Correspondence principle versus Planck-type theory of the atom
Author(s): Sandro Petruccioli
Source: *Archive for History of Exact Sciences*, Vol. 68, No. 5 (September 2014), pp. 599-639
Published by: Springer
Stable URL: https://www.jstor.org/stable/24569571

Arch. Hist. Exact Sci. (2014) 68:599–639
DOI 10.1007/s00407-014-0137-5

Correspondence principle versus Planck-type theory of the atom

Sandro Petruccioli

Received: 19 February 2014 / Published online: 23 April 2014

Abstract This article examines the problem of the origins of the correspondence principle formulated by Bohr in 1920 and intends to test the correctness of the argument that the essential elements of that principle were already present in the 1913 "trilogy". In contrast to this point of view, moreover widely shared in the literature, this article argues that it is possible to find a connection between the formulation of the correspondence principle and the assessment that led Bohr to abandon the search for a Planck-type theory. In fact, a thorough examination of Bohr's works shows that the birth of this principle coincided with the depletion of a research program whose origins may date back to Bohr's stay at the Rutherford's laboratory (summer 1912). Finally, this article argues that original program of research was abandoned when it became clear that Planck's quantum hypothesis for the harmonic oscillator was not an adequate support for the theoretical architecture of atomic physics; namely, there was evidence enough to justify a most drastic conclusion, according to Bohr: "I do not think that a theory of the Planck type can be made logical consistent".

1 Introduction

In the spring of 1920, Niels Bohr was invited to give a lecture at the Deutsche Gesellschaft Physikalische. This was a prestigious venue, giving Bohr the opportunity

Communicated by: Jed Buchwald.

I wish to thank Helge Kragh for his critical comments and suggestions that were particularly useful in the revision of the first draft of this article.

S. Petruccioli (✉)
Università dell'Aquila, L'Aquila, Italy
e-mail: sandropetruccioli@gmail.com

to illustrate his ideas to the most qualified and influential physicists of the time. Also present in addition to Planck were James Franck, Max Born, Alfred Landé and, of course, Albert Einstein who met Bohr here for the first time.

For some time, now physical research had been moving on an entirely new ground, full of the pitfalls arising from the crumbling of the very basis of classical mechanics and electrodynamics. Although some theoretical proposals—Bohr's brilliant insights among them—had achieved significant empirical evidence, there was not a single unified program catalyzing the various researches. One lived in a state of great uncertainty, both conceptual and methodological, and those fragments of theory that could boast satisfactory experimental support were not always appreciated because of their fragile and limited interpretative value. The very title chosen by Bohr for the conference reflected this general state of instability. Putting aside the youthful ambition that had led him to propose as his first major publication the formulation of a theory on the constitution of atoms and molecules,[1] he now concentrated on providing a contribution to the understanding of the phenomenon of radiative behavior of atoms, especially when subjected to the action of electric and magnetic fields. He entitled the conference "Über die Serienspektren der Elemente," and the text was published in October of the same year on the *Zeitschrift für Physik*.[2]

Bohr concluded his speech with these words: "In closing, however, I should like to emphasize once more that in this lecture I have only intended to bring out certain general points of view lying at the basis of the spectral theory. In particular, it was my intention to show that, in spite of the fundamental differences between these points of view and the ordinary conceptions of the phenomena of radiation, it still appears possible on the basis of the *general correspondence* between the spectrum and the motion in the atom to employ these conceptions in a certain sense as guides in the investigation of the spectra."[3] So he kept repeating that the laws of spectroscopy had definitively undermined the ordinary interpretation of radiative phenomena, breaking the link that should exist between optical and mechanical frequencies according to classical electrodynamics. Nevertheless, Bohr was certain of the existence of a correspondence between spectrum and atomic motions that would allow to repair that fracture, at least heuristically. According to him, it was because of this "unusual" relationship that it was possible to use the outdated classical concepts as guidelines for the investigation of atomic spectra. The term "correspondence" might look like a linguistic ruse to save the mechanical model of the atom, despite the existence of radiative processes no longer reconducible to the motion of charged particles. In fact, some colleagues considered this idea as some sort of "magic wand."[4] Of course, this was

[1] Bohr (1913b, c, d).

[2] Bohr (1920). The article was published in English in Bohr (1922b, pp. 20–60); also in Bohr (1976, pp. 242–282) [henceforth CW3].

[3] CW3 282; my emphasis.

[4] Cfr Kragh (2012, pp. 189–225). Sommerfeld in the first edition of *Atombau und Spektrallinien* wrote: "Bohr has found a magic wand [Zauberstab] in his analogy principle [...] which without clearing up the conceptual difficulties allows him to make the results of the classical wave theory directly useful for the quantum theory" Sommerfeld (1919, p. 403). In a letter to Bohr dated November 11, 1920, Sommerfeld expressed his doubts on a "principle" whose origins were in his opinion "foreign to the quantum theory," CW3 690.

not Bohr's intention, who in his lecture actually elevated the idea of correspondence to the status of principle, a "general law for the occurrence of transitions between stationary states."[5]

But there's more. Bohr associated this new principle to a radical change of strategy. Right in front of Planck, he remarked that if we try to use his famous quantum hypothesis in order "to explain the spectra of the elements [...] we encounter difficulties, because the motion of the particles in the atom, in spite of its simple structure, is in general exceedingly complicated compared with the motion of a Planck oscillator."[6] Then, he pointed to alternative ways that allow to generalize Planck's ideas in order to make them useful in the study of the properties of atomic systems. To understand the depth of the rift that was produced in this way within the quantum theory of the atom, it is sufficient to go back to what Bohr had argued, with apparent optimism, a few years before: Planck's original assumption as to the possible values for the energy of an atomic vibrator is consistent with the postulate of the atomic theory, according to which (a) "an atomic system can only exist permanently in a series of states corresponding with a discontinuous series of values for its energy," and (b) "any change of the energy of the system including absorption and emission of electromagnetic radiation must take place by a transition between two such states, [...] termed 'the stationary states' of the system."[7] At least until the great breakthrough of the correspondence principle, Bohr's research program was therefore firmly hinged on the idea that the Planck's quantum theory, as well as provide an effective interpretation of the phenomena of temperature radiation and specific heat, is also the tool that allows us to study situations "in which the statistical nature of the phenomena in many instances is only of secondary importance."[8] In other terms, according to Bohr, the quantum theory of atomic structure and its radiative processes was in any case a Planck-type theory.

It would therefore appear reasonable to say that in 1920 Bohr considered the original research program outdated and also that the need for its radical reformulation derived from the existence of a relationship of mutual exclusion between the two terms evoked in the title of this article, "correspondence principle" and "Planck-type theory." But things are not as simple as they seem, and such a statement should nevertheless be confronted with a different interpretive hypothesis that has long enjoyed much credit in the literature. Let's see what it is.

At the time of the Berlin conference, it had been less than a decade since Bohr had begun to grapple with the problems of the internal constitution of atoms, making his debut in the scientific community with the quantum theory of the hydrogen atom. From the first pages of the "trilogy," Bohr had found a kind of anchor of salvation in the peculiar convergence existing between quantum hypothesis and the classical theory of radiation in the so-called limiting region of low frequencies. Here, despite the discontinuous nature of the processes of radiation, it seemed fair to say that the

[5] CW3 250.

[6] Ibid., pp. 243–244.

[7] Bohr (1916, unpublished paper), in Bohr (1981, pp. 433–461: 434) [henceforth CW2].

[8] Ibidem.

frequency of radiation and frequency of electronic motion were identical. Moreover, the use of considerations regarding the limiting region had allowed Bohr to circumvent some obstacles in an attempt to extend Planck's ideas to the study of atoms and especially to secure the necessary empirical evidence for his theory. On the one hand, he had obtained a theoretical expression of the Rydberg constant coinciding, within measurement errors, with the value long known by spectroscopists. On the other hand, he had been able to predict that certain lines observed by William Pickering in stellar spectra and by Alfred Fowler in the laboratory were not attributable to ionized hydrogen, but to ionized helium.[9] It was not much compared to the ambitious project that Bohr had announced in 1912 at the end of his stay at the Rutherford laboratory.[10] But it was enough to attract Einstein's interest and to have him say that it was "an enormous achievement [and that therefore] the theory of Bohr must then be right."[11]

The limiting considerations that we find in the writings of 1913 seem very similar to those Bohr was to use years later to introduce the correspondence principle. This has let many historians see a meaningful continuity in the Danish physicist's methodological attitude. In their view, a sketch of that principle—enunciated for the first time in complete form in the Berlin conference—is already present in his early approaches to the quantum theory of the hydrogen atom. For example, in the classic paper of 1969, Heilbron and Kuhn argue, *inter alia*, that already the great article of 1913 holds "a first, but already powerful, formulation of the Correspondence Principle."[12] Other authors, such as J. Rud Nielsen, Olivier Darrigol, and Helge Kragh,[13] although with different accents, place the origin of that principle in the special relationship that exists in the limiting region between the frequency of the radiation emitted in the transition between states n_1 and n_2 and the harmonic component $(n_1 - n_2)\omega$ of the electron's motion, and that Bohr had commented in the following terms: "this is just what we would expect from analogy with the ordinary theories of radiation, since the motion of any periodic system which is not a harmonic vibrator can be resolved in harmonic terms corresponding with frequencies which are entire multiples of the frequency of revolution ω."[14]

In order to test the validity of this interpretative hypothesis, we should ask ourselves if we are really facing the natural evolution of the same pattern of reasoning aimed at establishing from the outset some correlation (said correspondence) between clearly

[9] According to the two expert spectroscopists, these lines had to be attributed to hydrogen and should correspond, according to Bohr's theoretical formula, to semi-integer quantum numbers. On the contrary, Bohr considered these lines to be emitted by ionized helium atoms whose frequencies were given by an expression similar to that of hydrogen but with a value four times greater than the Rydberg constant. The two interpretations were therefore subjected by Evan Evans to a sort of *experimentum crucis* that required the repetition of Fowler's measurements in a vacuum tube filled solely with helium and with ascertained absence of any traces of hydrogen; Evans (1913).

[10] See infra Sect. 2.1.

[11] This is what we read in a letter sent by G. von Hevesy to Bohr on October 23, 1913. Hevesy had met Einstein in Vienna on the occasion of the 85th Versammlung deutschen und Naturforscher Aertze and wrote to Bohr of his favorable impression, CW2 532.

[12] Heilbron e Kuhn (1969, p. 272).

[13] Rud Nielsen (1976, § 2), Darrigol (1997, p. 549), Kragh (2012, p. 63).

[14] Bohr (1916, unpublished paper), cit. CW2 445.

disjointed phenomenal and conceptual frameworks. In particular, Bohr's writings are expected to show the beginnings of a substantial continuity between the first and uncertain attempts to establish an analogy between quantum events and the ordinary laws of electrodynamics, and the correspondence principle. We will see that it is not so, although in Bohr's early works there are certainly cues intended to be included within this principle.[15] It can be shown that nothing similar to the correspondence principle—the singular logical relationship that allows us "to consider the possibility of the occurrence of a transition between two given stationary state as conditioned by the appearance in the motion of the corresponding harmonic vibration"—appears in Bohr's writings before 1918.[16]

In early 1916, the *Philosophical Magazine* had received an article by Bohr entitled "On the application of quantum theory to periodic system."[17] The manuscript had been sent by the University of Manchester, to which Bohr had returned two years after his first and successful meeting with Rutherford.[18] On March 17, Bohr had the drafts of his article ready but, as he stated in a letter to his friend Carl Oseen, some novelties in the field gave him cause to withdraw its publication.[19] Such a drastic decision arose from the publication of a "new and exceedingly interesting and important paper on the structure of spectral line" signed by Arnold Sommerfeld, an article which, according to Bohr, "has quite changed the present state of the quantum theory."[20] Equally meaningful and explicit were the words contained in the letter he sent two days later to Sommerfeld, informing him that he would have to carefully review his own theoretical positions "in view of all for which your paper has opened my eyes."[21] Sommerfeld's work had helped shed new light on the problems of quantum theory with the introduction, among other things, of a second quantum number to characterize the eccentricity of the elliptical orbit; it is quite likely that Bohr had already developed during those early months of 1916 an independent belief that made his mind naturally ready and enthusiastic to these new ideas. In the same letter to Oseen, commenting the article he had sent to the *Philosophical Magazine*, Bohr had in fact acknowledged the limitations of his attempts to bring within a logically coherent framework the assumptions introduced to explain the atomic processes dependent on a discontinuous distribution of stationary states. The resulting conclusion did not leave much room for doubt: "I do not think that a theory of the Planck type can be made logical consistent."[22]

[15] Cfr. infra Sect. 2.3.

[16] Bohr develops the argument that will be taken up within the correspondence principle in Bohr (1918a, pp. 15–16). In this regard, see J. Rud Nielsen "Introduction" to CW3, in particular § 3.

[17] Bohr (1916, unpublished paper), cit.

[18] Bohr thanked Rutherford for the invitation received in a letter dated June 19, 1914, CW2 594–595. He had offered a one-year readership, and Bohr had reached Manchester in October 1914, a few weeks after the beginning of the war operations and the closure of the German borders. Bohr stayed on in Manchester until July 1916.

[19] Bohr to Oseen March 17, 1916, CW2 571–573.

[20] Ibid., p. 572.

[21] Bohr to Sommerfeld March 19, 1916, CW2 603–604: 603.

[22] Bohr to Oseen March 17, 1916, cit.

There are therefore further clues that reinforce my original interpretative hypothesis allowing me to clarify the questions which I will attempt to answer in this article.

(a) Is it possible to find the connection between the assessment that took Bohr to abandon the search for a Planck-type theory and the formulation of the correspondence principle? In other words, is there any evidence that the birth of this principle coincided with the depletion of a research program whose origins may date back to the memorandum Bohr gave Rutherford when he left Manchester in the summer of 1912?
(b) Was that original program of research therefore abandoned when it became clear that Planck's quantum hypothesis for the harmonic oscillator was not adequate to support the theoretical architecture of atomic physics? And is it fair to say that the moment of transition was marked by the "withdrawn paper?"

First of all, it will be necessary to clarify in what sense Bohr intended to build a quantum theory of the atom along the lines of Planck's theory of thermal radiation. We will see that from 1913 to 1916, Bohr was to adopt different strategies—in some cases marked by clear forcing—in order to establish, in the light of the Planck's ideas, a compatibility between the mechanical model of the atom and the unavoidable element of discontinuity involved in the processes of interaction between matter and radiation. Only in this way could we, on the one hand, grasp the meaning of the statement about the impossibility of building a Planck-type theory of the atom and, on the other, give a foundation to that assessment with which Bohr would later acknowledge the correspondence principle as the source of the program that was to lead to quantum mechanics.

2 First attempts of a Planck-type theory of the atom

2.1 "... it was not taken seriously at all"

"In the investigation of the configuration of the electrons in the atoms, we immediately meet with the difficulty that a ring [occurring in the natural atoms] can rotate with an infinitely great number of different times of rotation, according to the assumed different radii of the ring; and there seems to be nothing to allow from mechanical considerations to discriminate between the different radii and times of vibration. In the further investigation, we shall therefore introduce and make use of a hypothesis from which we can determinate the quantities in question. The hypothesis is: that there, for any stable ring (any ring), will be a definite ratio between the kinetic energy of any electron in the ring and the [frequency] of rotation. This hypothesis, for which there will be given no attempt of a mechanical foundation, is chosen as the only one which seems to offer a possibility of an explanation of the whole group of experimental results which gather about and seem to confirm conceptions of the mechanism of the radiation as the ones proposed by Planck and Einstein."[23]

This passage is taken from the famous memorandum that Bohr wrote for Rutherford at the end of his time in Manchester as a visiting scholar, in which he outlined the

[23] N. Bohr, The Rutherford memorandum (1912) in CW2 136–143: 137.

research program he was to develop after his return to Copenhagen.[24] Although the problem of atomic spectra did not fall in Bohr's theoretical horizon yet, these few lines sketch some elements that were to be the basis for his future quantum theory of the atom. Firstly, a consideration on the limits of classical mechanics in the study of the configuration of electrons within the atom; its laws do not contain useful criteria for discriminating from the infinity of theoretically possible movements those that are allowed physically. Secondly, the idea that this criterion can be derived from the non-mechanical hypothesis that assigns a constant value to the ratio between the kinetic energy of each electron inside a ring and the frequency of its movement. Certainly, this was no quantization of atomic orbits, though according to Bohr it is quite clear—this is the third point—that his hypothesis is legitimized by the fact that similar ideas have allowed us to explain some important experimental results; and here, Bohr cites the works of Planck and Einstein.

In the following years, these assumptions were to suffer several transformations, while Rutherford's nuclear model was to become one of the irreplaceable cornerstones of Bohr's theory. We do not intend to return here to the reasons at the basis of this propensity, moreover associated with a total lack of confidence in the J.J. Thomson model.[25] Of course, Rutherford's ideas had the support of some experimental data such as: (a) the role assigned to the atomic number in representing the complexity of the electronic system and the position of the elements in the periodic table; (b) the existence of elements with different atomic weights and radioactive properties but with apparently identical chemical characteristics; (c) the regular displacement of the position of an element in the periodic table as a consequence of a radioactive process accompanied by the emission of an α or β particle. At first glance, these results should have but confirmed the unequivocal success of the nuclear hypothesis formulated by Rutherford, and the inexorable decline of the 'plum pudding model' that had fed the expectations of J.J. Thomson and the researches of the Cavendish laboratory for years.

Apparently, however, matters stood differently, as Bohr reminds us in one of his last writings. At the time, the situation presented very controversial aspects, although "the primary object of the discussions within the Manchester group [year 1912] was the immediate consequences of the discovery of the atomic nucleus."[26] Indeed, no one could ignore the enormous difficulties that one encountered when trying to develop "the general program of interpreting the accumulated experience about the ordinary physical and chemical properties of matter on the basis of the Rutherford model of the atom."[27] This gave rise to "more intricate problems" which quite often brought even those who were sympathetic to Rutherford to "not recognize the close relation [of

[24] The manuscript is not dated, but it is reasonable to place its drafting in late June/early July 1912. Bohr sent the manuscript to Rutherford with a letter dated July 6, 1912, which states "I send the remarks concerning the structure and stability of molecules, for which you kindly asked" CW2 577. On the discovery, reconstruction and interpretation of this manuscript cf. Rosenfeld (1963) and Heilbron and Kuhn (1969).

[25] From a long letter written by Bohr to his brother Harald October 23, 1911, it can be deduced, inter alia, that Thomson had displayed a total lack of interest for Bohr's criticism concerning some mistakes that he apparently committed in the calculation of the absorption, Bohr (1972) [henceforth CW1], original and English translation pp. 526–533.

[26] Bohr (1961, p. 1086).

[27] Ibid.

some results] to the fundamental features of Rutherford's atomic model":[28] such was, for example, Soddy's attitude when he enunciated the radioactive displacement law in its complete form.[29] Beyond the personal recollections of the Danish physicist, there is, however, objective evidence to support the view that Rutherford's "discovery" was long ignored or underestimated and that, contrary to what we would be led to believe, there was no mass adhesion to his revolutionary image of the atom.

First of all, there is a somewhat surprising episode, that occurred during the second Solvay Conference, held in Brussels in the autumn of 1913, on the theme of "la structure de la matière." Two years had elapsed since the publication of Rutherford's article on the *Philosophical Magazine* and, as I said, the nuclear model had had a significant number of empirical evidence; during the year, the same journal had published a long article in which a young Danish physicist showed the potentialities of the hypothesis of the nuclear atom as a basis for the interpretation of some spectroscopic laws; finally, Rutherford was part of the Comité scientifique international de l'Institut Solvay;[30] nevertheless, the task of holding the opening conference had been given to the "losing" J.J. Thomson, who obviously chose to entitle his speech "La structure de l'atome."[31] Thomson made no mention of the nuclear model, and he proposed a new and shaky hypothesis about the structure of the atom.[32] Rutherford had the comfort of a single, albeit authoritative, quotation by Marie Curie. Twice in the discussion he explained that some experimental results were interpretable only in the light of his hypothesis.[33] Evidently, Thomson had thought the arguments of his colleague quite uninteresting and therefore not worthy of a reply.

Beyond Thomson's unkind behavior, the episode shows how much uncertainty there was around the problem of the constitution of matter. In spite of what we might be led to believe, in the absence of a coherent theory and of a clear and empirical evidence, it was really difficult to sustain one or the other competing hypotheses. On the other hand, the fact that, beyond the small Manchester group, Rutherford's point of view gathered sporadic adhesion is confirmed by the issues of two major English journals of physics, the *Philosophical Magazine* and *Proceedings of the Royal Society* between 1911 and 1914.

As one might expect, a large part of the articles came from the two primary English schools which at the time belonged, respectively, to the Cavendish laboratory and the University of Manchester. Well, out of a total of more than 600 articles, while

[28] Ibid., p. 1085.

[29] Cf. Kragh (2012, p. 47). Bohr himself was to recall that while he considered the atomic number and the radioactive displacement laws as results that supported the Rutherford theory, Hevesy, Fajans and Soddy thought precisely the opposite and "they thought that [this] was completely against Rutherford," Interview to Niels Bohr by Thomas S. Kuhn, Léon Rosenfeld, Aage Petersen and Erik Rüdinger on October 31, 1962, AIP.

[30] The Comité scientifique international chaired by Lorentz was composed of M. Curie, W.H. Bragg, M. Brillouin, H. Kamerlingh Onnes, M. Knudsen, A. Righi, E. Rutherford and E. van Aubel.

[31] J.J. Thomson, La structure de l'atome, in Solvay II, pp. 1–44. In this lecture, Thomson presented a new model of the atom (cf. Kragh 2012, pp. 108–110).

[32] Reserves and doubts were expressed by Bohr in a letter to Rutherford October 16, 1913, CW2 587–589.

[33] Solvay II, pp. 50–51 and 53–54.

Rutherford's works between 1897 and 1914 are mentioned 93 times in 48 articles,[34] his famous 1911 paper, "The Scattering of α or β particles by matter and the structure of the atom" collected only eight citations.[35] But above all, it is striking that, with the exception of Charles T.R. Wilson,[36] these involve only researchers of the Manchester group.[37] We do not expect the situation to have been any better in other scientific circles as, for instance, the German one, dominated by skepticism toward the atomic models and where, according to Rutherford, "the people do not seem to be in the least interested to form a physical idea of the basis of Planck's theory."[38] However, it is surprising to discover that in about 7,500 pages published by the *Annalen der Physik* between 1912 and 1913, Rutherford's paper on the nuclear atom is quoted only once by a certain Fritz Mayer in a review article entitled "Zerstreuung der α-Strahlen."[39] During a long interview in the fall of 1962, Thomas Kuhn had tried to bring out from Bohr's memory episodes of those early days spent in Manchester to find out what would have "convinced of the likely validity of Rutherford's atom from first hearing."[40] Kuhn's interview is a document holding few surprises, and most of our curiosities are destined to remain as such. Opposite Kuhn sat an old man, marked by illness.[41] Bohr's recollections reveal no helpful clues to guide us in the reconstruction of the route that over those crucial months—between June 1912 and March 1913—led him to seek in quantum ideas the necessary tool to reveal the secret of atoms. One is, however, impressed when hearing Bohr say: "You see actually the Rutherford work was not taken seriously. We cannot understand today, but it was not taken seriously at all. There was no mention of it in any place. The great change came from Moseley. But before Moseley's work there was absolutely nothing about the Rutherford thing."[42]

[34] 76 references in 41 articles apart from those written by the same Rutherford, also in collaboration with other authors.

[35] Rutherford (1911). The quotations would be ten if one includes those contained in the articles of Rutherford himself: Rutherford (1912, p. 461) and Rutherford and Nuttal (1913, p. 702).

[36] Wilson (1912, p. 284). Wilson, a pupil of Thomson, was long his collaborator at the Cavendish Laboratory.

[37] Darwin (1912, p. 901, 1913, p. 201), Geiger (1912, p. 605), Geiger and Marsden (1913, p. 605), Marsden and Taylor (1913, p. 443), Moseley (1913, p. 1025), Bohr (1913a, p. 10, 1913b, p. 1.)

[38] Rutherford to Bragg, in Eve (1939, p. 208).

[39] Mayer (1913, p. 940).

[40] Interview to Niels Bohr by Thomas S. Kuhn, cit.

[41] Thomas Kuhn's interview to Niels Bohr, planned by the project for the Archive for the History of Quantum Physics, took place in the fall of 1962. Over 18 days, from October 31, five sessions were held almost entirely devoted to young Bohr's early years of scientific activity, the preparation of his doctoral thesis, his stay in England, first at Cambridge then in Manchester, as well as the publication in 1913 of his famous theory of the constitution of atoms and molecules. The interview was abruptly interrupted by a tragic event. The fifth session was held on November 17, and had concerned young Bohr's cultural education, his philosophical interests and his relationship with Harald Høffding of whom he had been a pupil. Early in the afternoon of the following day, Bohr died from a heart attack.

[42] The article to which Bohr refers is Moseley (1913), cf Kragh (2012, § 3.4). In this regard, it is also significant to point out what Peter Debye replied to Kuhn who had asked "whether, when you had been in Zurich when the Rutherford atom was suggested, I wondered whether you had heard of it." According to Debye "in Zurich we did not talk very much about models. [...] The model of Rutherford was compared to that of Thomson." Interview to Peter Debye by Thomas S. Kuhn and G. Uhlenbeck on May 3, 1962, AIP.

Here, Bohr's memory does nothing but confirm a truth preserved in the pages of scientific journals; moreover, it forces us to acknowledge that, in a not at all favorable scientific environment, Bohr had adhered to the Rutherford ideas because he felt that in this way "everything was now aligned."[43]

These words do not come from a judgement acquired a posteriori in light of the complex events of which he was one of the main protagonists; otherwise, one would not understand the reasons that led him to hypothesize, already in the mentioned memorandum, an original, and bold, "quantum" solution able to ensure that model a stability not reducible to the ordinary ideas of mechanics. Although at the time of the interview this important document had not as yet been discovered among the thousands of manuscripts of Bohr's archive,[44] he had no difficulty recognizing that when he was in Manchester "I only knew that we had this atom and that it seemed to be regulated from the inner part to the outer part by the quantum."[45] Nothing more than that. There was not even a hypothetical application of Planck's ideas to the periodic motion of the electrons. The "quantum" was a hypothesis in embryonic state, but at the same time it was so rooted in Bohr's thought as to convince him to admit that it somehow regulated Rutherford's atom and was responsible for its stability. It was really an extraordinary intuition, because Bohr categorically ruled out that the knowledge of the regularity of the spectra or of Balmer's formula already represented an element of the puzzle that he was to build a few months down the line with his sending Rutherford the first part of the "trilogy."

Of course, Bohr could not lack in his cultural background the knowledge of research in the field of spectroscopy. Anyone who had browsed the issue of a journal of physics or astronomy would have come across the contributions of spectroscopists intent on recording data of their careful measurements and in finding meaningful regularities in an impressive and chaotic collection of numbers. So in order to explain what that situation was like, Bohr used an extremely clever and clear analogy: "The spectra was a very difficult problem. There were two different schools—those in England, and then there was the school of the spectroscopists. And one thought that this is marvelous, but it is not possible to make progress there. Just as if you have the wing of a butterfly then certainly it is very regular with the colors and so on, but nobody thought that one could get the basis of biology from the coloring of the win[g] of a butterfly. So that was a way to look at it."[46] The colorful and regular lines of the spectra observed in the stars and in the laboratory were thus like the wings of butterflies, and it was hard to imagine that such objects could contain cues relevant to the construction of a physics of matter.

2.2 The "trilogy": a work in progress

On March 6, 1913, Bohr sent Rutherford the first chapter of the article on the constitution of atoms in which he developed the program drawn up during his last weeks in

[43] Interview to Niels Bohr by Thomas S. Kuhn ... on October 31, 1962, cit. p. 7.

[44] A first critical analysis of this document can be found in Rosenfeld (1963, "Introduction"). See also Heilbron and Kuhn (1969).

[45] Interview to Niels Bohr by Thomas S. Kuhn ... on October 31, 1962, cit. p. 10.

[46] Ibid., p. 8.

Manchester. In the enclosed letter, he expressed the hope that Rutherford would consider reasonable the point of view "that I have taken [...] as to the delicate question of the simultaneous use of the old mechanics and of the new assumptions introduced by Planck's theory of radiation."[47] The time elapsed was relatively short, and part of this had been dedicated to the preparation of the work on the absorption of α-particles.[48] However, his point of view on the whole matter had changed since the solutions he had outlined in the Memorandum; the quantization method was quite different, and above all a problem that had been ignored in the previous paper was now included, forcing him to redefine and extend the theory's very goals: "I have tried to show that it from such a point of view seems possible to give a simple interpretation of the law of the spectrum of hydrogen. And that the calculation affords a close quantitative agreement with experiments."[49] Rutherford's reaction was one of admiration, even if he could not hide doubts and some skepticism: "Your ideas [...] are very ingenious and seem to work out well; but the mixture of Planck's ideas with the old mechanics make it very difficult to form a physical idea of what is the basis of it."[50] Nevertheless, the young physicist's theoretical proposal seemed worthy of the most careful consideration, as it had obtained interesting first results in the explanation of an important class of phenomena, the atomic spectra, lacking for several decades in any interpretative basis. The three parts of Bohr's article were published in volume 26 of the *Philosophical Magazine* and appeared in the issues of July, September and November of 1913.

In reading this article today, we realize that only a combination of "favorable" circumstances have enabled us to receive a unique historical document. The text, written with no respect for canonical criteria, is presented as a work in progress that allows one to follow the evolution of Bohr's thinking closely; here are recorded, in particular, the reasons that led him to suggest from time to time a different use of Planck's quantum hypothesis. Such an editorial "singularity" was certainly favored by Rutherford's kindness toward the pressing demands of a young colleague so that the publication of his paper would take place "as soon as possible, on account of the accumulating literature on the subject."[51] But an equally important part should be attributed to the absence, in the magazines of the period, of any serious scientific filter; evidently, the authority of the scholar who submitted the article was the highest guarantee requested for its publication. In fact, it is legitimate to ask what credit could ever be given today to an author who, having developed a theoretical treatment and shown that some of his predictions are supported by experience, had come to

47 Bohr to Rutherford, March 6, 1913, CW2 581–583: 582.

48 Bohr (1913a).

49 Bohr to Rutherford, March 6, 1913, cit.

50 Rutherford to Bohr, March 20, 1913, CW2 583–584: 583.

51 Bohr to Rutherford, March 6, 1913, cit. 581. Bohr had taken seriously the work that the astronomer John Nicholson had dedicated to the interpretation of some spectra also appropriately exploiting Planck's quantum ideas, Nicholson (1912a, b). And though Bohr was engaged to highlight the relevant differences between his approach and that of Nicholson (see, for example, Bohr to Rutherford 31 January 1913 CW2 579–580), there were valid reasons for him to wish for a fairly rapid publication of his article. Heilbron, for example, speaks of the "alarming paper of John William Nicholson" in which Bohr found in the last weeks of 1912, while he was engaged in defining the conceptual framework of his theory, Heilbron (2013, p. 170).

declare within that same work, that one of the assumptions used "may be regarded as improbable."[52] And Bohr's hypothesis was not marginal; it called directly into question the basic objective of the article consisting "in an attempt to show that the application of the above [quantum] ideas to Rutherford's atom-model affords a basis for a theory on the constitution of atoms" and in the case of hydrogen in explaining "in a simple way" the law of the spectral lines.[53]

At the beginning of the third paragraph, significantly entitled "General Consideration continued," Bohr definitely belied himself by stating that it is unlikely to think that "the different stationary states correspond to an emission of a number of different energy-quanta,"[54] as required by a direct application of Planck's quantum formula to the atomic system. Hence, it only had to be acknowledged that the mechanism acting within the atom responsible for the emission of energy was still unknown, but certainly more complex than the one assumed in the case of heat radiation.

In the first part of the "trilogy," we can therefore see two consistently developed versions of the theory, each one following a different logical approach.[55] The first of these versions can be summarized in the following points.

A1. A system consisting of a nucleus and an orbiting electron is taken into consideration, and the following approximations are introduced: (a) the mass of the electron is negligible compared with that of the nucleus, and (b) its velocity is much smaller than that of light, so that is possible to neglect relativistic effects.

A2. Assuming, contrary to ordinary physical ideas, that there is no loss of radiant energy, electron orbits are elliptical and classical mechanics allows us to determine, for an inverse-square system, the expression of frequency ω and the major axis $2a$ as a function of the ionization energy W.

A3. It is assumed that at the beginning of the process, the electron is at infinite distance from the nucleus with an almost zero relative velocity and, furthermore, that "after the interaction has taken place [the electron] has settled down in a stationary orbit round the nucleus."[56]

A4. Finally, the hypothesis that the process of binding the electron is accompanied by the emission of a homogeneous radiation with frequency ν "equal to half the frequency [ω] of revolution of the electron in its final orbit"[57] is introduced.

A5. If one extends the validity of Planck's hypothesis to this context—according to which "the amount of energy emitted by the process considered is equal to $h\tau\nu$, where h is Planck's constant and τ an entire number"[58]—from A4 the "quantum" formula of the energy of the system follows:

$$W = \tau h \frac{\omega}{2}. \tag{2.1}$$

52 Bohr (1913b, p. 12).

53 Ibid., pp. 2–3.

54 Ibid., p. 12.

55 In a recent paper, Heilbron identifies four formulations of the theory, Heilbron (2013, pp. 175–177).

56 Bohr (1913b, p. 4).

57 Ibid., p. 4–5.

58 Ibid., p. 5.

At this stage, Bohr raises the question of the validity of the assumptions he introduced, "and also of the application made of Planck's theory," however, deferring the discussion.

A6. This leads to a first result apparently indicating, inter alia, the validity of the whole theoretical framework: by using the experimental values of e, m and h for the more bound state of the electron in the hydrogen atom, the values of ω, $2a$ and W are obtained, that "are of the same order of magnitude as the linear dimensions of the atoms, the optical frequencies and the ionization potentials."[59]

A7. Finally, the theory is able to derive Balmer's law for the hydrogen spectrum. By determining the amount of energy emitted by the system when it passes from state τ_1 to τ_2 according to (2.1), and assuming that in this process a homogeneous radiation of frequency ν is released with an amount of energy equal to $h\nu$, we obtain

$$\nu = \frac{2\pi m e^4}{h^3}\left(\frac{1}{\tau_2^2} - \frac{1}{\tau_1^2}\right). \tag{2.2}$$

When varying the quantum numbers τ_1 and τ_2, we get the ordinary Balmer series and the infrared series observed by Paschen. The agreement between this and the empirical formula is particularly good, as evidenced by the fact that for the so-called Rydberg constant there is a 5 % deviation between the theoretical $[2\pi m e^4/h^3]$ and the experimental value, therefore "inside the uncertainty due to experimental errors in the constants entering in the expression for the theoretical value."[60]

Having obtained a result that was supposed to represent a valid support to his theoretical approach, Bohr went back to the examination of the assumptions used which, however, comes down to a simple and also disappointing acknowledgment. For systems in which frequency is a function of energy, the use of Planck's hypothesis leads to conclusions incompatible with the observed homogeneity of the spectral radiation. Such an obvious result clearly shows the irrationality of Bohr's choice in following a path destined to end in paradoxical results. To this end, it is sufficient to reflect on the fact that (a) when the quantum condition for the harmonic oscillator is applied to the hydrogen atom, it translates into the hypothesis according to which to each state there should correspond the emission of a different number of quanta of energy and that (b) "as soon as one quantum is sent out the frequency [of the electron] is altered."[61] So the very idea of the stability of stationary states no longer holds and an unbridgeable rift is caused between the properties of the spectra and the processes involving particles inside the atom. But such a bizarre behavior admits but one explanation: Bohr realized that the untenability of A4 and A5 would have entailed the inevitable abandonment of A7, one of his main objectives, that is to say, the theoretical derivation of the Balmer's law for the hydrogen spectrum.

59 Ibidem.

60 Ibid., p. 9.

61 Ibid., p. 12.

The acknowledged impossibility of a direct application of Planck's ideas opened the way to a new version of the theory that, by Bohr's own admission, shares with the previous one two principal assumptions: "(1) The dynamical equilibrium of the systems in the stationary states can be discussed by help of ordinary mechanics, while the passing of the systems between different stationary states cannot be treated on that basis. (2) The latter process is followed by the emission of a *homogeneous* radiation, for which the relation between the frequency and the amount of energy emitted is the one given by Planck's theory."[62] Although the reference to Planck remains an essential requirement of the theory, in the new version Bohr shifts the heart of his reasoning. On the one hand, it still appears reasonable to assume that only a quantum approach to the processes of interaction between atoms and radiation can provide an expression for the energy released by the system as a function of the frequency of radiation. On the other hand, however, as mentioned above, major conceptual obstacles arise when trying to transfer the assumptions used for the energy of the harmonic oscillator—where ω is always equal to ν—to a system composed of orbiting electrons and radiation processes depending on discontinuous transitions between stationary states. Everything depends on the fact that, in this second case, there are no indications corroborating the existence of a simple relation between ω and ν. A fact Bohr was well aware of, having observed that the assumptions used lead to the paradox, in terms of ordinary ideas, according to which the homogeneous nature of the observed radiation is not compatible with the mechanical properties of the system on which each radiative process depends.

Even if the solution of the problem was no longer at hand, Bohr was nevertheless engaged in searching for a Planck-type theory. Therefore, the obstacles' only effect was that of forcing him to weaken the objectives of the theory and to use a simple formal analogy with the hypothesis of quantization of energy of the harmonic oscillator. In these terms, Bohr replaced A5 with a new hypothesis where the idea that there must be some relationship between the energy of the radiation emitted by the system and the frequency of motion of the electron was nevertheless maintained.

B5. "Let us assume that the ratio between the total amount of energy emitted and the frequency of revolution of the electron for the different stationary states is given by the equation $W = f(\tau) \cdot h\omega$, instead of by the equation $[W = \tau\, h(\omega/2)]$."[63]

B6. Proceeding as in the previous case, we arrive at a new expression for W and ω and for the frequency of radiation

$$\nu = \frac{2\pi m e^4}{h^3}\left(\frac{1}{f^2(\tau_2)} - \frac{1}{f^2(\tau_1)}\right). \tag{2.3}$$

B7. In order for this to be formally analogous to Balmer's law, it must be imposed for $f(\tau) = c\tau$.

B8. Then, the expression of frequency ν of the radiation relative to the passage of the system between two contiguous stationary states is obtained, with $\tau_1 = N$ and $\tau_2 = N - 1$, as well as that for the corresponding frequencies of revolution ω. For

62 Ibid., p. 7.

63 Ibid., pp. 12–13.

large values of N, the ratio between ω_N and ω_{N-1} will obviously be close to 1, and always in the same limiting region "according to the ordinary electrodynamics, we should [...] expect that the ratio between the frequency of radiation [ν] and the frequency of revolution [ω] also is very nearly equal to 1."[64] This requires that the constant c is equal to 1/2.

Therefore we return, albeit through a different conceptual path, to the expression for the energy of the stationary state

$$W = \tau h \left(\frac{\omega}{2}\right) \tag{2.4}$$

which had been previously imposed by Bohr onto the atomic system as a direct application of Planck's theory. In this case, however, the contradiction that had required the overcoming of the previous version of the theory seems to dissolve. It can be shown that (2.4) admits an interpretation totally independent from the idea, clearly devoid of any physical foundation, which assigns to each stationary state of an atomic system a different number of quanta of energy.

To this end, Bohr takes into account transition processes involving non-contiguous states but for which the corresponding quantum numbers are large compared with their difference. It is easy to show that even under these particular conditions, the approximation that allows one to assign the value $\tau/2$ to the function f still holds. Of course, for transitions between non-contiguous states, one will have $\nu = n\omega$, where n is the difference between the quantum numbers of the states involved in the process; a result that Bohr comments thus: "The possibility of an emission of a radiation of such a frequency may also be interpreted from analogy with the ordinary electrodynamics, as an electron rotating round a nucleus in an elliptical orbit will emit a radiation which according to Fourier's theorem can be resolved into homogeneous components, the frequencies of which are $n\omega$, if ω is the frequency of revolution of the electron."[65] The effectiveness of the analogy with the classical point of view thus reveals the real meaning of the quantum condition for the energy of the individual atomic states: "The frequency of the energy emitted during the passing of the system from a state in which no energy is yet radiated out to one of the different stationary states, is equal to different multiples of $\omega/2$, where ω is the frequency of revolution of the electron in the state considered."[66]

At first glance, this might not seem a particularly significant step forward since the frequency of radiation is still determined by the characteristics of the motion of the electron in the state from which the radiative process originates, and in any case not free from the conceptual strangeness which had aroused the immediate ironic comment of Rutherford: "It seems to me that you have to assume that the electron knows beforehand where it is going to stop."[67] But at least, in this case, the principle

[64] Ibid., p. 13.

[65] Ibid., p. 14. With reference to what I said in the introduction, this is the passage in which both Darrigol (1997, p. 549) and Kragh (2012, p 63) place the origins of the correspondence principle.

[66] Ibidem.

[67] CW2 583.

according to which the homogeneous radiation is emitted with a single quantum of energy was still applied. If the system at the beginning of the process is in a state N, with frequency ω of electron motion, whether the final state of the transition be $N' = N - n'$ or $N'' = N - n''$ a single quantum of energy with frequency, respectively, equal to $n'\omega/2$ and $n''\omega/2$ will be issued. In other words, (2.4) can be read as

$$W = h\left(\frac{\tau\omega}{2}\right), \tag{2.5}$$

where the frequency ν of the monochromatic radiation emitted is equal for each state to a half-integer multiple of the frequency ω of the electron in the initial state of the transition process. Bohr was forced to resort to a new and unorthodox application of Planck's original hypothesis, and he was convinced that he had succeeded in saving the theory since "we get exactly the same expressions as before for the stationary states, and from these by help of the principal assumptions [...] the same expression for the law of the hydrogen spectrum."[68]

But the discussion did not end here and, as if to soften the evident dissatisfaction with the results just obtained, Bohr proposed a further analysis. Thus, Bohr's article ever more like a "daybook," contains a further outline, a scheme of barely sketched reasoning that represents in short the third version of the theory. Further on we shall analyze it in detail, but already in the few lines of this "private memo" the nature and the extent of the changes he had in mind can be envisaged, and certainly they were not marginal changes.

It is not surprising for him to mean to return to "the agreement between the observed and calculated values of the constant entering in the expressions (2.2) for the Balmer series of the hydrogen spectrum";[69] after all, this was one of the most interesting results of the theory. However, by his words, one does not understand which aspects he still considered problematic, and hence worthy of further investigation. What is quite clear is Bohr's intention to overturn the previous interpretative schemes in order to adopt a whole new approach, one from which it is possible to derive the value of R without that the need for the theory to concentrate on the existence of a more or less favorable application of Planck's theory.

The new strategy abandons the idea of using such Planckian approach to obtain information on the expression for the energy of the nth state of the atomic system. The basis of this reasoning is completely different and concerns, on the one hand, the empirical law of the hydrogen spectrum and on the other, the hypothesis that single lines should correspond to a homogeneous radiation emitted during a transition between stationary states. Although any discussion of a formal nature was absent from the memo, Bohr was sure that in this way "we shall arrive at exactly the same expression for the constant in question as that given by (2.2), if we only assume (1) that the radiation is sent out in quanta $h\nu$ and (2) that the frequency of the radiation emitted

[68] Bohr (1913b, pp. 14).

[69] Ibidem.

during the passing of the system between successive stationary states will coincide with the frequency of revolution of the electron in the region of slow vibrations."[70]

Basically, the new theoretical scheme required that the quantum hypothesis should be freed from any modeling contamination in order to go back to the key idea according to which the radiation is always emitted as a quantum $h\nu$. The fact that a relationship actually able to express the dependence of the radiation from the dynamic state of the electrons could be found was not considered by Bohr a particularly relevant problem. It concerned, in fact, a hypothetical and indefinable mechanism responsible for the radiative processes, the knowledge of which, however, would not have been of any help in the derivation of the Rydberg constant.

At least two aspects deserve to be highlighted in Bohr's brief note. Firstly, the absence of any reference to the important 1910 article by Peter Debye on the derivation of the Planckian formula for thermal radiation. Secondly, his abandoning of the point of view of Planck as a necessary step for the construction of the atomic theory. The first debt owed to Debye was to be paid shortly after. The second question was to result in numerous changes of mind by Bohr, finding a natural and definitive placement only within the context of the correspondence principle. Bohr was moving in a sort of no man's land, and rather than pay attention to the formal rigor and to the consistency of his reasoning, he showed more interest in proposing alternative solutions in the hope that in the long run they would generate fruitful developments. In particular, this third and final version of the theory became the basic outline for the conference that Bohr was to hold on December 20, 1913, at the Danish Society of Physics.

2.3 A misleading analogy

The conference concluded an extraordinary year. Only a few weeks had elapsed since the publication of the last part of the trilogy; Bohr's great article had certainly not gone unnoticed even if eliciting contradictory reactions. Some, like Max Born considered Bohr's hypothesis to be too "bold" and "fantastic" and, like many physicists in Göttingen, was still waiting for more convincing experimental confirmation.[71] Others, such as Einstein and Sommerfeld, expressed their appreciation for the process of quantization and confessed that they had previously had similar ideas. Sommerfeld especially, although "rather skeptical about atomic model in general," wrote to Bohr that "the problem of expressing the Rydberg-Ritz by Planck constant h has for long time been in my mind."[72] But the highest and probably unexpected acknowledgment was to be bestowed on the occasion of the annual meeting of the British Association for the Advancement of Science, which was held in Birmingham from September 11 to 17, 1913. A general discussion took place on September 12 on the issue of radiation,

[70] Ibidem.

[71] These words can be found in a letter written by Harald to Niels Bohr in the fall of 1913 while he was in Göttingen, CW1 567. The first reactions of Paul Ehrenfest, who would later become one of Bohr's strongest supporters, were very negative. In a letter of August 25, 1913, he wrote to Sommerfeld "Bohr's work on the quantum theory of the Balmer formula [...] has driven me to despair. If this is the way to reach the goal, I must give up doing physics," quoted in Klein (1970, p. 278).

[72] Sommerfeld to Bohr, September 4, 1913, CW2 123.

and the initial speech was given by James Jeans who also gave a brief account Bohr's work stating that "[he] has arrived at a most ingenious and suggestive, and I think we must add convincing, explanation of the laws of spectral series."[73]

Bohr took the conference as an opportunity for a critical reflection on the many hints he had littered along his monumental article. Right from the start, however, doubts and uncertainties seemed to have disappeared, his words revealing an unexpected clarity of objectives. On the one hand, Bohr admitted that in order to ensure a solid conceptual foundation to Rutherford's model, "there can be no question of a direct application of Planck's theory."[74] On the other hand, he ruled out the possibility for the atomic theory to take advantage of hypothetical formal analogies with the Planckian equation for the energy of the harmonic oscillator: "When we consider how differently [this] equation is employed here and in Planck's theory it appears to me misleading to use this analogy as a foundation [...]."[75] Indeed, with these words, Bohr demolished the assumptions of the first two versions of the theory and expressed the need for a firmer alternative.

In his speech, Bohr illustrated a point of view able to overcome tentative interpretative schemes which had proved incomplete and contradictory. As already mentioned, he took up again the short note with which he had ended the discussion on the theoretical derivation of the Balmer formula. The core of Bohr's reasoning, now explicit, concerned the general validity of the quantum of energy. With this concept, in his view, it was not the case of some particular hypothesis being questioned, but of having "definitely parted with the coherent group of ideas on which the latter theory is based"[76] so one had to be ready to accept even radical changes in the body of knowledge of classical physics. This point of view was by no means obvious, as evidenced by the state of uncertainty that weighed on theoretical research; on the other hand, there were no valid reasons to support the thesis that "all cases of disagreement between the theoretical conceptions hitherto employed and experiment will be removed by the use of Planck's assumption regarding the quantum of the energy momentarily present in an oscillating system."[77]

In these terms, the problem could have been solved in an inextricable vicious circle; the quantum represents a radical departure from ordinary physical ideas, but not all "non-classical" phenomena admit a Planck-type interpretation. Bohr could see, however, a way out in the already cited Debye article, in which the Dutch physicist had shown that, without any conjectures on the nature of the system that emits the radiation, and assuming only that the amount of energy emitted is always equal to $h\nu$, you get the correct expression of the law of thermal radiation.[78] There was therefore another quantum approach, different from the Planckian one, to the study of the radiative behavior of the atom and the Debye scheme presented clear advantages. In the absence of reliable clues about the nature of the mechanism at the origin of the

73 Jeans (1913, p. 379).

74 Bohr (1914a), Eng. trans. in Bohr (1922b, pp. 1–19: 10) [also in CW2 283–301]. Here and hereinafter, the citations are taken from the English edition.

75 Ibid., p. 14.

76 Ibid., p. 10.

77 Ibidem.

78 Debye (1910).

radiation process, no one was forced to weigh the theory with assumptions destined to be largely arbitrary and in any case devoid of valid empirical evidence. Planck's quantum hypothesis, reinterpreted *à la* Debye, in the case of the atom was but to suggest that each radiation process is accompanied by the emission of a quantum of energy $h\nu$. This assertion was reinforced by two important corollaries: (a) the spectroscopic laws are not able to provide any information on the values of the orbital frequencies of the electron; (b) if the spectrum of an element contains a line of frequency ν, the atom is able to emit a quantity of energy $h\nu$ such that

$$E_2 - E_1 = h\nu, \tag{2.6}$$

where E_2 and E_1 are, respectively, the energies of the system at the beginning and at the end of the radiative process. With these assumptions, Bohr was able to show that the new theoretical scheme was compatible with Balmer's formula and therefore allowed to derive the expression of the Rydberg constant.

From the comparison of the empirical law for the hydrogen spectrum and the quantum condition (2.6), he considered reasonable to assume that, by less than a constant, the energy of an nth stationary state was given by

$$E_n = -\frac{Rhc}{n^2}; \tag{2.7}$$

consequently, the frequency of electron motion that is in that state would be

$$\omega_n^2 = \frac{2}{\pi^2}\frac{R^3h^3c^3}{e^4mn^6}. \tag{2.8}$$

Of course, this "quantum" relation gives an account of the element of discontinuity characteristic of the nuclear model of the atom, while hiding no conceptual Planck-type contamination. In this regard, Bohr's words leave no room for interpretation. If the radiation is emitted in the form of a single quantum, the atomic system makes a transition between the states n_1 and n_2, to which are associated, via mechanical model, two different frequencies of motion ω_{n1} and ω_{n2}. For this reason, "we are not justified in expecting any simple relation between these frequencies of revolution of the electron and the frequency of the emitted radiation."[79] In other words, the impossibility of using Planck's hypothesis constructively in the new theoretical context is confirmed.

So Bohr could only go back to the limiting considerations already used in the first part of the "trilogy." According to (2.8), as n increases, ω decreases rapidly and the frequencies of two contiguous stationary states remain almost the same ($\omega_{n+1}/\omega_n \to 1$); moreover, under the same limiting conditions and for contiguous states, (2.6) and (2.7) provide the following expression for the frequency of the emitted radiation

$$\nu = \frac{2Rc}{n^3}. \tag{2.9}$$

[79] Ibidem.

Assuming, then, by analogy with the law on thermal radiation that in the region of low frequencies there is a convergence between quantum hypothesis and the laws of classical electrodynamics, and we can then put $\nu = \omega$, from (2.9) and (2.8) we get the expression of the Rydberg constant

$$R = \frac{2\pi^2 e^4 m}{ch^3}. \tag{2.10}$$

Really not much is new, in formal terms, compared to what had been said in the trilogy, but an extraordinary step forward on a theoretical level. Indeed, Bohr obtained that result through an extremely simple interpretative scheme, supported only by the concept of stationary state and the idea that each radiative event depends on a discontinuous transition of the atomic system, and above all, the theory was no longer required to meet the typical conditions of a quantum approach *à la* Planck. All this, however, had a particularly high price: The results achieved do not lend themselves to any generalization and must remain strictly confined in the limiting zone. Only thus, in fact, conditions exist to affirm the existence of a numeric type of convergence between the frequencies of electron motion and those of the radiation, but without compromising the discontinuous nature of the radiative processes. Any extrapolation of these results would be in any case arbitrary as "we cannot expect to obtain a corresponding explanation of the frequency values of the other stationary state."[80]

In 1922, Cambridge University Press collected in a small volume Bohr's three essays on the application of quantum theory to the problems of atomic structure. The author's choice fell upon writings that in his opinion effectively represented "the different stages in the development of this theory."[81] The first two were the conferences of 1913 and 1920 mentioned in the preceding pages, while the third, with nothing but a few marginal changes, was based on a Danish address, given before a joint meeting of the Physical and Chemical Societies of Copenhagen on October 18, 1921. Bohr's introduction is very short, but in addition to the customary editorial notes, it contains opinions of major relevance. First, Bohr emphasizes the existence of a very close correlation between the views stated during the early part of the Berlin conference and in his speech of December 1913. In both cases, "no use is made of the new formal conceptions established through the later development of the quantum theory";[82] what is common to both texts is therefore, according to Bohr, the absence of formal approaches related to the development of the first quantum theory—but on this we shall return later. Second, Bohr states very clearly that "the first germ" of the correspondence principle, introduced during the second part of the Berlin conference, is located right in the 1913 text. A few lines later, he explained what he meant by the expression "first germ." The seed that was to produce a deep theoretical revision and the beginning of a new and more fruitful research program a few years down the line, concerned no generic considerations regarding the limiting zone where there is a significant convergence between quantum conditions and classical laws. Nothing

[80] Ibidem.

[81] Bohr (1922b, p. v).

[82] Ibidem.

like that. According to Bohr, the correspondence principle in the embryonic stages was in "the deduction of the expression for the constant of the hydrogen spectrum in terms of Planck's constant and of the quantities [e, m and c] which in Rutherford's atomic model are necessary for the description of the hydrogen atom."[83] Only there and nowhere else, sketching a theory that, although not intended to have immediate developments, demonstrated the possibility of gathering positive results even outside of a rigidly Planckian conceptual perspective. At the end of an extraordinary year, he believed that this was the methodologically correct attitude.[84] But, very soon Bohr was to retrace his own steps.

2.4 Retracing his steps

1913 had other major surprises in store for young Bohr. While he was preparing the text for the Society of Physics lecture, Rutherford informed him of an important discovery by Johannes Stark, at the time professor of physics at the Technische Hochschule in Aachen. Electric fields cause a separation of the lines of hydrogen and helium with a very similar effect to that highlighted by Zeeman with magnetic fields.[85] In the letter, Bohr was requested to check whether his theory was able to account for these effects. Already on December 31, he announced to Rutherford that "I have succeeded in accounting, at least partly, for the experiment of Stark on the basis of my theory."[86] With words that reveal his obvious satisfaction, he claimed to have found a very simple and plausible explanation for the characteristic difference between both the Stark effect and the Zeeman effect, as well as for the doublets of lines that can be observed, for example, in the hydrogen spectrum. Such a rapid and successful conclusion of his work was possible because "the line of arguments used is exactly analogous to that used in my first paper."[87]

The article containing these results, "On the effect of electric and magnetic fields on spectral lines," was written in a few days and published in the March issue of the *Philosophical Magazine*.[88] Bohr's expectations were, however, to be soon disappointed, as we read in a letter of "clarifications" that he sent to the editors of the magazine, but

[83] Ibid., pp. v–vi. John Heilbron implicitly seems to resume this opinion of Bohr's when he says that the same derivation of the Rydberg constant "which is only sketched in the trilogy, would become the most powerful, as it contains the seed of the 'Correspondence Principle' by which Bohr would seek to tease out the rules of the microworld by a comparison with calculations made using classical theory" Heilbron (2013, p. 176).

[84] Bohr considered this text to be an important stepping stone since, as he recalled years later, he was to use it as the basis for a number of important seminars held in Monaco and Göttingen in the summer of 1914. "I went to Göttingen in the summer of '14 [...] and there I gave a talk about the spectra from the point of view, or at any rate something like the points of view that I had put a half a year before in this paper in the Physical Society." Interview to Niels Bohr by Thomas S. Kuhn ... on November 7, cit., pp. 1–2.

[85] Rutherford to Bohr, December 11, 1913, CW2 589. Stark's work, published by Preußische Akademie der Wissenschaften, was announced in a brief note appeared in *Nature*, Stark (1913).

[86] Bohr to Rutherford 31 December 1913, CW2 591.

[87] Ibidem.

[88] Bohr (1914b).

that was never published.[89] Despite its obvious limitations, Bohr's article is an important contribution to the understanding of the events we are dealing with. Obviously, I am not referring to his attempt to explain the Stark and Zeeman effects, on which he would come back later with more luck. I am thinking of the general considerations on the theory's foundations that prompted him, after a very short time, to review some of the conclusions contained in the end-of-year conference. In particular, here Bohr abandons any reservation about the heuristic validity of formal analogies with Planck's quantum hypothesis and especially does not exclude that we can generalize the results obtained in the limiting region. In fact, he intends to demonstrate that: (a) for any stationary state we have

$$\frac{\mathrm{d}E_n}{\mathrm{d}n} = h\omega_n. \tag{2.11}$$

and that (b) having "assumed that the motion of the particles in the stationary states of the system can be determined by help of the ordinary mechanics,"[90] (2.11) is perfectly equivalent to

$$T_n = \frac{1}{2} nh\omega_n, \tag{2.12}$$

where T_n is the mean value of the kinetic energy of the particle on the nth state of the atomic system. In addition, in his view, there were good reasons to assert that it is possible to develop a theory that can unify under a single quantum hypothesis the radiation processes involving either a harmonic oscillator or an atom, at least in cases where you have simple periodic motions.

To prove (2.11) Bohr moves from the observation that when n is large the quantum condition on the frequencies

$$\nu = \frac{1}{h}\left(E_{n+1} - E_n\right) \tag{2.13}$$

approaches

$$\nu = \frac{1}{h}\frac{dE_n}{\mathrm{d}n}; \tag{2.14}$$

and this is trivially true if we assume, in accordance with the law on the spectrum of hydrogen, the energy of the nth state is

$$E_n = -\frac{R}{n^2}. \tag{2.15}$$

[89] "The considerations were of a preliminary nature and their main intention was to emphasize certain principal features of the explanation of the problem in question." N. Bohr, Draft of a note to Phil. Mag. Concerning the Stark effect (unpublished), CW2 370–371: 371.

[90] Bohr (1914b, p. 510).

Taking into account that, under the same limiting conditions, ν tends toward ω, (2.11) follows quite naturally from (2.14), which moreover has a significant empirical counterpart. By simply replacing E_n and ω_n with the values obtained through "a mechanical interpretation of the above-mentioned stationary states," we return once more to the well-known theoretical expression of the Rydberg constant

$$R = \frac{2\pi^2 e^4 m}{h^3}. \tag{2.16}$$

So far, apart from some difference in notation, Bohr's reasoning does not differ significantly from the conference text. However, overcoming the limits he had then deemed insuperable, he now considers it reasonable to go further. By using (2.16) for R, the expressions of the energy and the frequency of a generic stationary state become, respectively:

$$E_n = \frac{2\pi^2 e^4 m}{n^2 h^3} \quad \text{and} \quad \omega_n = \frac{4\pi^2 e^4 m}{n^3 h^3}. \tag{2.17}$$

In Bohr's opinion, this is enough to show that "the condition (2.11) holds, not only for large values of n but for all values of n."[91] A result of extraordinary theoretical value that allows us to determine what should be the relationship between the energy and the frequency of an electron that is in a stationary state. (2.11) thus represents the necessary condition for the existence of states inside the atom, which, at least for some systems, is equivalent to Planck's quantum hypothesis for the harmonic oscillator. Unfortunately, Bohr's reasoning is not free from logical flaws casting a shadow on the very possibility of a generalization of (2.11). In fact, from a true premise, "values (2.17) energy and the frequency of a generic state meet the (2.11)," he derives a consequence—"(2.11) is valid for any value of n"—in contrast with the limiting conditions that ensure its existence.

This was one of the many "stretches" that we find in the first formulations of the theory, and that in this case was probably used to legitimize the next step, in which Bohr showed the existence of a significant and complete analogy between the obtained result and the well-known Planckian relationship $E = nh\nu$. From (2.17), taking into account that in a stationary orbit, E is equal to the mean value of the total kinetic energy T of the particle, we obtain

$$T_n = \frac{1}{2} nh\omega_n. \tag{2.18}$$

The equivalence of the latter with (2.11) was then demonstrated through the application of Hamilton's principle to the system consisting of a particle moving on a closed orbit within a stationary field. Thus, even with a different itinerary, that analogy with Planck's ideas, that Bohr had rejected as devoid of any physical foundation and heuristic efficacy in front of the members of the Society of Physics, was re-established. "In

[91] Ibidem.

Planck's vibrators, the particles are held by quasi-elastic forces, and the mean value of the kinetic energy is equal to the mean value of the potential energy due to the displacements. Consequently, (2.18) forms a complete analogy to Planck's original relation $U = nh\nu$ between the energy U of a monochromatic vibrator and its frequency ν."[92]

There were thus good arguments to claim that the Planck-type condition for the stationary states—the ratio between the mean value of the kinetic energy of an orbiting electron and its frequency of revolution is equal to $(1/2)hn$—was derived from the particular coincidence that exists in the limiting region between quantum relations and classical laws. In addition, having extended the validity of (2.11) to the entire atomic system, Bohr could state that among all the mechanically possible states, those physically admitted must satisfy a condition rigidly similar to that originally used by Planck for the harmonic oscillator.

3 A last attempt

In October 1914, following an invitation by Rutherford, Bohr transferred to the University of Manchester for a readership. 1915 was to be a year of transition. Bohr had come back to address the problem of the absorption of particles α and β, and in July he had sent a long article to *Philosophical Magazine* representing "a direct continuation of the paper I wrote when I was here last time (Bohr 1913a)."[93] But, as he wrote to Hans Hansen, "The whole thing is not very exciting." He was to go back to the topics of the quantum theory of the atom during the last months of his stay in Manchester, gathering the research results in two articles.

The first one appeared in the September issue of the *Philosophical Magazine* with the title "On the quantum theory of radiation and the structure of the atom,"[94] while the second was received by the same journal at the beginning of the following year and, as I anticipated in the introduction, was withdrawn by the author shortly before being sent to print. In the latter, Bohr had set himself the aim of developing a theory based on assumptions regarding the properties of atoms that escape the ordinary laws of mechanics and electrodynamics, as well as to verify whether it was possible to present said postulates in "a mutually consistent form."[95] It was first necessary to circumscribe the field of application of the theory given its inability to account for the properties of an anyhow complex atomic system. Everything seemed rather to converge toward a drastic reduction of its validity range, which would have concerned only the class of periodic systems. John Nicholson had expressed strong reservations concerning the fact that some explanation of the spectra of complex elements could be provided by using the same assumptions introduced for hydrogen. And, although Bohr had not

[92] Ibid., pp. 510–511.

[93] Bohr to Hansen, 12 May 1915, CW2 516. Bohr's article dated July was published in the October issue: Bohr (1915b).

[94] Bohr (1915a). In this article, Bohr provides a correct interpretation of the famous Frank and Hertz experiment, around which an interesting debate developed, cfr. Kragh (2012, pp. 143–146).

[95] CW2 433.

accepted his criticisms,[96] already in the September article he had reformulated one of the basic assumptions in a "more cautious form: The relation between the frequency and energy of the particles in the stationary states can be determined by means of the ordinary laws of mechanics *if these laws lead to periodic orbits*."[97]

In this "unlucky" work, Bohr abandoned any strategy to verify the existence of favorable conditions for the use of quantum hypothesis to support the atomic model, undertaking on a much more ambitious project: to demonstrate the possibility of building an atomic theory along the lines of Planck's. It was a goal that he considered so important as to overshadow the fact that such a theory would still have spoken only of the simplest systems, such as hydrogen. Of course, if successful, it would be a useful starting point for generalizations up to the most demanding tasks of interpretation. And speaking of this project Bohr wrote to Oseen: "I have written a paper in which I have made an attempt to show that the assumptions of the theory can be put on a logical consistent form, which covers all the different kinds of applications [...] which assume a discontinuous distribution of the possible (stationary) states."[98] So at the beginning of 1916, Bohr had sent to that famous journal a work that was supposed to mark a crucial step in his research program. In the article, Bohr addressed a wide range of topics systematically, which it is certainly not possible, and perhaps not even useful, to account for in this context. For our purposes, it is sufficient to represent the general structure of the theory and to rebuild the logical–conceptual framework within which Bohr hoped to place a coherent discussion of atomic phenomena.

Everything starts from a fundamental postulate that Bohr calls "assumption A" in which the contrast between the notion of quantum discontinuity and the classic image of physical reality is expressed with great effectiveness. "An atomic system can exist permanently only in a certain series of states corresponding with a discontinuous series of values for its energy, and any change of the energy of the system including absorption and emission of electromagnetic radiation must take place by a transition between such states. These states are termed "the stationary states" of the system."[99] Also due to the total absence of terms relating to some modeling representation,[100] Bohr had reason to claim that it is "consistent with Planck's original assumption as to the possible values for the energy of an atomic vibrator."[101] The foundations of the new project were thus laid down: the theory is supported by a single postulate fully compatible with Planck's quantum hypothesis, the effectiveness of which would therefore not be limited to applications of an essentially statistical nature. Indeed, Bohr used the word "consistent"

[96] "These calculations have been criticized by Nicholson (1914), who has attempted to show that the configurations chosen for the electrons in the atoms are inconsistent with the main principles of the theory, and has also attempted to prove the impossibility of accounting for other spectra by help of assumptions similar to those used in the interpretation of the hydrogen spectrum. Although I am quite ready to admit that these points involve great and unsolved difficulties, I am unable to agree with Nicholson's conclusions" Bohr (1915a, p. 399).

[97] Ibidem, my emphasis.

[98] Bohr to Oseen March 17, 1916, cit.

[99] CW2 434.

[100] Unlike what we find in his earlier articles, see for example Bohr (1915a, p. 396).

[101] CW2 434.

in such a strict sense as to enable him to reduce the assumption A to a simple variant of Planck's original concepts. This profound conviction was at the root of Bohr's rejection of the so-called "second Planck's theory," which the German physicist had formulated in those years and taken up in the new edition of *Vorlesungen*, published in early 1913.[102] In Planck's later work—Bohr writes—assumption A is abandoned and replaced by the assumption "That an atomic system can exist permanently *for any value* of its energy, and that *merely the emission of electromagnetic radiation is discontinuous*; such radiation being emitted only if the energy of the system has certain *critical values* and then always in finite amount."[103] Planck had come belatedly to admit that the black body theory could not prescind from the idea of discontinuity, and in recent work had tried to minimize its impact, considered to be devastating, by confining the discontinuity to the emission processes alone.[104] Although this solution, as Planck had shown, would allow him to obtain similar results in certain statistical applications, Bohr rejected it, judging it—among other things—contrary to the his quantum image of the atom. In particular, he was to demonstrate the incompatibility between theories based on the two different Planckian notions of discontinuity in the third paragraph of the article, concerning the probability of the various stationary states in a distribution of statistical equilibrium. I will not deal here with this aspect but rather focus on the first two paragraphs, in which Bohr addresses: (a) the conditions that must be satisfied in the stationary states and (b) the nature of the radiation that is emitted or absorbed during a transition between different states. In both cases, assumption A, to which Bohr attributes "an entirely negative character," needs to be supplemented by further postulates.

3.1 The stationary states

If we look at the atom through its modeling representation, assumption A reveals an evident inconsistency between the concept of stationary state and classical electrodynamics; this, however, does not mean that as long as we are dealing with periodic orbits, the dynamical equilibrium of a stationary state should not be described by the laws of mechanics, thus developing on this basis a "theory for the stationary states of periodic systems." Of course, according to Bohr, such a theory had to be consistent with the fundamental postulate, that is to say a Planck-type theory.

The basic steps of Bohr's demonstration can be summarized schematically in these terms. Firstly, by applying Hamilton's principle to a mechanical system with constant frequency of vibration ω, we get the mean value of the kinetic energy (equal to that of the potential energy)

$$\bar{T} = \omega \oint T \mathrm{d}t \tag{3.1}$$

[102] Planck (1913). On this see Kuhn (1978, chap. X).

[103] CW2 434, my emphasis.

[104] "Of course my project can't be carried through without resort to hypotheses, and I fear that your hatred of the zero-point energy extends to the electrodynamic emission hypothesis that I introduced and that leads to it. But what's to be done? For my part, I hate discontinuity of energy even more that discontinuity of emission." This Planck wrote to Paul Ehrenfest in the spring of 1915; cit. in Kuhn (1978, p. 253).

This expression then allows us to state that the Planckian condition for the energy of a linear atomic oscillator with constant frequency ω, $E = nh\omega$, is equivalent to

$$\frac{\bar{T}}{\omega} = \oint T\mathrm{d}t = \frac{1}{2}hn \tag{3.2}$$

Always according to Hamilton's principle, in the case where we are concerned with small variations in the motion of a periodic system, themselves also periodic, we get

$$\mathrm{d}\bar{W} = 2\omega\delta\left(\frac{\bar{T}}{\omega}\right) \tag{3.3}$$

where W represents the total energy equal to the sum of the energy of motion of the particle and the potential energy of the system. Finally, taking into account that for any orbit in dynamic equilibrium W must be constant and equal to E, from (3.3) it follows that, for systems with periodic orbital motions, "the total energy is the same for every state of the system which satisfies the (3.2) for the same value of n."[105] This is equivalent to saying that for periodic systems only stationary states that, in accordance with the laws of mechanics, comply to (3.2) are allowed and consequently, given the demonstrated equivalence, they satisfy Planck's expression for the energy of the harmonic oscillator. According to Bohr, this is "a necessary condition for the validity of assumption A."

Therefore, there are no irreconcilable contradictions between the "quantum" concept of stationary state and the classical description of the orbital motion of electrons, between a mechanical representation of the atom and assumption A; it is in fact the latter to establish that for periodic motions, the only states permitted are again those for which Planck's hypothesis is satisfied.[106] So Bohr gets to a first important conclusion: "we might regard [this hypothesis] as a relation expressing certain characteristic properties of the distinctive motions of an atomic system." But the fact that in addition to this, Planck's hypothesis included another meaning concerning the radiative behavior of atoms was to have implications of such significance as to influence the fortunes of the article, but also, as we shall see, to suggest the formulation of the correspondence principle.

[105] CW2 436.

[106] Starting from the finding that "$\bar{T}/\omega$ will remain constant for any small variation of motion, produced by external forces, for which W is unaltered" Bohr devoted ample space to the discussion on the role played by the invariant quantities in the quantum theory; however, they represented "a necessary condition for the application of ordinary mechanics to the stationary states of periodic systems"(ibid.). Of course, Bohr recognized the merits of Paul Ehrenfest in having achieved this result and for his formulation of the adiabatic principle. The latter, to which Bohr preferred to always refer to as the principle of mechanical transformability, played a crucial role together with the correspondence principle in the construction of the atomic theory up to the birth of quantum mechanics.

3.2 The nature of radiation

The reference to Planck also informs the study of the radiative process that, according to the quantum postulate, is associated with a transition between stationary states of the atom. Such is the topic of the next paragraph, in which Bohr expressed his belief that, without attempting "to describe the mechanism of transition in any detail," it is possible to get a consistent theory of spectral lines by means of a formal generalization of Planck's original theory. The generalization which Bohr intends is the transferring to the atomic system of the condition that the frequency of any radiation emitted or absorbed by a harmonic vibrator of constant frequency is equal to the frequency of vibration of the particles. If in Planck's theory, on the basis of simple kinematic considerations, this assumption is regarded as completely natural, things get complicated in the case of the theory of atomic spectra where a logical step that cannot be taken for granted is required. However, with a show of great optimism Bohr claimed that "we therefore get from [$E = nh\omega$] the following relation between the frequency of the radiation n and the amount of energy emitted during the passing of the system between two successive stationary states,"[107]

$$E_{n+1} - E_n = h\nu. \tag{3.4}$$

Even if we limit ourselves to the formal aspect, here there's just nothing for granted, since in support of this conclusion it would be necessary for the following conditions to be simultaneously fulfilled: (i) $\omega_{n+1} = \omega_n = \omega$ and (ii) $\nu = \omega$, which is difficult to sustain in the light of the "condition of reality" of the stationary states obtained earlier by Bohr. In fact, (3.2) does not provide any information concerning the relationship between the frequencies of motion corresponding to different values of energy, nor of course does it allow us to state that for every n (i) is satisfied. Failure to verify (i) implies the invalidity of (ii) and then of the very premise for the entire procedure. However, this did not prevent Bohr from stating, with evident surprise for the reader, that (3.4) "expresses Planck's original assumption of emission and absorption in quanta $h\nu$."[108] In fact, the validity of this statement was traced back again to the often cited 1910 work by Debye, but what interested Bohr the most was the fact that Planck's original assumption could be regarded not only in relation to the properties of the stationary states but also as a statement about a property of the process of radiation.

If the aim was the building of a general theory of spectra, the procedure of generalization needed additional steps because, as Bohr acknowledged a little later: "the frequency of vibration will not be equal for two successive stationary states,"[109] i.e., it is not always true that $\omega_{n+1} = \omega_n = \omega$; and (b) it is not even to be expected for there to be a simple relationship between the frequency of the monochromatic radiation and the frequency of vibration in the stationary states involved in the process of radiation; on the contrary, this is totally incomprehensible in the light of ordinary physical ideas.

---

[107] CW2 443.

[108] Ibidem.

[109] Ibidem.

It is certain only, once again, that, in the low frequencies zone, $\omega_{n+1}/\omega_n \rightarrow 1$ and $\nu \approx \omega$; here, since condition (i) is satisfied, so is (ii).

At this point, taking up a result from previous articles Bohr states that a necessary condition for the general validity of (3.4) is that when n is large $\mathrm{d}E/\mathrm{d}n = \omega h$; and, like two years before, he believes this relation to be true "not only in the limit but quite generally for any value of n."[110] Then, the condition of reality of the states—(2.11) in the previous paragraph—was the result of a procedure burdened with obvious logical contradictions; now, Bohr succeeds in demonstrating this to be a direct consequence of the condition required by the laws of mechanics for states in dynamic equilibrium; in short, (2.11) ensues from (3.2).

We could say that with this, the circle is closed. On the one hand, it had been possible to bring back to assumption A the two conditions, respectively, concerning the existence of stationary states and the processes of interaction between atom and radiation. On the other hand, there was an obvious logical compatibility between those conditions since both are dependent on the $\mathrm{d}E/\mathrm{d}n = \omega h$ relationship between the energy and frequency possessed by the electron in a generic stationary state. On a closer look, this result was not at all surprising, if only one takes into account that that relationship had the extraordinary "power" to keep incompatible representations of reality together: the space–time description of the motion of electrons in a given state and the discontinuous evolution of the system accompanying every interaction with radiation. It was probably quite enough to assert that it was possible "to show that the assumptions of the theory can be put on a logical consistent form." But even in this case, as we shall see shortly, everything is not so obvious.

Let us return again to the article's text in order to follow the last step in the formal generalization of Planck's theory to which Bohr had referred to at the beginning of the paragraph. If so far he had limited himself to considering only transitions between contiguous stationary states, Bohr now argues that the general condition for the energy of radiation is also valid in the case of transitions between two generic stationary states.

$$E_{n_2} - E_{n_1} = h\nu\,; \tag{3.5}$$

and, according to him, even in this case this is a generalization consistent with the initial hypothesis on the frequency of the radiation emitted by a harmonic oscillator. Obviously, from the quantum point of view nothing changes if the states involved in the process are or not contiguous: in these states, the frequency of the electron's motion is always different and, whatever the cause, the emitted radiation is always monochromatic. Also, for two generic stationary states, in the limit where n_1 and n_2 are large compared with their difference, the same quantitative convergence takes place as would be expected from the classical theory of radiation: we must merely expect $\nu \approx (n_2 - n_1)\omega$, confirming the fact that "the motion of any periodic system which is not an harmonic vibrator can be resolved in harmonic terms corresponding with frequencies which are entire multiples of the frequencies of revolution ω."[111] Bohr believed, therefore, that there were good reasons to conclude that by using

[110] Ibid., p. 268.

[111] Ibid., p. 269.

Planck's quantum ideas "it seems possible to construct a theory based on (3.2) and (3.5) which covers the spectrum of an harmonic vibrator as well as that of the hydrogen atom."[112] Ultimately, it was the realization of a long-pursued objective: unify under the same theory and in the light of a system of assumptions and postulates formally compatible with each other, both the phenomena related to the thermal radiation and those regarding the spectra of the elements.

Upon a closer look we discover, however, that his reasoning is by no means free from stretches and contradictions, and this poses a serious problem for the effective success of his attempt. A very clear confirmation is given from the examination of one of the key steps of his theoretical scheme—where Bohr had claimed that $\mathrm{d}E/\mathrm{d}n = \omega h$ is a consequence of $\bar{T}/\omega = 1/2hn$. In short, we can certainly say that (2.11) derives from (3.2) and (3.3) but in order to ensure compliance with the minimum standards of rigor we must assume that in (3.2) n is a continuous variable. Aware of this, Bohr used an expression, "if for a moment we consider n as a continuous variable,"[113] that had a very strong theoretical implication: it contained the invitation not to take into account the discrete nature of atomic processes and to basically deny assumption A, the pillar of the whole theoretical system.

Although Bohr had missed his target, we cannot deny that this attempt would, however, provide essential information for the reformulation of his research program. Undoubtedly, the new approach contained interesting changes compared with his previous not very convincing attempts; in particular, it brought the whole system of theoretical hypotheses down to a single postulate, an assertion concerning the nature and physical properties of the atomic system that Bohr considered to be the necessary requirement for a Planck-type theory. From here, the theory could develop along the directions outlined by the two meanings in which Planck's hypothesis split when applied to the atomic system: one concerning the electron motion in a generic stationary state, the other the process of interaction with radiation. In the case of the stationary states, even if only for the simplest systems, a relationship between energy and frequency was found such that could express the necessary condition for their existence. The study of the radiative process, on the other hand, had been much more complex and strewn with obstacles, in which apparently the conditions for a generalization of the Planckian concepts no longer existed.

Bohr had not succeeded in his attempt and the question of how to reconcile these two aspects remained open, both being essential to a full understanding of the structure and physical behavior of atoms within a consistent theoretical framework. Nevertheless, he considered that article as a crucial step in the maturing of his theoretical point of view. In fact, it showed which were the insurmountable limits for an atomic theory required to meet the requisites of a Planck-type interpretative scheme and at the same time to respect the conceptual constraints imposed by the quantum postulate. If one stuck to a mechanical representation of the atomic system, this was the best possible outcome from a theory which was forced to entrust its potential interpretation to a weave of classical concepts and quantum hypothesis.

112 Ibidem.

113 Ibid. 268.

Bohr decided not to publish the article when the drafts were already on his table, and we cannot think the cause of such a drastic choice to have been a belated evaluation of the lackluster results of that work. As he wrote to Oseen, he was fully aware of the impossibility of developing a logically consistent Planck-type theory.[114] It was precisely because of this "negative value," initially, that he had thought that in any case the article ought to be brought to the attention of the scientific community. Instead he was forced to change his mind when he realized that, with the publication of a new paper by Sommerfeld, entirely new horizons opened up for the quantum theory of the atom. Perhaps to make his choice less traumatic, Bohr spoke in his letters of a simple postponement of the publication, for the time necessary to make the changes and additions required in light of Sommerfeld's theoretical innovations.

After a few years, the withdrawn paper was brought to the attention of readers in the German language version. In fact, Bohr had expressed some misgivings on whether to publish a collection of his articles, because of the great changes that had occurred in the meantime in the field of atomic physics. He thought, indeed, that those articles contained positions that "do not conform at all points to my present views of the problems treated."[115] However, what led him to accept the publisher's project was the opportunity thus offered him to publish the "withdrawn paper" alone. The latter appeared as the tenth and last of the contributions collected in the volume *Abhandlungen über Atombau aus den Jahren 1913–1916* published in 1921. Here are all of Bohr's most important works, starting from the trilogy[116] up to the choice of concluding the collection with the withdrawn paper, certainly not due to editorial requirements. With said choice he meant reaffirm that that "unlucky" contribution had, however, played an extremely important role in the process of scientific maturation, calling it a "connecting link between my first papers on the atomic problems and the later ones."[117] In addition, Bohr considered it a good starting point for his preface, a couple of pages offering us an effective and striking summary. Of course, he does not present a critical analysis of the reasons that had hampered the implementation of a challenging project or actually demonstrated its impracticability. It was rather a different reading, aimed at highlighting the elements of continuity with a radically changed theoretical context in which, in Bohr's opinion, the correspondence principle assumed a central role. Witness the fact that in a large text of about 6,500 words Planck's name was virtually ignored.[118]

[114] Bohr to Oseen, March 17, 1916, CW2 571–573: 572.

[115] Bohr (1921, p. iv).

[116] In fact, there was a lacuna and, in light of what we have so far gathered, it can be argued that it had not been a trivial oversight. In that collection, the conference of December 1913 was absent, a text that was the antithesis of the theoretical perspective of the withdrawn paper and where not surprisingly, as it will become clear in the next section, Bohr located the "first germ" of the correspondence principle.

[117] Ibid., p. v.

[118] The quotes are just two. The first one concerns Plank's oscillator brought forth as an example of a periodic system in the framework of the exam for Ehrenfest adiabatic principle (ibid. p. vi). The second one concerns a reference to the withdrawn paper in which "a preliminary attempt was made in the section in question to extend the assumption made by Planck about the a priori probability of a harmonic oscillator to systems of several degrees of freedom" (ibid. p. vii).

4 "Instead of doing this ..."

"My life from the scientific point of view passes of[f] in periods of over happiness and despair, of feeling vigorous and overworked, of starting papers and not getting them published, because all the time I am gradually changing my views about this terrible riddle which the quantum theory is. In the last years, things have in this respect of unsuccessful literary occupation come to a climax (I hope so at any rate) and I have been rewriting and rewriting again the paper on the principles of the theory of which we spoke already in England. Some time ago, when the paper was getting quite unmanageable, I resolved to divide it in four parts, and the first part was printed as separate copies some months ago."[119]

This severe and merciless account is contained in a letter Bohr had sent on August 15, 1918, to Owen Richardson, at the time professor of physics at King's College London. More than two years had passed since Bohr had refrained from publishing his article on the *Philosophical Magazine* and his optimistic predictions of the time had been undermined by an intellectual commitment that had proved to be particularly arduous. Quantum theory was a terrible puzzle, and the various attempts to shed light on its conceptual foundations had produced, after exhausting rewriting, a text which Bohr did not seem particularly happy about. Twenty months later, Bohr had the opportunity to present his ideas in the conference held at the Deutsche Physikalische Gesellschaft, following an invitation by Max Planck; his attitude had by now changed radically, all doubts and uncertainties about the new structure of the quantum theory atom dispelled.

In front of that prestigious audience, he took up the line of thought from the pages of the withdrawn paper, in the aim of finally finding a consistent solution to the problem that had run his ambitious attempt into the ground. If that article had shown what the absolute limit for an atomic theory was, understood as a direct articulation of Planck's original hypothesis, it was perhaps time to remove this assumption. It was now clear to Bohr that quantum notions could be used constructively without imposing to the theory of atomic spectra the assumption of Planck's ideas as a privileged reference. After a long and bumpy research itinerary, he had in fact managed to collect enough evidence to say that the unsustainability of such a theoretical approach was due to the extraordinary complexity of the motions of atomic particles when compared to those of a Planckian oscillator. Therefore, the claim that "assumption A" should be consistent with Planck's original assumptions had to be abandoned as nothing more than a sterile reductionist condition, and the question that had to be posed was a completely different one: "how should Planck's result be generalized in order to make its application possible"?[120] This question brought the focus back to that interpretive dichotomy regarding the two parts in which the Planckian condition breaks up, which Bohr had failed to recompose within the same interpretive scheme in the withdrawn paper. Now he was trying to present it again in decidedly more explicit terms: "Thus we might regard [Planck's] equation as a relation expressing certain characteristic properties of the distinctive motions of an atomic system and try to obtain the general

[119] Bohr to Richardson, August 1918 (CW3, pp. 14–15: 14).

[120] CW3 244.

form of these properties. On the other hand, we may also regard [that] equation as a statement about a property of the process of radiation and inquire into the general laws which control this process."[121] With different survey instruments, and especially giving up the search for a Planck-type theory, Bohr was convinced he would reach different results and prove that it was possible "in a purely formal manner to develop a spectral theory, the essential elements of which may be considered as a simultaneous rational development of the two ways of interpreting Planck's result."[122]

The differences compared to the withdrawn paper are obvious. Then, the idea of constructing a formally consistent theory concerned the possibility of reducing the postulates regarding the stationary states and the radiative processes into a "mutually consistent form." Now, it is simply stated that the evidence on which the theory of spectra rests derives from a rational generalization of the two meanings assumed by Planck's result within the atomic model. Regardless of the adopted approach, the same question begged an answer, a question arising from the convergence that exists in the zone of high quantum numbers between classical electrodynamics and quantum conditions: what is the relationship between the motion of atomic particles and the nature of the radiation emitted in a process of transition, between a causal space–time description peculiar to the laws of classical physics, and the typically discontinuous nature of the processes regulating interaction between radiation and matter? In the withdrawn paper, Bohr had thought of translating this relationship into a simple mathematical equation between the energy and frequency of motion valid for any stationary state; in the Berlin conference, he connected the link between motion and radiation directly to the correspondence principle.

Actually, the origin of that question was to be sought in the singular and conflictual coexistence between classical laws and quantum ideas that had been used to support the modeling of the atom and to ensure the conditions for its observability. In particular, the problem arose from the fact that, while the so-called frequency relation explains the monochromatic nature of the emitted radiation, the latter provides no "information about the motion of the particles in the atom, as is supposed in the usual theory of radiation."[123] Everything we can deduce from the comparison between empirical laws and the frequency relation confirms that each process is associated with a change in radiative energy inside the atom; it was sufficient evidence to justify the assumption that when the atom does not interact with the radiation it is permanently located in certain distinctive states. In this regard, significant results were obtained which, while not diminishing the contrast between the ordinary physical ideas and the reality of atomic systems, they allowed "to develop a consistent theory on the assumption that the motion in these states can be described by the use of the ordinary mechanics."[124] One would think that in Bohr there was an unconditional and aprioristic acceptance of a mechanical picture of the atomic system, as Pauli and Heisenberg later harshly rebuked. But not at all. He saw, rather, in that theory the only instrument available at

[121] Ibidem.

[122] Ibidem.

[123] Ibid. 245.

[124] Ibidem.

the time to describe the behavior of particles inside the atom; the very effectiveness of such a theory made him wonder whether even in a quantum context it was not possible, by analogy with ordinary electrodynamics descriptive ways, to establish some relationship between the nature of the radiation emitted by an atom and "the harmonic components occurring in the motion of the system." The concept of discontinuity ought to have suggested an attitude of great caution, ruling out in the first place for that relationship to be expressible in formally rigorous terms. But Bohr had on the contrary been long engaged in the realization of precisely such a goal, because clearly he was convinced that a theory built on the Planckian model ought to have provided a similar relationship to that for the energy of the harmonic oscillator for atomic systems too. Now, Bohr took note of the failure of this project, admitting that at best it was possible to assert the existence of "a far-reaching *correspondence* between the various types of possible transitions between the stationary states on the one hand and the various harmonic components of the motion on the other hand."[125] The word "correspondence" was not chosen at random but arose from a scrupulous, and for some obsessive, attention to language that is one of the characteristic features of Bohr's theoretical approach. Indeed, with that term, he was able to express with great effectiveness the existence of an unusual logical relationship between two physical processes that by their very nature were irreducible to a single interpretive scheme, but thanks to that concept, according to Bohr, "the present theory of spectra is in a certain sense to be regarded as a rational generalization of the ordinary theory of radiation."[126]

In order to illustrate the theory of spectra, Bohr once again used the example of the hydrogen atom and followed closely the approach developed at the conference that he had held many years earlier, in December 1913, at the Danish Society of Physics. He therefore took up again the expression of the frequency and the major axis in the nth stationary state, obtained from Balmer's empirical formula,

$$\omega_n = \frac{1}{n^3}\sqrt{\frac{2h^3K^3}{\pi^2 e^4 m}} \quad \text{and} \quad 2a_n = \frac{n^2 e^2}{hK}, \tag{4.1}$$

to address an important theoretical point which actually marks the transition from the old to the new research program. Bohr did observe that in order to derive the theoretical expression of the constant K, two distinct routes were possible. The first one, perhaps the simplest and apparently also the most obvious, was to compare the motions determined by (4.1) with Planck's formula for the harmonic resonator; this was the solution adopted since the "trilogy." The second, marked a turning point and was introduced by Bohr in the following way: "*Instead of doing this* I shall show how the value of K can be found by a simple comparison of the spectrum emitted with the motion in the stationary states, a comparison which at the same time will lead us to the principle of correspondence."[127] It is not about choosing between two formally different procedures that allow us to achieve the same result—in fact, on balance, in

125 Ibid. 246.

126 Ibidem.

127 Ibid. 247, my emphasis.

the first case, we would get a value of K four times lower than in the second one. The difference here is deeper, directly involving aspects of conceptual and theoretical nature. In short, Bohr indicates a radical alternative: in the first case, one is forced to recognize the validity of Planck's formula for the stationary states of the atom; in the second, on the contrary, we simply exploit a general correspondence between spectrum and movement to translate it into a principle that leads the individual radiative processes back to the existence of certain mechanical properties of atomic particles.

As in December 1913, from the Balmer formula:

$$\nu = \frac{K}{(n'')^2} - \frac{K}{(n')^2} = (n' - n'')K\frac{n' + n''}{(n')^2(n'')^2} \tag{4.2}$$

Bohr developed some limiting considerations, this time taking into account transitions between two generic stationary states with quantum numbers larger than their difference. According to (4.1), the latter can be approximated by

$$\nu \sim \left(n' - n''\right)\omega\sqrt{\frac{2\pi^2 e^4 m}{Kh^3}}. \tag{4.3}$$

where $\omega = \omega_{n'} = \omega_{n''}$. If, in general, Bohr notes, it is impossible to obtain a relation that makes the frequency of the radiation depend from that of revolution of the electrons, in the limiting area it is correct to expect that "the frequency of the radiation emitted by a transition between two stationary states [...] will coincide with the frequency of one of the components of the radiation which according to the ordinary ideas of radiation would be expected from the motion of the atom in these states."[128] The frequency ν of the radiation emitted in the transition between the states n' and n'' should be equal to that of the radiation which, according to classical theory, is produced by the corresponding tth harmonic component of motion, or from $(n' - n'')\omega$. And in order for this to occur, it is sufficient to impose the condition that the radicand in (4.3) be equal to 1; we therefore get

$$K = \frac{2\pi^2 e^4 m}{h^3}. \tag{4.4}$$

Despite the essential and irreducible difference existing between the ideas of quantum theory and the ordinary theory of radiation, the coincidence between the experimental and the theoretical value of the constant K was therefore interpreted by Bohr as a clue about the existence of a "*connection between the spectrum and the atomic model of hydrogen*";[129] and that clue led directly to the correspondence principle.

There are many points of contact with the text of the 1913 conference. Even at that time Bohr had ruled out the use of a direct application of the Planck's theory and had judged misleading the use of the analogy with that theory as a foundation; moreover, even then he had renounced to proposing a generalization of the results obtained in

128 Ibid. 248.

129 Ibid. 249, italics in the original.

the limiting region. But it is interesting to discover that there is also a significant affinity with the procedure used to derive the theoretical expression of the Rydberg constant, which confirms the opinion Bohr had expressed in the preface to the CUP volume: the 1913 conference holds the first seeds of the correspondence principle, and they concern in particular "the deduction of the expression for the constant of the hydrogen spectrum." The theoretical expression of constant K is not achieved, as Bohr had long believed, by imposing a quantum condition on the atomic particles similar to that required for the Planckian harmonic oscillator. The reasoning needed to be overturned; that expression could be reached by requiring that, similarly as to what might be expected according to classical electrodynamics, there should be a convergence between radiation and movement in the limiting region, between the frequency of a line of the spectrum and one of the harmonic components in which the electron motion can be solved. The agreement between the experimental and the theoretical value of constant K provided thus an indirect confirmation that even for quantum processes there was some correlation between motion and radiation; a statement that is the necessary premise of the correspondence principle.

The first step toward the formulation of the new principle consists in a generalization of the recently found correspondence between the different frequencies of radiation and the harmonic components of the motion. The term "correspondence" expressed with great effectiveness the idea that there was no way to mitigate the difference, even in the limiting region, between classical and quantum mechanisms of emission; Bohr, however, thought that "this correspondence between the frequencies determined by the two methods must have a deeper significance."[130] In particular, he was certain that it would also cover the relative intensities with which each line is issued. If from the quantum standpoint the emission of the radiation, corresponding to the τth harmonic of the electron's motion, depends on a process of transition between states n' and n'', for which $n' - n'' = \tau$, it follows, according to Bohr, that "the relative intensity with which each particular line is emitted depends [...] upon the relative probability of the occurrence of the different transitions."[131] The singular correspondence relationship, through which what is expected according to classical electrodynamics is extended to the quantum case, thus takes on a quite general meaning, which allows us to say that when we are dealing with large quantum numbers, the relative probability of a given transition must be "connected in a simple manner" to the amplitude of the corresponding harmonic component of the motion.

By the second generalization, Bohr came to the enunciation of the principle of correspondence where the essential elements of his new research program find an effective summary: (a) the knowledge that there is an irreducible contrast between the concept of quantum discontinuity and the ideas of classical physics and (b) the hypothesis that by a process of "rational transcription"[132] we can use all the information provided by

[130] Ibidem.

[131] Ibidem.

[132] Bohr uses this expression in Bohr (1925, p. 849): "The correspondence principle expresses the tendency to utilize in the systematic development of the quantum theory every feature of the classical theories in a rational transcription appropriate to the fundamental contrast between the [quantum] postulates and the classical theories."

ordinary mechanics and electrodynamics for the construction of a consistent quantum theory of the atom. Once acknowledged that in a limited region of the spectrum, a particularly striking connection can be struck between the two approaches to the processes of radiation, it was not at all risky to draw from it a general law regarding any transition between the atoms' stationary states: "Thus we shall assume that even when the quantum numbers are small the possibility of transition between two stationary states is connected with the presence of a certain harmonic component in the motion of the system."[133] Naturally, Bohr was not able to say in the motion of which state the harmonic component was due to appear in order to be able to affect the occurrence of a transition process.[134] Despite these obvious difficulties, a few months later, in front of the authoritative assembly of physicists gathered in Brussels for the third Conseil de physique Solvay Bohr expressed full satisfaction for the results achieved: "the examination of the atomic problems hitherto treated by means of the theory of multiply-periodic systems has given an unrestricted and convincing support for the above view, which may be referred to as the *correspondence principle*."[135]

With this principle, he finally abandoned the idea that the conditions existed to exploit Planck's ideas in the study of atoms, both in the determination of the states and in the study of the physical processes in which they are involved. As we have seen, he was convinced that this was not the way forward and, as he wrote to Oseen, it was necessary to finally acknowledge that a Planck-type theory was impossible. The coincidence resulting for high values of n cannot be generalized, if by that is meant the establishing of some relationship that would make the nature of the radiation depend on the properties of the motion of electrons. This should be excluded on principle regardless of what we know about the mechanism, still shrouded in mystery, causing the radiative processes. The generalization Bohr was thinking of, and that found full expression in the principle of correspondence, was of a completely different nature. It concerned the idea that there might be some logical relationship between profoundly distinct and mutually incompatible theoretical contexts. In the limiting area, we do not discover that discontinuous quantum processes reacquire the well-known peculiarities highlighted by the theory of classical electrodynamics and that a dependency between optical and mechanical frequencies is re-established. We discover, on the contrary, that for particular approximations of numerical order, the same physical process corresponds to a type of description that refers to different theoretical systems and conceptual apparatus; they have in the Planck h constant an insuperable separation element. The core of Bohr's reasoning consists therefore in assuming that this particular relationship, called "correspondence," could always be valid as requested

[133] Ibid. 250.

[134] In fact, in some later works, Bohr would try to solve the obvious difficulty arising from the fact that, outside of the limiting region, the "corresponding" amplitudes can be quite different in the two stationary states involved in the transition, coming also to suggest that the sought frequency was the mean value of the corresponding vibrations, calculated on a continuous series of hypothetical "intermediate states." It was the same ploy used by Ehrenfest to make it somewhat easier to explain the idea of correspondence to participants at the Solvay Conference. "This is the simplest way that can be imagined" claimed Ehrenfest during the discussion in reply to *W*. L. Bragg, who had asked how it was possible to define the average value of the corresponding frequency ω for a transition; Solvay III pp. 255–156), CW3 388–89.

[135] CW3 377, italics in the original.

by the fact that "the possibility of the occurrence of a transition between two given states [is] conditioned by the appearance in the motion of the corresponding harmonic vibration."[136]

If this is the meaning of the correspondence principle and if the idea of a relation of correspondence between motion and radiation is a way, alternative to Planck's idea, to establish a relationship between the energy and frequency of a harmonic oscillator, there are no grounds for searching the distant origins of that principle in the limiting considerations that accompanied the first wavering steps of Bohr's theory. The reconstruction contained in this article tells us something else: that the original research program hinged on the idea that it is possible to generate a quantum theory of the atom on Planck's model, would be revealed in time as highly regressive; that Bohr succeeded in avoiding that theory from crashing against insurmountable obstacles, renouncing the search for a Planck-type interpretation and assigning to the concept of correspondence the task of representing the "positive heuristics" of the new research program. I therefore consider that there are sufficiently founded arguments to speak of a correspondence principle in opposition to a Planck-type theory of the atom and just as many reasons to locate in the withdrawn paper of 1916 the turning point of Bohr's research project.

As everybody knows, the formulation of the correspondence principle was greeted by most in the scientific community with skepticism and distrust, especially for the unusual and indefinable logical and linguistic tools at work within it. Someone might see in it an extreme attempt to disguise the theory's puzzles and frailty by resorting to complicated linguistic formulas and concepts at odds with traditional scientific rigor; ultimately, as Sommerfeld said, a magic wand. Yet that principle produced consequences that were to affect the subsequent development of Bohr's theoretical concepts significantly. That principle was to support a research program that became a crucial point of reference in many matters connected to the foundation of quantum mechanics; it exhausted its heuristic function in 1927 with the formulation of the principle of complementarity and the postulate on the essential discontinuity of the microscopic processes that Bohr placed as the basis of the quantum mechanics.[137] With this postulate, Bohr would finally unveil the real physical content of Planck's quantum hypothesis, acknowledging the crucial role it played in the construction of a new physics.

[136] Bohr (1923, in CW3 p. 376).

[137] In the first lines of the opening paragraph entitled "Quantum postulate and causality" Bohr asserts: "The quantum theory is characterized by the acknowledgment of a fundamental limitation in the classical physical ideas when applied to atomic phenomena. The situation thus created is of a peculiar nature, since our interpretation of the experimental material rests essentially upon the classical concepts. Notwithstanding the difficulties which hence are involved in the formulation of the quantum theory, it seems, as we shall see, that its essence may be expressed in the so-called quantum postulate, which attributes to any atomic process an essential discontinuity, or rather individuality, completely foreign to the classical theories and symbolized by Planck's quantum of action" Bohr (1928, p. 580).

References

Bohr, N. 1913a. On the theory of the decrease of velocity of moving electrified particles on passing through matter. *Philosophical Magazine* 25: 10–31.

Bohr, N. 1913b. On the constitution of atoms and molecules, part I. *Philosophical Magazine* 26: 1–25.

Bohr, N. 1913c. On the constitution of atoms and molecules, part II. *Philosophical Magazine* 26: 476–502.

Bohr, N. 1913d. On the constitution of atoms and molecules, part III. *Philosophical Magazine* 26: 857–875.

Bohr, N. 1914a. Om Brintspektret. *Fysisk Tidsskrift* 12: 97–114.

Bohr, N. 1914b. On the effect of electric and magnetic fields on spectral lines. *Philosophical Magazine* 27: 506–524.

Bohr, N. 1915a. On the quantum theory of radiation and the structure of the atom. *Philosophical Magazine* 30: 394–415.

Bohr, N. 1915b. On the decrease of velocity of swiftly moving electrified particles in passing through matter. *Philosophical Magazine* 30: 581–612.

Bohr, N. 1918a. On the quantum theory of the line-spectra, part I: On the general theory. *Kongelige Danske Viedenskabernes Skrifter, Naturvidenskabelig og matematisk Afdeling*, 8 Roekke. IV. I: 1–36.

Bohr, N. 1918b. On the quantum theory of the line-spectra, part II: On the hydrogen spectrum. *Kongelige Danske Viedenskabernes Skrifter, Naturvidenskabelig og matematisk Afdeling*, 8 Roekke. IV. I: 37–100.

Bohr, N. 1920. Über die Serienspektren der Elemente. *Zeitschrift für Physik* 2: 423–469.

Bohr, N. 1921. *Abhandlungen über Atombau aus den Jahren 1913–1916*. Braunschweig: Vieweg & Sohn.

Bohr, N. 1922a. On the quantum theory of the line-spectra, part III: On the spectra of elements of higher atomic number. *Kongelige Danske Viedenskabernes Skrifter, Naturvidenskabelig og matematisk Afdeling*, 8 Roekke. IV. I: 101–108.

Bohr, N. 1922b. *The theory of spectra and atomic constitution*. Cambridge: Cambridge University Press.

Bohr, N. 1923. L'application de la théorie des quanta aux problèmes atomiques. In [Solvay III] 228–247.

Bohr, N. 1924. On the Application of the quantum theory to atomic structure. *Proceedings of the Cambridge Philosophical Society* 24(Suppl): 1–42.

Bohr, N. 1925. Atomic theory and mechanics. *Nature* 116(Suppl.): 845–852.

Bohr, N. 1928. The quantum postulate and the recent development of atomic theory. *Nature* 121(Suppl.): 579–590.

Bohr, N. 1961. The Rutherford memorial lecture 1958. Reminiscences of the founder of nuclear science and of some developments based on his work. *Proceedings of the Physical Society* 78: 1083–1115.

Bohr, N. 1972. *Collected works, vol. 1, Early work (1905–1911)*, ed. J. Rud Nielsen. Amsterdam.

Bohr, N. 1976. *Collected works, vol. 3, The correspondence principle (1918–1923)*, ed. J. Rud Nielsen. Amsterdam.

Bohr, N. 1981. *Collected works, vol. 2, Work in atomic physics (1912–1917)*, ed. U. Hoyer. Amsterdam.

CW1 *see* Bohr, N. 1972.

CW2 *see* Bohr, N. 1981.

CW3 *see* Bohr, N. 1976.

Darrigol, O. 1992. *From c-numbers to q-numbers: The classical analogy in the history of quantum theory*. Berkeley: University of California Press.

Darrigol, O. 1997. Classical concepts in Bohr's atomic theory (1913–1925). *Physis* 34: 545–567.

Darwin, C.G. 1912. A theory of the absorption and scattering of the α rays. *Philosophical Magazine* 23: 901–920.

Darwin, C.G. 1913. On some orbits of an electron. *Philosophical Magazine* 25: 201–210.

Debye, P. 1910. Der Wahrscheinlichkeitsbegriff in der Theorie der Strahlung. *Annalen der Physik* 33: 1427–1434.

Evans, E.J. 1913. The spectra of helium and hydrogen. *Nature* 92: 5.

Eve, A.S. 1939. *Rutherford. Being the life and letters of the Rt Hon, Lord Rutherford*. Cambridge: Cambridge University Press.

Geiger, H. 1912. Note on the scattering of α particles. *Proceedings of the Royal Society* 86: 235–240.

Geiger, H., and E. Marsden. 1913. The laws of deflexion of α particles through large angle. *Philosophical Magazine* 25: 604–623.

Heilbron, J.L. 2013. Nascent science. The scientific and psychological background to Bohr's Trilogy. In *Love, literature and the quantum atom*, ed. F. Aaserud, and J.L. Heilbron, 103–200. Oxford: Oxford University Press.

Heilbron, J.L., and Th.S. Kuhn. 1969. The genesis of the Bohr atom. *Historical studies in the physical sciences* 1: 211–290.
Jeans, J.H. 1913. Discussion on radiation. *Report of the British Association for the Advancement of Science*, Birmingham (376–381).
Klein, M.J. 1970. *Paul Ehrenfest. Vol. 1: The making of a theoretical physicist*. Amsterdam: North-Holland.
Kragh, H. 2012. *Niels Bohr and the quantum atom. The Bohr model of atomic structure 1913–1925*. Oxford: Oxford University Press.
Kuhn, Th.S. 1978. *Black-body theory and the quantum discontinuity, 1984–1912*. Oxford: Oxford University Press.
Langevin, P., and M. de Broglie (eds.). 1912. *La théorie du rayonnement et les quanta. Rapports et discussions de la réunion tenue à Bruxelles du 30 octobre au 3 novembre 1911*. Paris: Gauthier-Villars.
Lodge, O.J. 1913. President's address. *Report of the British Association for the Advancement of Science*, Birmingham (3–42).
Marsden, H., and T.S. Taylor. 1913. The decrease in velocity of α-particles in passing through matter. *Proceedings of the Royal Society* 88: 443–454.
Mayer, F. 1913. Zerstreuung der α-Strahlen. *Annalen der Physik* 41: 931–970.
Moseley, H.G.J. 1913. The high-frequency spectra of the elements. *Philosophical Magazine* 26: 1024–1034.
Nicholson, J.W. 1912a. The constitution of the solar corona. I. *Monthly Notices of the Royal Astronomical Society* 72: 139–150.
Nicholson, J.W. 1912b. The constitution of the solar corona. II. *Monthly Notices of the Royal Astronomical Society* 72: 677–693.
Nicholson, J.W. 1912c. The constitution of the solar corona. III. *Monthly Notices of the Royal Astronomical Society* 72: 729–739.
Nicholson, J.W. 1914. The high-frequency spectra of the elements and the structure of the atom. *Philosophical Magazine* 27: 541–564.
Pais, A. 1994. *Niels Bohr's times: In physics, philosophy, and polity*. Oxford: Oxford University Press.
Petruccioli, S. 2006. *Atoms metaphors and paradoxes. Niels Bohr and the construction of new physics*. Cambridge: Cambridge University Press.
Petruccioli, S. 2011. Complementarity before uncertainty. *Archives for History of Exact Sciences* 65: 591–624.
Planck, M. 1913. *Vorlesungen über die Theorie der Wärmestrahlung*, 2nd ed. Leipzig: Barth.
Rosenfeld, L. (ed.). 1963. *Niels Bohr: On the constitution of atoms and molecules*. Copenhagen: Munksgaard.
Rosenfeld, L., and E. Rüdinger. 1967. The decisive years 1911–1918. In *Niels Bohr, his life and work as seen by his friends and colleagues*, ed. S. Rozental, 38–73. New York: Interscience.
Rud Nielsen, J. 1976. Introduction in CW3 3–46.
Rutherford, E. 1911. The scattering of α and β particles by matter and the structure of the atom. *Philosophical Magazine* 23: 669–688.
Rutherford, E. 1912. The origin of β and γ rays from radioactive substances. *Philosophical Magazine* 24: 453–462.
Rutherford, E., and J.M. Nuttal. 1913. The scattering of α particles by gases. *Philosophical Magazine* 26: 702–712.
[Solvay I] *La théorie du rayonnement et les quanta. Rapports et discussions de la Réunion tenue à Bruxelles du 30 octobre au 3 novembre 1911 sous les auspices de M. E. Solvay.* 1912. Paris: Gauthier-Villars.
[Solvay II] *La structure de la matière. Rapports et discussions du Conseil de physique tenu à Bruxelles du 27 au 31 octobre 1913 sous les auspices de l'Institut international de physique Solvay.* 1921. Paris: Gauthier-Villars.
[Solvay III] *Atomes et électrons. Rapports et discussions du Conseil de physique tenu à Bruxelles du 1er au 6 avril 1921 sous les auspices de l'Institut international de physique Solvay.* 1923. Paris: Gauthier-Villars.
Sommerfeld, A. 1915. Zur Theorie der Balmerschen Serie. *Sitzungsberichte der Bayerischen Akademie der Wissenschaften München* 425–458.
Sommerfeld, A. 1916. Zur Quantentheorie der Spektrallinien. *Annalen der Physik* 51: 1–94.
Sommerfeld, A. 1919. *Atombau und Spektrallinien*. Braunschweig: Vieweg.
Stark, J. 1913. Observation of the separation of spectral lines by an electric field. *Nature* 92: 401.
Thomson, J.J. 1904. On the structure of the atom. *Philosophical Magazine* 7: 237–265.
Thomson, J.J. 1913. On the structure of the atom. *Philosophical Magazine* 26: 792–799.

Thomson, J.J. (1921). La structure de l'atome. In [Solvay II] 1–74.
Wilson, C.T.R. 1912. On an expansion apparatus for making visible the tracks of ionizing particles in gases and some results obtained by its use. *Proceedings of the Royal Society* 87: 277–292.