own experience also, questions of this last sort are not rare even in discussions among quite sophisticated modern physicists having an active interest in the history of ideas.

An explanation, though certainly not a justification, for the change in perspective may follow from the peculiar nature of Bohr's major contributions.

First in any such list, and also first chronologically, must be his fruitful introduction, in 1913, of Planck's quantum of action into the dynamical description Rutherford's new (1911) model of the atom. The boldness and depth of his ideas are perhaps belied by their mathematical simplicity and ultimately provisional character. Rutherford's model of the electron orbiting a tiny charged nucleus and held in place by electric attraction was immediately suggested by experimental results from his lab (Geiger, Marsden). And yet the model contradicts basic principles of classical electrodynamics. For the orbiting electrons, being charged particles in accelerated motion, should according to classical electrodynamics continuously radiate electromagnetic waves, losing energy and spiraling into the nucleus. Bohr simply postulated what was necessary to accommodate Rutherford's model, that this classical picture was wrong and that the electron could peaceably orbit in what he called stationary states. Transitions between these stationary states were supposed to occur only discontinuously, with the release of all the energy into light whose frequency obeyed Planck's law (frequency equals energy divided by Planck's constant). Finally, with an eye toward a successful old piece of numerology describing the spectrum of hydrogen (Balmer's formula), Bohr postulated that the energy of the electron's orbit must, in stationary states, be related to the frequency of its classical motion in almost the same way as for the photon—but with an additional numerical factor, 1/2 times a whole number. (The 1/2 is the license of genius.)

From these postulates the Balmer formula could be "derived," with the most important bonus that the numerical factor that appears in it could be related to fundamental quantities—the charge and mass of the electron and Planck's constant. Soon the fruitfulness of this line of thought proved itself in many new applications. Among the most remarkable of these was to the spectrum of ionized helium—where Bohr made the crucial observation that by going beyond his initial approximation of an infinitely heavy nucleus, subtle discrepancies between accurate hydrogen and ionized helium spectra could be understood (reduced mass correction).

A "theory" based, as Bohr's atomic theory was, on an uneasy mix of nearly contradictory concepts and cribbing from experimental data, was clearly meant to be used as a scaffold and to be discarded when a more finished structure could support itself. In fact it has been entirely superseded, by modern quantum theory. In a certain sense, then, Bohr's theory is no longer of direct scientific interest. But to neglect it for this reason is to miss out on a circle of ideas having their own intrinsic beauty, a full appreciation of which requires historical understanding. Here is what a man with very different scientific taste and instincts, Einstein, had to say about it:

> That this insecure and contradictory foundation was sufficient to enable a man of Bohr's unique instinct and tact to discover the major laws of the spectral lines and of the electron shells of the atoms together with their significance for chemistry appeared to me like a miracle—and appears to me as a miracle even today. This is the highest form of musicality in the sphere of thought.

The deep structure of Bohr's work on the hydrogen atom exhibits the special style of his thought, which remains visible throughout his major work. It is especially marked by three features: closeness to experimental reality, willingness to entertain ideas that are clearly provisional and logically incomplete, and lurking in the background strong suspicion that virtually *all* knowledge is provisional and incomplete. Here is Einstein again, giving what Pais calls the best characterization ever given of Bohr:

> He utters his opinions like one perpetually groping, and never like one who believes he is in possession of definite truth.

During the years 1913 to 1924 Bohr was the undisputed leader in the development of the so-called old quantum theory. Roughly speaking, the old quantum theory was the extension of Bohrian modes of thought, and in particular the sort of ideas implicit in his atomic model, into ever wider domains. A leading role in these developments was the correspondence principle, according to which the quantum laws must go over into the laws of classical physics in the limit of large quantum numbers. In skillful hands this typically Bohrian, apparently vague principle could be a tool of extraordinary power. For instance, it was used to understand and predict selection rules and the polarization laws for atomic radiation, and ultimately led to the exclusion principle and the prediction of electron spin to rationalize otherwise apparently anomalous aspects of atomic spectra.

Another astonishing Bohr contribution of these times was his development of a fundamental understanding of the periodic table of elements, around 1920—*prior* to the discovery of the exclusion principle and of spin. We now know, of course, that these latter concepts are indispensable in logically accounting for the periodic table. Nevertheless Bohr, with his deep knowledge of the phenomena and feeling for the possibilities of existing theoretical ideas, managed, from an incredibly complex and murky situation, to isolate concepts of lasting value (closed shells, building-up principle) and even to predict the existence and properties of a new element, hafnium, which was duly found by his Danish colleagues.

Starting in 1925 and 1926 with the discoveries of Heisenberg and Schrödinger, the new quantum mechanics replaced the old. The flavor of the subject changed; intuition and metaphorical reasoning from close acquaintance with the data were for the most part replaced by mathematico-deductive reasoning. Especially in atomic and molecular physics and in the foundations of condensed matter physics, where the semi-quantitative methods of the old quantum mechanics could be readily accommodated to the new, progress was extraordinarily rapid. Bohr's position evolved from that of intellectual leader into that of mentor for the new generation. For several years, his Copenhagen institute was the prime meeting-place and clearinghouse for the new developments.

While he did not play a leading role in the technical development of the new quantum theory, Bohr was very much concerned with the logical and philosophical underpinnings of the subject. Perhaps today he is (alas) probably most familiar to the general public, and even to many practicing physicists, for his ideas concerning the interpretation and philosophical implications of quantum mechanics. I cannot begin to do justice to this very subtle and controversial subject here. The spirit of the so-called Copenhagen interpretation, largely due to Bohr and his disciples and still regarded as the standard interpretation, is that the meaning of the formalism of quantum theory must always be referred to a completely specified experimental situation, which in turn must be describable in classical terms. The Copenhagen interpretation is an interpretation of renunciation, as the following formulations of Bohr make clear:

> The unambiguous interpretation of any measurement must be essentially framed in terms of the classical physical theories, and we may say that in this sense the language of Newton and Maxwell will remain the language of physicists for all time.

> There is no quantum world. There is only an abstract physical description. It is wrong to think that the task of physics is to find out how nature is. Physics concerns what we can say about nature.

Many physicists, myself included, are not satisfied with these formulations. Though most agree that quantum theory *can* be applied within the rigidly circumscribed domain Bohr assigned it, not all are content to leave it at that, or to divide the world into intrinsically different "classical" versus "quantum" pieces. Also the world is more than a laboratory, and one really must strive to describe its behavior even when there is clearly no well-defined experimental situation in Bohr's sense—a problem that becomes particularly severe in cosmological applications of quantum theory. In the famous Einstein-Bohr debates, Bohr defended quantum mechanics against Einstein's yearning for a more classical theory; but some of us are coming to feel in defending his valuable hard-won ground he compromised too much. Quantum mechanics should be pushed as hard as possible, to the point where it can describe within itself a recognizable caricature of the world as it is experienced, and thus begin to provide its own self-consistent interpretation—or else there should be some definite change in its formulation.

This has not yet been accomplished, however, and Bohr's renunciations were not made lightly. They were made in reaction to the ease with which one can get into mistakes and contradictions by carrying over intuitions about the behavior of physical objects from daily experience into the domain of quantum mechanics. One particularly notorious example is the tendency to conceive entities like electrons or photons as particles or waves respectively (or *vice versa*), for either entity can behave "like" a particle or a wave under different conditions—in fact, of course, strictly speaking they behave like neither. Another example is the tendency to visualize a particle as having both a position and a velocity, whereas Heisenberg's uncertainty principle tells us that in quantum mechanics it cannot have precisely defined values of both simultaneously. Bohr's main response to these and other related dualities is the concept of complementarity. According to this somewhat elusive but alluring notion, there may be alternative concepts or aspects of reality, each useful in itself, that cannot be validly considered simultaneously. Thus any attempt to measure, or validly consider, a particle with a definite position precludes measuring or considering the particle as having a definite velocity—and *vice versa*. Position and velocity are valid concepts separately, but not simultaneously—they are complementary.

In his later years Bohr made some tentative, provocative, and perhaps playful attempts to apply the notion of complementarity well beyond physics. For example, he mooted the idea that to determine the state of a working brain sufficiently to predict its future behavior, one would have to disturb

Vignettes: Installment in a Debate

***In Michael Innes's mystery novel* The Weight of the Evidence, first published in 1943 (now a HarperPerennial paperback), one Professor Pluckrose is killed by a falling meteorite while reposing in his college quadrangle. It is quickly ascertained that the meteorite arrived not directly from the heavens but, in the manner of Galileo's demonstration of the law of falling bodies, from the college tower. In investigating the case by attempting to penetrate the academic mind detectives Appleby and Hobhouse interview Pluckrose's colleague Professor Crunkhorn and his academic subordinate Mr. Church.**

Crunkhorn hesitated . . . . "Perhaps it had better be mentioned that Pluckrose had a good deal of mathematics. He was a biochemist and interested in genetics, which requires a certain amount of mathematics nowadays. He had enough mathematics to ride various hobbies. And one of these was ballistics." . . .

"Ballistics?" [Appleby] said. "You mean—?"

"Rules," said Church, "worked out by Newton and others when the bosses told them that the targets must jolly well be hit. Which is the sort of supply-and-demand affair they call science."

And professor Crunkhorn and his assistant eyed each other with a sudden serious animosity. At the moment, thought Appleby, one of the radical issues between the old and the young. Is science the disinterested pursuit of knowledge which the world may apply if it will? Or is it an activity always dependent upon economic and political demands?

the brain so much as to affect the behavior. Perhaps the flavor left by Bohr's more adventurous musings on quantum theory and complementarity is best conveyed by a joke he used to tell on himself:

> The first talk was brilliant, clear and simple. I understood every word. The second was even better, deep and subtle. I didn't understand much, but the rabbi understood all of it. The third was by far the finest, a great and unforgettable experience. I understood nothing and the rabbi didn't understand much either.

Beginning in the mid- and late 1930s, when he was close to 50, Bohr had a sort of physicist's rebirth. He developed a new metaphor for atomic nuclei, based on an analogy to liquid drops, that has proved remarkably fruitful. Using ideas of this kind, he was able immediately to seize upon the discovery of nuclear fission by Hahn, Strassman, and Meitner in 1939 and to provide in very short order the foundations for a semi-quantitative understanding of its features, such as which nuclei were the most likely to fission, how much energy would be necessary to make fission likely, the likely decay products, and so forth. This work was epitomized in a truly remarkable paper written with John Wheeler, wherein several concepts (semiclassical quantization of extended objects, use of Morse theory arguments in physics, instantons) that would only be fully appreciated decades later appear in germinal form. The more dramatic immediate impact of this work, of course, was in the development of nuclear weapons and nuclear power. Bohr attempted, with complete lack of success, to influence the political fallout from these developments, a tragicomic story that is recounted both in Pais's book and, with different emphases, in Richard Rhodes's *The Making of the Atomic Bomb*.

Coming back to the issue raised at the top of this discussion, I think this recounting suggests why in the ordinary course of their training most physicists, let alone others, may get an insufficient appreciation of Bohr's contribution. It is because his most characteristic work was in provisional theories, often of a semi-phenomenological character, whose technical content has been largely superseded. Even in the area of interpretation of quantum mechanics, where his ideas are still very much alive, it seems most unlikely that a doctrine of limitation and renunciation, however revolutionary and constructive in its time, can satisfy ambitious minds or endure indefinitely. Like the rest it will be digested and transformed and in its new form no longer bear Bohr's distinctive mark or name explicitly. Yet, as his contemporaries realized, no one will have contributed more to the finished product. Pais's book, by telling the story as it happened, helps capture a rich and intrinsically interesting intellectual style and preserve its achievements.

The preceding discussion has emphasized the intellectual side of Bohr. However, it would be wrong to fail to mention the impression one gets, both from Pais's book and from the lovely collection of reminiscences *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues* (S. Rozental, Ed.; Elsevier, 1985), of the rootedness and inner harmony of his life and personality. He was apparently regarded with deep affection by all who knew him well. Pais's book contains many warm anecdotes and amusing stories, and some outright jokes, that help make it entertaining as well as edifying.

A fascinating man, Bohr; and a fascinating book, this, which should help do justice to his memory.

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An Advocate from the Past

Women in Science. With an Introductory Chapter on Woman's Long Struggle for Things of the Mind. H. J. MOZANS. University of Notre Dame Press, Notre Dame, IN, 1991. xxiv, 452 pp. Paper, $14.95. Reprint, 1913 ed.

Originally published in 1913, *Woman in Science* echoes both the problematics and the opportunities confronting well-educated and ambitious "new women" of the early 20th century. Its unlikely author, a Jesuit professor of science at the University of Notre Dame, was determined to demonstrate that women had a capacity for all intellectual activity, and most particularly for science. His method was to provide an extraordinary catalogue of exceptional women who had—or should have—won prizes, advanced degrees, and accolades from ancient times to the 20th century.

John Augustine Zahm, writing under the anagrammatic pseudonym H. J. Mozans, often lectured on scientific topics to popular audiences. His account of women scientists mixes quotations from well-known sources, anecdotes, and wry humor into a detailed account of women's contributions in the major scientific fields, including medicine, archeology, and technology. He also describes the "many and diverse obstacles" that opposed women's advancement in education and thus in science. His capacity for using French, German, Italian, and English sources makes the account unusually broad-based and leads him to conclude, for example, that the Golden Age of Greece provided no golden opportunities for women whereas the so-called Dark Ages permitted many women in Italy unprecedented access to university education in science and medicine. At some points he virtually catalogues women scientists at work, including physicians in the Middle Ages, women mathematicians in early modern Italy, and women natural scientists in the 19th century. At other times, his detailed sketch of intrepid women like Octavie Coudreau, who explored and wrote six volumes about the Amazon River, highlights the ways in which family connections and extraordinary courage and conviction, as well as scientific talent, let them join the ranks of exceptional scientists.

Zahm's observations are comparative, provocative, and often preliminary. He never hesitates, however, to draw his own independent conclusions even as he calls Voltaire flippant and cocksure for the *philosophe's* dismissal of women's intellectual capacity. Zahm's own moral intention and didacticism lead him toward an alternative enthusiasm and a somewhat romantic notion of what women could and should be doing as scientists. Thus women in medicine are inevitably compassionate and charitable, while most who studied astronomy never forgot their earthly duties. He gives disproportionate attention to women involved in religious orders, overcompensating perhaps for the tendency of others to ignore the intellectual life afforded to women in convents and religious orders. In general, Zahm follows John Stuart Mill's argument that it is the circumstances of women, particularly their access to education, that accounts for the achievement (or lack of achievement) by women.

There are aspects of the book that grate on current sensibilities. One is Zahm's presumption in using the singular "woman" in his title and throughout the book. Few scholars today would be comfortable identifying a generic woman; no simple stereotype exists in either history or science. Much of Zahm's historical narrative is couched in terms of women in a world of men, but there is virtually no discussion about the ways in which the scientific enterprise is encoded with masculine values that in themselves may inhibit women's participation.

Zahm's "exaggerated optimism," as Cynthia Russett points out in her preface to this edition, allows him to envision significant possibilities and major contributions by women in the 20th century. Zahm's volume answers his own rhetorical query: Given the accomplishments of so many women, at so many times and in so many places, how can one doubt their capacity for original work in