Introduction

Helge Kragh

The papers presented in this volume are the outcome of the Copenhagen conference that in June 2013 marked the centenary of Niels Bohr's atomic theory, a milestone in the history of physics. After Heilbron's commissioned public lecture, the conference itself was set off by Hastrup's "Prelude." Unsurprisingly, all of the subsequent papers deal with aspects of Bohr's scientific work, either related to his original atomic theory, to the later quantum mechanics, or to the still ongoing philosophical discussion concerning the strange quantum world. While in many of the papers Bohr is the central figure, in others he plays a more peripheral role. Inevitably, given the subject, there is some overlap between several of the papers. A few of them are not primarily about Bohr and his theories, but focus on scientists with whom he interacted, such as Arnold Sommerfeld in Germany, George Hevesy in Hungary, Paul Ehrenfest in the Netherlands, and J. J. Thomson and Ernest Rutherford in England. Although the volume spans widely, it does far from cover all aspects of Bohr's contributions to physics, philosophy, and culture. To mention but one example, none of the papers deals with nuclear physics, a field in which Bohr did very important work in the 1930s. Fortunately this aspect of his scientific life is well covered in the scholarly literature, including volume 9 of the *Niels Bohr Collected Works*. Other possible examples are Bohr's political activities and his cultural role in Danish society.

The volume is divided into four parts, the first one dealing with the origin of Bohr's atomic model and how it subsequently was presented in more popular forms. When 26-year-old Niels Henrik David Bohr set off for Cambridge, England, in September 1911 atomic theory was not on his mind. His main concern was the electron theory of metals, the subject of his doctoral dissertation, and also, with no less intensity, his beloved fiancée and later wife Margrethe Nørlund. As **Finn Aaserud** points out in his paper, the two concerns were not separate. Based on correspondence between Niels and Margrethe from 1910 to 1913, to which the Bohr family has given him special access, Aaserud unveils traits in Bohr's personality and thinking that have not been previously appreciated, if known at all. Bohr, we learn, was not only a highly ambitious physicist; he was also a sensitive human being who shared the literary interests of Margrethe. Moreover, the letters offer a rare insight in his early views concerning philosophy and religion. Unable to believe in the Christian God he severed his ties to the Danish Lutheran Church in 1912, never to return to it. The correspondence also shows how important Margrethe was for his scientific project even at this early stage. Bohr needed someone to share his thoughts with, and Aaserud argues that in this respect she was essential to his scientific creativity, such as it manifested itself in the great Trilogy of 1913.

The target for Bohr's travel in 1911 was the famous Joseph J. Thomson, whom Bohr admired and in a letter to Margrethe described as "a tremendously great man." It is Thomson and his pre-quantum atomic model, rather than Bohr and his model, which is the subject of **Jaume Navarro**'s paper. Or perhaps one should say so-called model, for Navarro argues that what is generally known as the Thomson model was not central to Thomson's thinking. The story presented in science textbooks going from Thomson's "plum-pudding model" over Rutherford's nuclear model to Bohr's quantum model is, if not a myth, then a truth with great modifications. According to Navarro, Thomson's ideas of matter and radiation can be better understood in terms of so-called Faraday tubes, a concept which he used to explain the discrete subatomic charges that he called "corpuscles" (but that other physicists called electrons). Bohr was unimpressed by Thomson's model, whether conceived in terms of Faraday tubes or electrons, just as Thomson later ignored Bohr's model. Navarro suggests that he did not see Bohr's model as a rival to his own, based as it was on the notion of Faraday tubes.

Whatever can be said about Bohr's indebtedness to Thomson's ideas of atomic structure, the theory he introduced in the summer of 1913 was completely different and empirically much more fertile. **Helge Kragh**'s paper gives a brief survey of the theory and its development until the emergence of quantum mechanics, emphasizing the changes in the theory and the associated models that made the Bohr atom *anno* 1923 very different from the original ring atom. About the only common denominator was that the electrons moved in stationary orbits. Whereas his later and more advanced model was a many-electron model designed to explain the periodic system of the elements, the successes of his original model were largely limited to one-electron atoms or ions. Kragh calls attention to two of the lesser-known predictions the significance of which only became recognized much later. One of them was the existence of atoms in very high quantum states, what is known as Rydberg states but what more appropriately might be called "Rydberg-Bohr states." No less important was Bohr's insight that different isotopes have slightly different optical spectra and that the spectral shift may be used to identify and possibly isolate isotopes. Remarkably, Bohr did not consider his prediction of the isotope effect very important. It was left to others to verify it and develop it into a powerful method applicable to a wide range of sciences.

Arne Schirrmacher's contribution analyses in a comparative perspective the model aspect of Bohr's atomic theory, a theme which is also developed in some of the later contributions. What is a model and what role does it play? How does the concept of model relate to the concepts of analogy and metaphor? By looking at representations of different kinds of atomic models in the period from about 1900 to 1930, Schirrmacher places Bohr's planetary model in a historical tradition that includes, among others, Jean Perrin in France and Johannes Stark and Arnold Sommerfeld in Germany. He suggests that the planetary model worked as an important conceptual tool for the later correspondence principle and in this way helped in transcending the limitations of the semi-classical Bohr theory. As he makes clear and illustrates with historical examples, atomic models may be mental or mathematical (as they are in quantum mechanics), but they may also be visualizable and concrete – made by wires and wooden balls for example. The latter kind of models typically serves pedagogical purposes and appears in elementary textbooks and popular expositions.

In this respect there is a direct connection between Schirrmacher's paper and the one of **Kristian Hvidtfelt Nielsen**, whose subject is popular science books on atomic theory in the period 1918-1924, that is, before the emergence of quantum mechanics. Although relativity theory attracted far more popular interest than atomic theory, there was a market for presenting to the layman Bohr's new ideas about atomic structure and the strange implications of the quantum atom. Nielsen analyzes a dozen popular books published in Germany, Denmark and the United Kingdom, paying attention to similarities as well as differences. Authors included journalists, physicists and philosophers (*e.g.*, Bertrand Russell), and they made use of a variety of metaphors and analogies to explain the mysteries of the atom. As Nielsen writes, readers of such books "would have been able to see the Bohr atom as a planetary system, but also as different kinds of musical instruments, a staircase, and even an angry man." Historiography of popular science has only recently attracted wide attention. Nielsen's paper is a contribution to a growing branch of history of science which takes seriously the rich resource represented by popular books and articles on science.

The second part of the volume deals primarily with developments in atomic theory that built on and extended Bohr's quantum theory of the atom. The earliest and most important of the extensions was due to Sommerfeld and his school in Munich. Sommerfeld's biographer **Michael Eckert** describes how the German physicist responded to Bohr's theory and eventually, after some hesitation, decided to engage fully in generalizing it into what became known as the "Bohr-Sommerfeld theory" of atomic structure. The correspondence that Eckert cites from the years 1913-1916 shows that Sommerfeld only came to embrace the Bohr atom after a period of considerable doubt with regard to its value and explanatory power. Only by late 1915, stimulated by work on the Zeeman effect and also the Stark effect, was he ready to generalize Bohr's theory into a much more sophisticated theory in which the electrons moved in elliptic orbits with quantized eccentricities. By taking into account the theory of relativity he famously explained hydrogen's fine structure. Since then Sommerfeld and his school greatly influenced the course of atomic and quantum physics.

Bohr's correspondence principle, much discussed in the past and still a matter of discussion among philosophers of science, is dealt with by several of the authors. As **Robert Rynasiewicz** points out, there are several versions of "the" correspondence principle and different opinions of what role it plays in atomic physics and physics generally. Although there may be no "correct" version, there are some versions that are more historically authentic than others. Bohr and his assistant Hendrik Kramers originally formulated it as a way of deriving intensities and polarizations of spectral lines, whereas later physicists typically conceived it, and still conceive it, as a constraint of theory construction. By carefully following the historical development since 1918 Rynasiewicz seeks an answer to the "puzzle" of how the correspondence principle passed from one form to the other. Although admitting that Bohr's statements were not always very clear or even consistent, he concludes that, to Bohr and Kramers, the correspondence principle was more than just a selection principle. In addition to discussing the ordinary correspondence principle concerned with radiation and transition probabilities, Rynasiewicz also refers to various extensions of it, including Wolfgang Pauli's so-called mechanical correspondence principle from 1922.

One thing is the understanding of a principle of physics, while another is how it is actually used in connection with practical problems. **Martin Jähnert** notes that many physicists in the early 1920s felt a tension between the original formulation of the correspondence principle and the problems they were addressing. They consequently tweaked the principle, in the sense that they adapted parts of it without changing its core idea. In connection with a case study of attempts to explain the Ramsauer effect before wave mechanics, Jähnert describes how James Franck and Friedrich Hund in Göttingen adapted the correspondence principle to scattering problems. Although their correspondence approach did not lead to a satisfactory explanation of the effect, the case nicely illustrates what Jähnert calls "adaptive reformulation." Most physicists at the time did not regard the correspondence principle as either a fundamental theoretical principle or a magic wand (as Sommerfeld called it), but instead as a research tool that, if properly adjusted, could be used to solve concrete problems.

The old quantum theory scored one of its most impressive successes when Paul Epstein and, independently, Karl Schwarzschild in works of 1916 were able to calculate the Stark effect in hydrogen, that is, the splitting of the spectral lines in an electric field. When three years later Kramers used the correspondence principle to calculate the polarizations and intensities of the components, the success seemed complete. Sommerfeld enthusiastically spoke of the Stark effect as "a beautiful capstone on the edifice of atomic physics." However, **Anthony Duncan** and **Michel Janssen** disagree, showing in their paper that the praise was not quite deserved. This was not clearly recognized at the time, but a detailed comparison of the old theory of the Stark effect and the one based on the new wave mechanics, as first presented by Epstein and Erwin Schrödinger in 1926, makes the inadequacies of the older theory clear. Apart from giving a better agreement with experiment, wave mechanics avoided a problem that was inherent in the old method, namely, that the predicted orbits depend on the coordinates chosen to impose the quantum conditions. As Duncan and Janssen demonstrate in detail, there is no corresponding problem of non-uniqueness in the wave-mechanical treatment of the Stark effect.

The correspondence principle was not the only general principle that played an important role in the development of the old quantum theory. So did the adiabatic principle or, in Paul Ehrenfest's original terminology, the adiabatic hypothesis – which states, roughly, that only adiabatic transformations can be performed mechanically. This is the subject of the paper of **Enric Pérez** and **Blai Pié**, who trace the history of the hypothesis-turned-a-principle and, at the same time, the early relationship between Bohr and the Dutch physicist. Ehrenfest initially dismissed the "cannibalistic Bohr model." However, after Bohr began to use the adiabatic principle – and to highlight the significance of Ehrenfest's insight – Ehrenfest turned into an enthusiastic supporter of Bohr's theory. In 1918 Bohr reformulated the hypothesis into what he called the "principle of mechanical transformability" in order to stress the difference from the thermodynamical origin of Ehrenfest's hypothesis. From their reading of Bohr's later papers Pérez and Pié suggest that Bohr returned to an understanding of the adiabatic principle that was less mechanistic and more in line with Ehrenfest's original hypothesis.

Bohr's 1918 memoir "On the Quantum Theory of Line Spectra" was complex, dense, and difficult to read. It was important not only because it introduced the correspondence principle and Bohr's new version of the adiabatic principle, but also because it used the mathematical technique of action variables and associated angle variables in a quantum context. **Michiyo Nakane** details in her paper the historical roots of action-angle variables and their relation to the Hamilton-Jacobi equation, a story that involves in equal measure eminent mathematicians, astronomers and physicists. Carl Jacobi, Henri Poincaré and Carl W. L. Charlier, a Swedish astronomer, are some of them. The story also involves the quantum calculations of the Stark effect made in 1916 by Epstein and Schwarzschild, which Nakane discusses in mathematical details. It was by combining the ideas of the two German scientists – a physicist and an astronomer – that Bohr introduced his definition of action-angle variables in his memoir of 1918. He used it to explain the properties of conditionally periodic atomic systems.

While the papers of Part 2 mentioned so far all deal with aspects of the old quantum theory, **Jeroen van Dongen** takes us to the year 1926, when the "Einstein-Rupp" experiments were performed. What experiments? They are virtually unknown today even among many historians of modern physics, which van Dongen argues is because they have purposefully been written out of history. The German experimentalist Emil Rupp is known to have been guilty of scientific misconduct, even to have fabricated data and imagined non-existent experiments. So why taint Einstein's reputation by connecting him with the fraudulent Rupp? Van Dongen lifts the Einstein-Rupp experiments out of the dark, arguing that they were directly as well as indirectly important in a crucial phase of the new quantum mechanics. He documents Einstein's role and the effect the experiments had on his thinking about light, and suggests that they played a role in shaping Bohr's ideas about complementarity and quantum uncertainty. In a historiographical section he accuses some physicists and historians of science of immoral behavior by deliberately omitting Rupp and his experiments from the historical record.

The third part of the volume is devoted to philosophical and contemporary aspects relating to Bohr's theories. The first two contributions both deal with the concept of model in the Bohr atom, the issue also discussed in Schirrmacher's paper in Part 1. According to **Giora Hon** and **Bernard Goldstein**, there was no Bohr model. Or rather, Bohr did not intend to develop a *model* of the atom in 1913, but instead proposed a *theory* of the real constitution of the atom. Since Bohr did not say so explicitly, their evidence for the claim is and must be circumstantial, namely, based on Bohr's and contemporary physicists' use of the term "model" and associated terms. They consequently analyze how words were used at the time, making the historiographical point that philosophers and historians of science should take words and phrases seriously as markers of concepts. Hon and Goldstein further suggest that Bohr associated "model" with non-quantum representations of the atom, such as in the theories of Thomson and Rutherford. This, they conjecture, was the reason why Bohr did not label the rival atomic theory of John Nicholson a model, but called it a theory.

Without taking issue with the model-theory problem discussed by Hon and Goldstein, **Theodore Arabatzis** and **Despina Ioannidou** place the Bohr model within the modern philosophical tradition concerning models and analogies. Inspired by Mary Hesse, Ernan McMullin and other philosophers of science they argue that Bohr's model – and they have no problem with "model" – was motivated by the analogy between the atom and the planetary system. In agreement with Jan Faye they consider the Danish philosopher Harald Høffding a likely source for Bohr's interest in models and analogies. But analogies may be positive as well as negative, and Arabatzis and Ioannidou pays particular attention to the negative analogy between electrons and planets (the first interact repulsively and the latter attractively). The two authors argue that the negative analogy served as the prime motivation for Bohr to develop Rutherford's nuclear atom into an atomic model, whereas many of its successes in spectral physics were motivated by positive analogies. As to the concept of model, they take it to be an idealized and simplified representation of a physical system.

The famous Einstein-Podolsky-Rosen (EPR) argument of 1935 and Bohr's reply to it have given rise to a minor industry in the philosophy of physics. The paper by **Guido Bacciagaluppi** is a contribution to this industry, a careful reading of Bohr's reply that differs from most other readings. The problem of non-locality in quantum mechanics was a major concern in the EPR paper, but one that many have argued Bohr did not find very important or perhaps even did not really understand. Bohr spent much of his reply discussing the quantum behavior of a single particle and thus may seem to have missed the "spooky action at a distance" that arises from the two-particle EPR experiment. However, Bacciagaluppi argues that there is a clear analogy between the two cases and that Bohr was fully aware of it. Bohr did not misunderstand the locality problem raised by Einstein and his coauthors. Bohr discussed the case of a single particle passing through a slit, which according to Bacciagaluppi corresponds to the EPR case of two separate particles. At the end of his paper Bacciagaluppi deals with the prehistory of EPR, providing "a more charitable explanation" of why Bohr did not react to earlier versions of Einstein's worries about locality.

While far from as well known as the EPR reply, a memoir that Bohr and Léon Rosenfeld wrote in 1933 on the quantum-electromagnetic field has recently attracted a good deal of attention. According to standard history of physics the Bohr-Rosenfeld article was soon forgotten, but this, say **Thiago Hartz** and **Olival Freire**, is quite wrong. They document in their paper that the memoir of 1933 was read and considered important at least until the mid-1960s. Among the physicists who studied it and were inspired by its ideas were Jacques Solomon in France, Matvei Bronstein in Soviet Russia, and Bryce DeWitt in the United States. A major point in the Hartz-Freire paper is that physicists used the memoir by Bohr and Rosenfeld in a variety of ways, often with purposes and meanings quite different from those of the original authors. Hartz and Freire generalize that "science consists of uses" and advocate that uses of scientific theories should be given as much (or more) attention as the theories themselves. In this respect, their paper can be compared with the one of Jähnert on uses of the correspondence principle.

Bohr's view concerning the divide between the classical and the quantum world has been the subject of much philosophical discussion with no indication so far of consensus. **Henrik Zinkernagel** enters the debate by asking if Bohr was a "quantum fundamentalist," that is, if he believed in the ontological claim that everything is fundamentally of a quantum nature and, epistemologically, that what is in the world can ultimately be described in terms of quantum mechanics. He argues against recent attempts to see Bohr as an ontological (if not epistemological) fundamentalist, among other reasons because such a position would leave the measurement problem unsolved. Zinkernagel's analysis leads to the conclusion that Bohr was at most a "restricted quantum fundamentalist" and that, to Bohr, physical systems described by classical mechanics were indispensable. But all this is surely of philosophical and biographical, not scientific, interest? Not according to Zinkernagel, for he points out that similar problems pop up in the attempts to understand the very early universe and the transition from a primordial quantum foam to the classically describable cosmos. Bohr's view on quantum mechanics is still of scientific relevance, he concludes.

If there is a single principle which permeates and even defines all of quantum mechanics, it is the superposition principle of states, the meaning and history of which Hari Dass investigates in his paper. This fundamental principle – "the very bedrock of quantum theory" – is often considered as just a consequence of the linearity of the Schrödinger equation, but **N. D. Hari Dass** points out that in a historical context the concept of wave function needs to be distinguished from the concept of state. He opines that "the history of an idea is as important as the idea itself," a statement that historians (more than philosophers) of science surely will agree upon. The idea of a quantum state can be traced back to the stationary states of Bohr's atomic theory of 1913, when the states were associated with orbiting electrons. It was only with Paul Dirac's transformation theory, and in particular with his classic textbook *Principles of Quantum Mechanics*, that the superposition principle came to be seen as truly foundational and wholly different from the superposition of waves in classical physics. Hari Dass writes about the "extreme allergy to any change" that characterizes the superposition principle, yet he also briefly speculates about a future development of quantum mechanics in which this bedrock may have to be modified.

Physicists and philosophers continue to investigate the meaning of quantum mechanics and suggest new interpretations of the wave function. Based on the ideas known as "protective measurement" **Shan Gao** outlines a new picture of quantum reality which, he argues, justifies two of the fundamental assumptions in Bohr's old atomic theory: First, that electrons are particles and, second, that they undergo discontinuous quantum jumps. Although Gao's paper is of a scientific-conceptual, and not historical nature, he does point out that the new picture can be seen as a revised version of Schrödinger's short-lived proposal of 1926, namely, that the wave function ψ represents the electron's charge density by $e|\psi|^2$. Since Gao also says that the motion of electrons can be conceived as guided by the wave function, another possible predecessor may be Louis de Broglie's "pilot wave" interpretation of 1927. Electrons and other particles are described as small objects that do not move continuously, but discontinuously and randomly. Moreover, the picture presented by Gao is realistic in so far that $|\psi|^2$ not only gives the probability of finding a particle in a certain location, but also the probability density of the particle actually being at this location.

It was a crucial moment in the history of quantum physics when Heisenberg and Pauli, in the spring of 1925, concluded that the very concept of electron orbits in atoms and molecules must be discarded. And yet Bohr's correspondence principle requires that for large quantum numbers, quantum mechanics leads to classical mechanics. **Michael Nauenberg** reexamines the question of the quantum-classical or micro-macro relationship by referring to the historical situation in 1926, when Schrödinger thought he could describe elliptically moving electrons as compact wave packets. However, as H. A. Lorentz and Heisenberg pointed out, it is not possible to construct *permanently* localized wave packets that represent electrons in high states of the atom. Nauenberg uses "Schrödinger's misunderstanding" to consider the question from a modern perspective, referring to calculations of his own and also to experiments with Rydberg atoms containing a single electron in a high quantum state. In agreement with expectations based on the correspondence principle the result is elliptic orbits resembling those of the Bohr-Sommerfeld theory.

The fourth and last part of the volume contains papers of a historical nature with an emphasis on national, political and institutional aspects related to Bohr's life until about 1930. What since 1965 has been the Niels Bohr Institute (but until then was [Copenhagen] University's Institute for Theoretical Physics) was inaugurated in 1921 and soon became of singular importance to the development of quantum physics, indeed no less important than the theories emerging from Bohr's fertile mind. As **Peter Robertson** observes in his paper on the early history of the institute, the term "theoretical" in its name should not be understood as opposite to "experimental," but rather as referring to fundamental or pure physics. The institute grew rapidly in size, staff and visitors during the 1920s, not least as a result of generous funding from the Rockefeller-funded International Education Board. Why did the Copenhagen institute become the Mecca of quantum physics rather than, say, Göttingen, Berlin or Cambridge? Robertson points to two main reasons, one being Denmark's neutrality during World War I and the other being Bohr's personal view on international cooperation in science. Indeed, with 63 visiting scientists from 17 different countries in the period 1921-1930 Bohr's institute was truly international.

Bohr's international attitude was nothing new, as **Shaul Katzir** documents in his paper on Bohr's stay with Rutherford in Manchester during World War I. The war came to interfere with the scientific work of both Rutherford and Bohr, although in the case of the latter not very seriously. Bohr was eager to maintain the fragile links between the scientific communities of the belligerent countries, to mediate between scientists from Great Britain and the Central Powers. At a time when both German and British scientists experienced difficulties in addressing each other directly, the young Danish physicist was able to address both and, for example, establishing an indirect link between Rutherford and Sommerfeld. He was a mediator in atomic physics in the same sense that Willem de Sitter in the Netherlands was a mediator in general relativity theory. Katzir also describes Rutherford's heavy engagement in war-related research and his somewhat ambivalent attitude to it. Although a patriot willingly applying his scientific expertise for the cause of British victory, Rutherford too hoped to restore the international character of science after the end of the terrible war.

News of the Bohr atom arrived at different times and was received with different interest in the various European countries. **Gábor Palló** describes how the model or theory was received in Hungary, in 1913 still part of the Austro-Hungarian Empire. This happened at a very early date, in a public lecture of November 1913, but only because the speaker, the radiochemist George Hevesy, was a friend of Bohr and well informed about his ideas. Indeed, while staying together in Manchester in 1912 Hevesy significantly influenced Bohr's thoughts and subsequently became an early advocate of the new atomic model. But, as Palló points out, Hevesy's support was not enough to turn the model into a research subject for the small Hungarian community of physicists and chemists. It was not as a quantum theory that it attracted some interest, but instead as a small part of the general zeitgeist as it manifested itself in publications of a natural-philosophical kind. The local culture adapted the Bohr model to its own, more metaphysical interests.

As Hevesy promoted Bohr's atomic model in Hungary, so did Carl Wilhelm Oseen promote it in Sweden, if somewhat later and in a different and, at first, more reserved way. **Karl Grandin**'s paper covers a broad ground, dealing not only with the reception of Bohr's work in Sweden but also with the central role that Bohr came to play in the Swedish physics community, leading in 1929 to the unprecedented proposal to make him a member of the Nobel Committee. Oseen was originally critical of the quantum features of Bohr's model, which he wanted explaining in terms of classical physics, but he was nonetheless positively inclined towards the model. It became known in Sweden not primarily through Bohr's writings but through lectures given by Bohr's young "diplomats," Kramers and Oskar Klein. Apart from describing how this happened, Grandin details Oseen's crucial role in the negotiations that led to Bohr's Nobel Prize in 1922 and, closely connected with it, the reserved 1921 prize being awarded to Einstein. The papers by Palló and Grandin both testify to the importance of personal relations in the dissemination of the new atomic theory and, more generally, in Bohr's life and career.

Altogether the papers in this volume give a many-facetted picture of Niels Bohr, his seminal contributions to atomic physics, and the scientific environment of which he was part and did so much to shape. They tell us about the fascinating details of Bohr's semi-classical quantum atom, about his deep thoughts concerning the meaning of quantum mechanics, and about how his ideas were received abroad. Bohr's atomic model has long been obsolete and is today mainly of pedagogical and historical interest, but the consequences of the revolution that started in 1913 are still much alive.