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10 ∴ 'It was the spring of hope, it was the winter of despair'

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(c) In pursuit of principles: Ehrenfest, Einstein, and Bohr

As I have already stated, the new logic called for by quantum phenomena did not arrive until the mid-1920s. The search for general principles underlying quantum physics began earlier, however, producing three general ideas which later could be incorporated in the new quantum mechanics.

1. *Ehrenfest*[71] *on adiabatics*. Paul Ehrenfest studied physics in his native Vienna, where his contact with Boltzmann (under whose guidance he obtained the Ph.D.) was decisive in directing him to his principal scientific devotion, statistical physics. As was noted earlier (5d, e) that was the branch of physics which had served Planck and Einstein as their prime tool in their earliest work on quantum theory. Ehrenfest studied their papers carefully. As a result he became probably the first after the founders to publish on quantum problems, beginning in 1905.[72] These early papers already showed what Einstein later called 'his unusually well developed faculty to grasp the essence of a theoretical notion, to strip a theory of its mathematical accoutrements until the simple basic idea emerged with clarity. This capacity made him... the best teacher in our profession whom I have ever known.'[73] He was respected by all who knew his work except by himself.

Ehrenfest's initial reactions to Bohr's work were decidedly negative. In 1913 he wrote[74] to Lorentz: '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.' After Sommerfeld had come out with his work on fine structure, Ehrenfest wrote[75] to him: 'Even though I consider it horrible that this success will help the preliminary but still completely monstrous Bohr model on to new triumphs, I nevertheless wish physics at Munich further success along this path.'

Ehrenfest's contribution of interest to us here, his 'adiabatic principle', was inspired by his critical analysis of the contributions by Planck and Einstein, not by those of Bohr, even though, as it turned out, the main applications of his principle were to issues in atomic physics. He published[76] this work in ever more systematic detail in the course of the years 1911-16, most of it from Leiden where, since late 1912, he had been installed as successor to Lorentz.

The gist of the adiabatic principle can be stated as follows. If you give me the quantum rules for a particular system, then I can tell you the rules for a whole class of other systems. The proof is based on the hypothesis that Newtonian mechanics continues to apply as long as systems are in a stationary state, while the quantum theory only comes in to account for jumps from one such state to another. As Bohr had stressed from the beginning,[29] this assumption also applied to his own work. Here I shall only indicate Ehrenfest's reasoning in terms of a special case: the general argument is too technical for the style adopted in this book.*

Consider a system in periodic motion characterized by a single quantum number, call it n, and by specific values of parameters such as the nuclear charge, the intensity of some external field of force, etc. Now let these parameters be subjected to extremely slow and smooth changes, called adiabatic transformations (a term borrowed from thermodynamics). What happens to the number n? The adiabatic principle says: n does not change, it remains invariant. In his unpublished paper of 1916, Bohr put it like this: 'The great importance in the Quantum theory of this invariant character has been pointed out by P. Ehrenfest; it allows us by varying the external conditions to obtain a continuous transformation through possible states

from a stationary state of any periodic system to the state corresponding with the same value of *n* of any other such system containing the same number of moving particles.'[77] The 'other' system may be quite different from the starting system. For example, one can connect in this way the rule for quantizing a (one-dimensional) oscillator with the one for the (non-relativistic) Bohr atom. This clearly brings much improved coherence to the old quantum theory: one still did not know why any system is quantized but now one could at least link the quantization of vastly distinct systems.

Ehrenfest knew that already in 1914 Einstein had recognized[78] the importance of his work but was not aware of Bohr's unpublished paper of 1916. When in 1918 Bohr incorporated this manuscript in a major paper he stressed 'the great progress ... recently obtained by Ehrenfest'.[79] When in that year Kramers returned to Copenhagen from a visit to Leiden, with regards from Ehrenfest, Bohr sent him a letter, the beginning of a long correspondence, in which he wrote: 'I hope very much to meet you when the war is over.'[80] In 1922 Ehrenfest wrote81 to Bohr about the adiabatic principle: 'I have never discovered anything - and quite surely never will discover anything - that I can love so fervently.'

The two men first met in 1919 when Bohr gave a lecture in Leiden on 'Problems of the atom and the molecule'[82] and attended Kramers' thesis defense. In December 1921 Ehrenfest lectured in Copenhagen.[83] He had come to venerate and love Bohr. In 1919, right after Bohr's visit to Leiden, he wrote to him: 'You had gone, the music had faded away.'[84] When in 1929 he took along his gifted young student Hendrik Casimir to a physics meeting in Copenhagen he said to him along the way: 'Now you are going to meet Niels Bohr and that is the most important thing to happen in the life of a young physicist.'[86]

* See Refs. 10 and 11 for more details

[2. Einstein on probability]

3. *Bohr on correspondence*. Barring the period of World War II, the longest gap in time during which Bohr did not publish was between 1915 and 1918. A letter he wrote[95] in the summer of 1918 explains why. 'I know that you understand ... how my life from the scientific point of view passes off in periods of overhappiness 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.' Bohr had so very much to cope with at that time. He had begun his demanding efforts to establish an institute of his own. Quantum physics in Copenhagen and elsewhere, was in a state of rapid flux. Its logical foundations, Bohr's overriding concern, were as obscure as ever. 'I suffer from an unfortunate inclination to make results appear in systematic order,' he wrote[96] in 1919. The subject was hardly ripe for doing so.
As mentioned, in 1916 Bohr withdrew a general review. He needed more time for reflection. The result of these ruminations was a lengthy memoir 'On the quantum theory of line spectra'. Parti appeared[38] in April l918, part II in the following December,[97] part III in 1922.[98] 'Already at the appearance of part I a manuscript of the whole treatise existed . . . [The] delay of the later parts was due in the first place to the nature of the subject.'[99] Quantum physics kept evolving before Bohr's eyes, even as he was attempting to commit his thoughts to paper.
In his later years Bohr used to say that he had perhaps never worked harder than in the course of preparing this work. His papers came out during the confusing years of war and immediate post-war and were published in a rather out of the way journal. All this, as well an I their difficult style, may have limited their initial impact. 'In the last few years I have often felt myself scientifically very lonesome, under the impression that my efforts to develop the principles of the quantum theory systematic-ally to the best of my ability have been received with very little understanding.' [100]
The topics covered in the memoir are these. In part I, a general introduction, Bohr recapitulated the developments sketched in the preceding parts of this section,

acknowledged his indebtedness to Einstein and Ehrenfest for what was to follow, and announced the main new topics and results he intended to describe. Part II, the most important one, deals with the hydrogen atom including its fine structure and its behavior in external electric and magnetic fields. Here he also gave his own treatment of the phase integral method, including his 'perturbation theory,' (inspired by Ehrenfest's adiabatic principle) which describes what happens if the forces inside the atom are modified by small external forces. Part III treats of the spectra of higher elements. It contains an appendix in which Bohr recapitulated developments since 1918 and retracted some earlier conclusions. A planned part IV, to deal with molecules was never published but has survived in fragmentary draft form.[101] So has the draft of another paper 1921(), meant to replace[102] parts III and IV, in which Bohr states with great precision the kind of interplay between theory and experiment so typical for the days of the old quantum theory.

The question is not only the development of the interpretation of experimental facts, but just [as] much by means of these to develop our deficient theoretical conceptions.[103]

In the three-part paper the main tool, of Bohr's making, is the correspondence principle, perhaps better called correspondence postulate, not so named by him until 1920, in a lecture given in Berlin.[104] (Prior to that he had called it the analogy principle.) We have already briefly met this principle in (8f) when discussing Bohr's last and best discussion in 1913 of the hydrogen spectrum. To repeat, in that case he had argued that for large values of the principal quantum number n the hydrogen levels lie so close together that they form 'almost a continuum'; and that therefore the *classical* continuum description of the emission of radiation should be very nearly valid for transitions between two very close-lying states both with very large n. Now, in 1918, he produced a quite similar kind of reasoning applied to other atomic properties.
Comments by Kramers, closest to Bohr in those years, may help to recreate the climate of those times. In 1923 he wrote: 'It is difficult to explain in what [the correspondence principle] consists, because it cannot be expressed in exact quantitative laws, and it is, on this account, also difficult to apply. [However,] in Bohr's hands it has been extraordinarily fruitful in the most varied fields.'[10B] Also in 1923, in the issue of *Naturwissenschaften*[106] commemorating the first ten years of the Bohr theory: 'In this night of difficulties and uncertainty Bohr['s] . .. principle is a bright spot.' And in 1935, on the occasion of Bohr's 50th birthday: 'In the beginning the correspondence principle appeared to the world of physicists as a rather mystic wand that did not work outside Copenhagen,'[107] not unlike Sommerfeld who earlier had called the principle 'a magic wand ... which allows us to apply the results of the classical wave theory to the quantum theory'.[108] In later years Oskar Klein, also close to Bohr around 1920, remarked[109] that Bohr had made great progress at that time 'in spite of the abyss, whose depth he never ceased to emphasize, between the quantum theoretical mode of description and that of classical physics'.

Bohr's new applications of the correspondence principle* resulted from an important change in outlook since 1913. At that earlier time his prime concern had been to understand the discreteness of the hydrogen spectrum and, more specifically, the particular frequency values of its spectral lines Meanwhile, new developments had forced him to reconsider his basic postulate according to which an electron in a state, any state, with energy E_1 can jump to a state with lower energy E_2 accompanied by the emission of a photon with frequency v: $E_1 - E_2 = h\nu$. In 1913 he had not yet considered the fine structure which splits E_t and E_2 into various energy levels. Can any of the split E_1-levels go to any of the split E_2's? Can any of the split E_l (or E_2's) go into a lower split E_l (or E_2)? The Stark effect also leads to splitting; accordingly the same questions arise there too. It was known already in 1916 that not all possible transitions occur. An example was the experimentally studied[111] fine structure of singly ionized helium in transitions $n = 4 \rightarrow n = 3$. If all accompanying changes in the auxiliary quantum number k were permitted, then one should find 4 x 3 = 12 spectral lines. The number actually seen was considerably smaller. Speculations

arose:[58] Are some transitions forbidden? Or allowed but producing lines with too low an intensity to be detectable?
Bohr's memoir of 1918-22 centered on the now paramountly important question of line intensities. In general terms, he continued to adopt the strategy followed in 1913 (8f): in the low-frequency limit, equate optical frequencies (corresponding to transitions between neighboring stationary states with high *n)* with the (classical) mechanical frequencies of an electron circling a center of charge, which in turn equal the light frequencies emitted according to the classical theory. Now, however, he had to extend his earlier reasoning to the case that more than one quantum number appears. His main results can be summarized as follows.

(i) Consider classically an ensemble of orbiting electrons emitting very low frequency radiation. Let some a priori conceivable frequency actually be missing as a consequence of the detailed classical theory Invoking the correspondence principle, Bohr argued that this same frequency should as well be forbidden in the quantum theory. From this he could deduce** a selection rule for Δk, the change in k in low frequency quantum jumps: $\Delta k = +1$ for one-electron systems (hydrogen, ionized helium), $\Delta k = 0, \pm 1$ for more complicated systems. The same result was also obtained[112] independently by Sommerfeld's student Adalbert Rubinowicz. In the example $n = 4 \rightarrow n = 3$, this rule reduces the number of fine structure lines from 12 to 5 - an almost correct result. I shall come back in (11f) to a last needed refinement

(ii) Applying the same reasoning to emission in some fixed direction, Bohr obtained another selection rule valid for an atom in an external field: $\Delta m = 0, \pm 1$

(iii) In more detail, emission with $\Delta m = 0$ corresponds to light polarized*** parallel to the field direction. For $\Delta m = \pm 1$ light is polarized perpendicular to that direction.

(iv) Bohr correctly guessed[113] the range of admissible m values.

(v) All previous statements referred to what happens at low frequencies. Bohr made the daring and, it turned out, correct extrapolation that *the selection and polarization rules stated above hold for all frequencies.*

Bohr also outlined the way one could estimate intensities of allowed spectral lines by correspondence arguments. Inspired by Einstein's notion of spontaneous emission (actually so named first by Bohr[114]) he produced a classical formula for the probability per unit time for emission of light of a given frequency by an ensemble of atoms. He did not himself work out the consequences of that equation. Kramers, who was given this task, did so brilliantly, first working out the general theory in much more detail, then via correspondence arguments calculating spectral intensities for fine structure and the Stark effect. Finally, his comparison of the results with experiment turned out to work very well. Kramers' resulting paper, his thesis,[39] contains the principal confirmations of Bohr's ideas.

During 1921-4 Bohr continued to elaborate and improve his presentation of the principles of the quantum theory.
1921. A review of the situation in the preface[115] to a book[116] containing the German translation of his collected papers of the years 1913-16, including the one[48] he had withdrawn in 1916; an amplification[117] of his results on polarization; his report to the Solvay conference of October 1921. Because of Bohr's forced absence due to illness this report was presented by Ehrenfest, along with comments of his own.[118]
1922. A simplified presentation of the correspondence principle and its applications, in the Guthrie lecture[119] before the Physical Society of London; further elaborations[120] of selection principles; publication[121] in German, English, and French of a selection of Bohr's papers from the years 1914-21; unpublished notes[122] of his Göttingen lectures. 1923. Appearance in book form[123] of a German translation of the three-part memoir; a lecture[124] in Liverpool on the correspondence principle, given before the British Association for the Advancement of Science; a general review[126] on line spectra and atomic structure; publication of the first part[126] of a planned new

comprehensive treatise on atomic structure in which Bohr announced[127] that this was 'the first of a series of essays ... [on] atomic structure'. Bohr did not publish the manuscript[128] of the second part, nor did he complete fragments[29] of manuscripts from 1923—4, presumably meant to be incorporated in later parts.

The correspondence principle is, I think, Bohr's greatest contribution to physics after his derivation of the Balmer formula. It is the first manifestation of what would remain the leading theme in his work*: classical physics, though limited in scope, is indispensable for the understanding of quantum physics*. In his own words:

> Every description of natural processes must be based on ideas which have been introduced and defined by the classical theory.[130]

At first glance cognoscenti might well think that Bohr wrote this after 1927, when he began developing complementarity concepts. However, these lines date from 1923!

* For a discussion of this work in much more detail than given here, see Refs. 11 and 110
** Bohr realized that this answer holds only for radiation of the electric dipole type.
*** Polarization direction was defined in (4e).

71. For an account of Ehrenfest's early life and work see M. Klein, *Paul Ehrenfest,* North-Holland, Amsterdam 1970.
72. Ref. 71, Chap. 10.
73. Einstein, *Out of my later years,* p. 236, Philosophical Library, New York 1950.
74. P. Ehrenfest, letter to H. A. Lorentz, 25 August 1913, quoted in Ref. 71, p. 278.
75. P, Ehrenfest, letter to A. Sommerfeld, May 1916, quoted in Ref. 71, p. 286.
76. Complete references are found in P. Ehrenfest's review article, *Naturw*. 11, 543, 1923.
77. Ref. 48, p. 436.
78. Einstein, *Verh. Deutsch. Phys. Ges*. 16, 820, 1914.
79. Ref. 38, Introduction.
80. N. Bohr, letter to P. Ehrenfest, May 1918, CW, Vol. 3, p. 12.
81. P. Ehrenfest, letter to N. Bohr, 8 May 1922, NBA.
82. This lecture is reproduced in CW, Vol. 3, p. 201.
83. CW. Vol. 3, p. 36.
84. P. Ehrenfest, letter to N. Bohr, 4 June 1919, CW, Vol. 3. p. 16.
85. H. B. G. Casimir in NBR, p. 109.
86. Einstein, letter to M. Besso, 18 November 1916, repr. in *Albert Einstein-Michele Besso correspondance,* p. 78, Ed. P. Speziali, Hermann, : Paris 1972.
87. Lecture on 26 April 1917, notes in NBA.
88. Fysi*sk Tidsskr*. 17, 67, 1918-19.
89. Einstein, *Verh. Deutsch. Phys. Ges*. 18, 318, 1916.
90. Einstein, *Mitt. Phys. Ges. Zurich* 16, 47,1916; *Phys. Zeitschr*. 18,121,1917.
91. For more details see SL, Chap. 21, sections (b), (c), and (d).
92. E. Rutherford, *Phil Mag*. 49, 1, 1900.
93. IB. Chap. 6, section (e).
94. Einstein, letter to M. Born, 27 January 1920, repr. in *The Born-Einstein* vrs, p. 23, Walker, New York, 1971.
95. N. Bohr, letter to O. W. Richardson, 15 August 1918, CW, Vol. 3, p. 14.
96. N. Bohr, letter to A. Sommerfeld, 27 July 1919, CW, Vol. 3, p. 17.
97. N. Bohr, *Kong. Dansk. Vid. Selsk. Skrifter,* 1918, p. 37,
98. CW, Vol. 3, p. 103.
99. N. Bohr, *ibid*. p. 101, CW, Vol. 3, p. 167. V. Vol. 3, p. 178.
100. N. Bohr, letter to A. Sommerfeld, 30 April 1922, CW, Vol. 3, pp. 39, 691. Cf. Also N. Bohr, letter to A. Haas, 11 April 1922, CW, Vol. 3, p. 647.
101. CW. Vol. 3, p. 185.
102. CW. Vol. 3, p. 36.
103. CW. Vol. 3, p. 397.
104. CW. Vol. 3, p. 241.

105. H. A. Kramers and H. Hoist, *The atom and the Bohr theory of its structure,* : S9. Knopf, New York 1923,
106. H. A. Kramers, *Naturw*. 4, 550, 1923.
107. H. A. Kramers, *Fysisk Tidsskr*. 33, 82, 1935.
108. Sommerfeld, *Atombau und Spektrallinien,* 3rd edn, p. 338, Vieweg, Braunschweig 1922.
109. O. Klein, NBR, p. 77.
110. P. Kristensen, *Fysisk Tidsskr*. 84, 124, 145, 1986.
111. F. Paschen, *Ann. der Phys*. 50, 901, 1916.
112. A. Rubinowicz, *Naturw*. 19, 441, 465, 1918.
113. N. Bohr, CW, Vol. 3, p. 142, footnote.
114. CW, Vol. 3, p. 73.
115. CW, Vol. 3, p. 303; English transl. in CW, Vol. 3, p. 325.
116. N. Bohr, *Abhandlungen uber Atombau,* Vieweg, Braunschweig 1921.
117. N. Bohr, *Zeitschr. Phys*. 6, 1, 1921; English transl. in CW, Vol. 3, p. 350.
118. CW, Vol. 3, p. 357.
119. N. Bohr, *Proc. Phys. Soc. London* 35, 275, 1923; CW, Vol. 3, p. 417.
120. N. Bohr, *Phil. Mag*. 43, 1112, 1922; CW, Vol. 3, p. 447.
121. N. Bohr, *Drei Aufsdtze uber Spektren und Atombau,* Vieweg, Braunschweig 1922; *The theory of spectra and atomic constitution,* Cambridge University Press 1922; *Les spectres et la structure de I'atome,* Hermann, Paris 1922.
122. Repr. in CW, Vol. 4, p. 341.
123. N. Bohr, *Uber die Quantentheorie der Linienspektren,* Vieweg, Braunschweig 1923.
124. CW, Vol. 3, p. 576.
125. N. Bohr, *Ann. der Phys*. 71, 228, 1923, English transl. in CW, Vol. 4, p. 611
126. N. Bohr, *Zeitschr. Phys*. 13, 117, 1923; English transl. in CW, Vol. 3, p. 45"
127. CW, Vol. 3, p. 458, footnote.
128. Found in CW, Vol. 3, p. 501; English transl. in CW, Vol. 3, p. 532.