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The Early Reception of Bohr's Atomic Theory (1913-1915): A Preliminary Investigation

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The Early Reception of Bohr's Atomic Theory (1913-1915): A Preliminary Investigation

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Contents

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On the Constitution of Atoms and Molecules.

By N. BOHR, *Dr. phil. Copenhagen* *.

Introduction.

IN order to explain the results of experiments on scattering of α rays by matter Prof. Rutherford † has given a theory of the structure of atoms. According to this theory, the atoms consist of a positively charged nucleus surrounded by a system of electrons kept together by attractive forces from the nucleus; the total negative charge of the electrons is equal to the positive charge of the nucleus. Further, the nucleus is assumed to be the seat of the essential part of the mass of the atom, and to have linear dimensions exceedingly small compared with the linear dimensions of the whole atom. The number of electrons in an atom is deduced to be approximately equal to half the atomic weight. Great interest is to be attributed to this atom-model; for, as Rutherford has shown, the assumption of the existence of nuclei, as those in question, seems to be necessary in order to account for the results of the experiments on large angle scattering of the α rays ‡.

In an attempt to explain some of the properties of matter on the basis of this atom-model we meet, however, with difficulties of a serious nature arising from the apparent

* Communicated by Prof. E. Rutherford, F.R.S.
† E. Rutherford, Phil. Mag. xxi. p. 669 (1911).
‡ See also Geiger and Marsden, Phil. Mag. April 1913.

Phil. Mag. S. 6. Vol. 26. No. 151. *July* 1913. B

The beginning of the first part of Bohr's trilogy, as it appeared in the July 1913 issue of *Philosophical Magazine*. The second and third parts were published in September and November the same year.

1. Introduction

Niels Bohr's quantum theory of the atom, introduced in the summer of 1913 in a landmark paper in *Philosophical Magazine*, is recognized as one of the foundations of the modern physical world view. Together with related advances, such as Rutherford's nuclear model and the associated notions of isotopy and atomic number, it initiated a new and immensely fruitful research programme that eventually lead to the emergence of quantum mechanics. According to Robert Millikan, the period 1912-1914 was "comparable in importance with the period of the laws of Galilean-Newtonian mechanics some three centuries earlier." A major reason was Bohr's theory: "For the immense field of spectroscopy was essentially an unexplored dark continent prior to the advent of Bohr's theory. Bohr's equation has been the gateway through which hundreds of explorers have since passed into that continent until it has now become amazingly well mapped."[1]

The purpose of this essay is to map how Bohr's theory was initially received in the physics community since its appearance in the summer of 1913 until the end of 1915. Because of the technical nature of Bohr's work, in the period it was almost only known by scientists and played a role only in scientific contexts, mostly related to physics but in a few cases also to chemistry and astronomy. There is no point in looking for the public or popular reception of the theory, for there was no such reception until several years later. There is in the earlier historical literature several works that deal with the reception of Bohr's theory, but they are not systematic studies and they do not aim at a full discussion of how scientists knew about and responded to the theory.[2] The

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[1] Millikan 1951, p. 110.

[2] The most useful accounts of the early reception history are to be found in Heilbron 1964, Hoyer 1974, and Hoyer 1981, but see also Rosenfeld 1963, Mehra and Rechenberg

primary source for the present reception study is the scientific literature in the form of articles, books and conference proceedings, but of importance are also informal information as found in letters and reminiscences. Citations to Bohr's works give an indication of the impact of the theory, but they are of little value if used in isolation. Citation data from existing data bases are unfortunately unreliable and therefore useless as tool for the historian.[3]

My reason for ending the survey about the end of 1915 is in part that at that time Bohr's original theory was widely recognized and on its way to be further developed into the Bohr-Sommerfeld theory. The first two and a half years marked the childhood of the theory, after which period it entered a new and more mature phase increasingly influenced by Sommerfeld and other German physicists. My review is largely limited to England, Germany and the United States, or rather to the scientific communities and publications from these countries. Although Bohr's theory was known also in other countries (such as France, Italy, the Netherlands, and the Scandinavian countries), to my knowledge it was only in England and Germany that it was widely discussed and the subject of a great deal of interest. A fuller study would include a broader selection of countries and invite a comparative perspective, but here the comparison is limited to England and Germany.

After a summary presentation of Bohr's atomic theory, such as laid out in his great work of 1913, I examine the immediate response to it, meaning responses until about the end of November of that year. In Section 4 I deal with the reception in England, the first country in which the theory attracted serious

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1982 and Pais 1991, pp. 152-155. Hoyer 1981 is volume 2 of *Bohr's Collected Works*, here abbreviated as BCW II (other volumes of the *Collected Works* are abbreviated similarly).

[3] A reception study based solely on citation data from ISI Web of Science would be a catastrophe. Although this data base includes sources back to 1900, a search for Bohr's papers in *Philosophical Magazine* gives no result!

attention. Section 4.2 looks at the lack of response from J. J. Thomson, the established master of atom-building, while Section 4.3 is devoted to the opposition against Bohr’s theory from J. W. Nicholson and other British physicists of a more classical and conservative attitude. Response from American scientists, limited as it was, is dealt with in section 4.4, which also touches on the relationship between Bohr’s theory and the chemists. German physicists were for a while rather sceptical, but by the spring of 1914 Bohr’s ideas were well known and discussed, primarily as a theory related to spectroscopy. Although the theory was favourably received, it also attracted some criticism, both for empirical and conceptual reasons (Section 5.2). As argued in Section 5.3, the discovery of the Stark effect was a major reason why German physicists began taking the Bohr atom serious, although Stark himself resisted it.

Sommerfeld was somewhat slow in taking up atomic theory à la Bohr, but when it happened it was with great consequences. By the end of 1915, when Sommerfeld presented his first extensions of Bohr’s theory to the Bavarian Academy of Sciences, it was well established and about to transform into a more general and even more powerful quantum theory of the atom. My survey of the reception history stops here, although in a few cases I shall refer to scientific literature dating from later years.

2. Content and character of Bohr’s trilogy

The term ”Bohr’s atomic theory of 1913” may be understood in at least two different ways. It is often conceived in a rather narrow way, essentially referring to his model of simple atoms governed by the quantum postulates of stationary states and the mechanism for emission and absorption of light by transitions from one quantum state to another. This is the historical legacy of the theory, what proved to be viable and of central importance in the construction of our present picture of the atom built on quantum mechanics. A century later, Bohr's planetary atom is still a favoured model that appears in physics and chemistry textbooks at high school level. However, this was not necessarily how scientists conceived the theory in the first years after its publication, and it was at any rate only one part of the much larger system that Bohr laid out in his trilogy.

I shall here take a more historical view and speak of Bohr's theory as basically what was presented in his three papers from the summer and fall of 1913. It is important to recall that Bohr's three papers, comprising a total of 71 pages in *Philosophical Magazine*, carried the common title "On the Constitution of Atoms and Molecules," thus indicating that the work was meant to be more than just a new physical theory of the structure of atoms. Molecules – and thereby chemistry – was an important part of Bohr's very ambitious theory of matter, which also included aspects of radioactivity, X-rays, magnetism and more. It was this broad spectrum of subjects contemporary readers were faced with, and they gave different priorities to the subjects. Consequently they did not always agree what Bohr's theory was really about. In particular, while some physicists paid much attention to the theoretical basis in the quantum postulates, others chose to ignore this part and focus on the relationship between predictions and experiments.

The first part of Bohr's trilogy, subtitled "Binding of Electrons by Positive Nuclei," appeared in the July issue of *Philosophical Magazine* and was dated 5 April 1913.[4] This part, today considered by far the most important of the three papers, introduced the Bohr-Rutherford model and the quantum rules for atomic

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[4] Bohr 1913b, followed by Bohr 1913c and Bohr 1913d. The three papers are reproduced in BCW II and also in Rosenfeld 1963. Bohr 1913b can be found online as http://web.ihep.su/dbserv/compas/src/bohr13/eng.pdf.

structure and emission of light, after which Bohr used the rules in constructing his famous model of the hydrogen atom. The model of one-electron atoms brilliantly explained and generalized the Balmer spectrum, reduced the empirical Rydberg constant to a combination of more fundamental constants (namely $R = 2\pi^2 me^4/h^3c$), and it also offered a resolution of the puzzle of the so-called Pickering-Fowler lines. In addition, Bohr proposed that the angular momentum was quantized, he reproduced Einstein's law for the photoelectric effect, and he explained experiments by the American physicist Robert W. Wood on the absorption of light by sodium vapour.

The second part of the sequel, which appeared in September, concerned "Systems Containing Only a Single Nucleus." It focused on atomic systems with several electrons arranged in one or more rings, including calculations of the mechanical stability of such ring systems. For atomic systems with up to 24 electrons Bohr sketched a promising explanation of the periodic system that furthermore indicated a qualitative explanation of the variation of atomic volumes that had been known for long. He suggested a mechanism for the production of the characteristic lines in the X-ray spectrum, and at the end of the paper he addressed questions of isotopy and radioactivity, arguing that the beta rays had their origin in the nucleus rather than the surrounding electronic system.

Appearing in the November issue of *Philosophical Magazine*, the last part of the trilogy was mostly devoted to the structure of molecules. Bohr suggested that the covalent bond was constituted by a system of electrons revolving on a ring common to two nuclei, and on this basis he calculated the energy and dimensions of the hydrogen molecule as well as its heat of formation. Moreover, he concluded that the hydrogen molecule would dissociate into atomic hydrogen rather than hydrogen ions (H^- and H^+), in agreement with experiments. Bohr also considered molecular systems with a greater number of electrons, proposing configurations for hydrogen chloride (HCl), water (H_2O) and the tetrahedrically formed methane (CH_4). His theory of molecules and many-electron atoms was basically classical, relying only superficially on the quantum rules laid out in Part I. Bohr ended his trilogy by restating the basic assumptions of his theory such as he had introduced them in Part I. Summarizing his theory, he said:

> It is shown that, applying these assumptions to Rutherford's atom model, it is possible to account for the laws of Balmer and Rydberg connecting the frequency of the different lines in the line-spectrum of an element. Further, outlines are given of a theory of the constitution of the atoms of the elements and of the formation of molecules of chemical combinations, which on several points is shown to be in approximate agreement with experiments.[5]

3. The earliest responses

A few reactions to Bohr's atomic theory appeared even before the publication of Part I of the trilogy.[6] Of particular interest is a letter of 20 March 1913 in which Rutherford commented on Bohr's manuscript for the *Philosophical Magazine*. Apart from some minor criticism, Rutherford complained that it was "very difficult to form a physical idea" of the basis of Bohr's theory, a complaint that would soon be repeated by other British physicists. More specifically, Rutherford referred to what he called "one grave difficulty," namely this: "How does an electron decide what frequency it is going to vibrate at when it passes from one stationary state to the other? It seems to me that you would have to assume that

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[5] Bohr 1913d, p. 875.

[6] For these reactions, see Rosenfeld 1963, Hoyer 1974 and BCW II.

the electron knows beforehand where it is going to stop."[7] Rutherford instinctly sensed the element of acausality associated with Bohr's atom, a feature which would only move to the forefront of discussion several years later.

Rutherford's uneasiness was shared by Paul Ehrenfest, who in a letter to H. A. Lorentz of 25 August 1913 expressed his immediate reaction to Bohr's theory of the atom in this way: "Bohr's work on the quantum theory of the Balmer formula (in the *Phil. Mag*.), has driven me to despair. If this is the way to reach the goal, I must give up doing physics."[8] Ehrenfest was thoroughly familiar with quantum theory, but Bohr's way of applying quantum concepts to atomic structure puzzled him. It did not appeal to him at all, and it took several years until he came to accept Bohr's approach. As late as in the spring of 1916 he thought of the Bohr atomic model as "completely monstrous."[9]

Arnold Sommerfeld in Munich was among the physicists to whom Bohr sent preprints of his paper in the July issue of *Philosophical Magazine*. At the time he received the preprint, Sommerfeld had already read the paper, as he told Bohr in a postcard of 4 September: "I thank you very much for sending me your highly interesting work, which I have already studied in Phil. Mag. The problem of expressing the Rydberg-Ritz constant by Planck's *h* has for a long time been on my mind," he wrote. "Though for the present I am still rather sceptical about atomic models in general, calculating this constant is undoubtedly a great feat. … From Mr. Rutherford, whom I hope to see in October, I may perhaps learn more

[7] Rutherford to Bohr, 20 March 1913, in BCW II, p. 583.
[8] Ehrenfest to Lorentz, 25 August 1913, as quoted in Klein 1970, p. 278.
[9] Ehrenfest to Sommerfeld, April-May 1916, in Eckert and Märker 2000, p. 555. The German phrase is "ganz kanibalischem."

details about your plans."[10] Sommerfeld's scepticism did not evaporate instantly, and only at the end of 1914 did he seriously engage himself with the new atomic theory which he soon extended in a most fruitful way (see Section 5.3).

On the same date that Sommerfeld wrote his postcard there appeared in *Nature* what is possibly the first reference to Bohr's atomic theory in a scientific publication. Evan J. Evans, a member of Rutherford's group in Manchester, had for some time done experiments on the spectra of hydrogen and helium, a line of work that was directly inspired by Bohr's ideas as Evans knew them from Rutherford. To put it briefly, the so-called Pickering-Fowler spectral lines were thought to be due to hydrogen, but in that case their wavelengths contradicted Bohr's theory. Bohr had therefore argued that the line of wavelength 4686 Å and the other few lines could be reproduced by his theory on the assumption that they were caused by ionized helium (He^+) rather than neutral hydrogen atoms.[11] At the time Bohr made the suggestion there was no direct experimental evidence for it, and it was such evidence that Evans reported in *Nature*. Evans found the same 4686 line as Alfred Fowler had reported, but in pure hydrogen with no trace of helium: "For some time I have been investigating the origin of the 4686 line, and the experiments already carried out support Bohr's theory."[12]

Fowler was not quite convinced and in a subsequent letter to *Nature* he raised various objections, suggesting that "Dr. Bohr's theory (*Phil. Mag*., July,

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[10] BCW II, p. 603. The reference to the meeting in October was to the second Solvay conference in Brussels, where Sommerfeld and Rutherford were among the invited physicists.

[11] Bohr 1913b, pp. 10-11. For details, see Maier 1964, pp. 476-486, Robotti 1983 and Hoyer 1974, pp. 168-173.

[12] Evans 1913, dated 11 August. Evans mentioned Bohr and his theory but without referring to his paper in *Philosophical Magazine*. The identification of the Pickering-Fowler 4686 line with helium (but not with He^+) was also made by J. Stark in 1914, see below in Section 5.2.

1913) does not at present seem to give much evidence for helium, in preference to hydrogen, as the origin of the lines in question."[13] Only after Bohr had modified his own analysis by taking into account the finite mass of the nucleus – that is, had replaced in his formula the electron's mass with the reduced electron-proton mass – did Fowler concede that Bohr's atomic theory gave a correct explanation. As he wrote to Bohr, "Your letter published in last week's 'Nature' struck me as a valuable addition to your Phil. Mag. paper of July."[14] Although this was not the last word in the case of Fowler's lines, there is no doubt that the dispute did much to highlight Bohr's theory and make it known at an early date. In Fowler's Bakerian Lecture, delivered on 2 April 1914, Bohr's theory appeared prominently. While Fowler fully recognized Bohr's explanation of the 4686 line as due to "proto-helium," he maintained that "The assignment of the '4686' series to proto-helium may nevertheless be considered to be independent of Bohr's theory."[15]

As Bohr had argued in 1913, if his theory of Fowler's lines were correct one should also expect another helium series of lines very close to the ordinary hydrogen spectrum. In late 1914 Evans elaborated Bohr's argument, saying that "The presence of the lines … would greatly strengthen the experimental evidence in favour of Bohr's theory, but their absence would immediately show that the

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13 Fowler 1913.

14 Bohr 1913e. Fowler to Bohr, 27 October 1913 (BCW II, p. 503). Fowler publicly admitted the agreement in a note accompanying Bohr's paper (pp. 232-233), though not without pointing out that "Dr. Bohr's theory has not yet been shown to be capable of explaining the ordinary series of helium lines." The helium spectrum remained a problem for Bohr's theory and was only understood after the old quantum theory had been replaced by the new quantum mechanics of Heisenberg and others.

15 Fowler 1914, p. 258.

theory was incorrect."[16] In what Evans thought of as an *experimentum crucis* he found helium lines in complete accordance with Bohr's prediction.

The 1913 meeting of the British Association of the Advancement of Science, which took place in Birmingham 10-17 September, provided an opportunity for Bohr to get his new theory of atoms and molecules on the scientific agenda. Rutherford had suggested to the organizers that Bohr should be invited to take part in the discussion on radiation,[17] but due to his new position in Copenhagen Bohr was uncertain about his possibility of going to Birmingham. Yet, realizing the importance of the meeting, in the last minute he decided to attend it. Although he did not give a formal paper, he participated in some of the discussions and also gave a brief account of his theory on the 12th of September. According to the description in *Nature*:

> His [Bohr's] scheme for the hydrogen atom assumes several stationary states for the atom, and the passage from one state to another involves the yielding of one quantum. Dr. Bohr also emphasised the difficulty of Lorentz's scheme for distinguishing between matter and the radiator. … Prof. Lorentz intervened to ask how the Bohr atom was mechanically accounted for. Dr. Bohr acknowledged that this part of his theory was not complete, but the quantum theory being accepted, some sort of scheme of the kind suggested was necessary.[18]

Bohr's intervention took place in the discussion following James Jeans's exposition of the problems of radiation theory in which Jeans had given an

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[16] Evans 1915, dated December 1914. Bohr naturally greeted Evans's result (Bohr 1915a).
[17] See extract of letter from Rutherford to Bohr of 13 May 1913, in Hoyer 1974, p. 173.
[18] "Physics at the British Association," *Nature* 92 (1913), 304-309 (p. 306). Keller 1983, pp. 173-176 describes the Birmingham meeting and Bohr's role in it.

account of Bohr's "most ingenious and suggestive, and I think we must add convincing, explanation of the laws of spectral series."[19] Bohr later recalled that Jeans's "lucid exposition was, in fact, the first public expression of serious interest in considerations [Bohr's theory] which outside the Manchester group were generally received with much scepticism."[20]

Although Jeans found Bohr's theory convincing, he was less happy with its foundation in the two quantum postulates: "The only justification at present put forward for these assumptions is the very weighty one of success."[21] It was not only British physicists who became aware of Bohr's theory through Jeans's presentation. In a report on the Birmingham meeting in the *Physikalische Zeitschrift*, Paul Ewald included in full the part of Jeans's review in which he dealt with Bohr's theory.[22] He also gave a few more details on Bohr's critical remarks to Lorentz's notion of resonators and material particles. According to Ewald's report, Bohr argued that the relationship between the two concepts could be understood on the basis of his new model of the atom: "The atom belongs to 'matter' when the electron moves in a stationary orbit round the positive nucleus; the atom is a 'resonator' at the time of transition from one orbit to another, that is, at the time it radiates."[23]

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[19] Jeans 1913, p. 379.

[20] Bohr, Rutherford Memorial Lecture 1958, in BCW X, pp. 383-415, on p. 393.

[21] Jeans 1913, p. 379. While favourably impressed, Jeans realized the problems of Bohr's theory of which he mentioned "the difficulties of explaining the Zeeman effect and interference." That Jeans did not follow Bohr all the way is illustrated by his suggestion of a "dynamical interpretation" of h, corresponding to a value of the inverse fine-structure constant of $16\pi^2$. This kind of classical interpretation, so foreign to Bohr's approach, did not appear in Jeans 1914.

[22] Ewald 1913, pp. 1298-1299. Ewald's report was complementary to and in some respects more detailed than the one in *Nature*.

[23] Ewald 1913, p. 1301.

Jeans spoke even more positively, and in greater detail, about Bohr's theory in his influential report on radiation and quantum theory that appeared the following year. The new quantum theory of atoms, as exposed in the "very remarkable and intensely interesting Papers by Dr. Bohr, of Copenhagen," appeared prominently in the report. As Jeans phrased it, Bohr's fundamental assumption "is not inconsistent with the quantum-theory and is closely related to it."[24] Although Jeans expressed some reservation with respect to the applicability of Bohr's theory to more complex atoms, he praised it for having opened a rich field by the use of quantum theory to problems of atomic structure. Moreover, he showed, more clearly and in greater detail than Bohr had done, that the photoelectric effect as interpreted by Einstein "is now seen to be a necessary logical extension of Bohr's theory of absorption."[25]

Bohr's presence at the Birmingham meeting was noted by *The Times*, which on 13 September referred to Jeans's praise of the young Danish physicist and his new theory of the hydrogen spectrum.[26] Other speakers at the radiation symposium included H. A. Lorentz, Ernst Pringsheim, Augustus Love and Joseph Larmor, but none of them seems to have referred to Bohr's theory in their presentations. On the other hand, in his presidential address Oliver Lodge called attention to the "very remarkable" agreement between the observed spectrum lines of hydrogen and those calculated on the basis of Bohr's theory. "Quantitative applications of Planck's theory, to elucidate the otherwise shaky stability of the astronomically constituted atom, have been made," he said. "One

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[24] Jeans 1914, p. 51. The main section on Bohr's theory appeared on pp. 50-57.
[25] Ibid., p. 64.
[26] Pais 1986, p. 209 and Hoyer 1974, p. 174.

of the latest contributions to this subject is a paper by Dr. Bohr in the 'Philosophical Magazine' for July this year."[27]

In another part of the meeting Samuel B. McLaren, a professor of mathematics at University College, London, referred briefly to Bohr in connection with the theory of magnetism.[28] Shortly after the Birmingham meeting he called attention to Bohr's use of Planck's constant as a measure of the angular momentum of revolving electrons, suggesting that Bohr's theory gave support to his own idea of an elementary magnetic quantity or "magneton." According to McLaren, "Bohr's postulate of a natural unit of angular momentum was very prominent" at the meeting of the British Association. "By making Planck's constant *h* an angular momentum, Dr. Bohr has introduced an idea of the first importance," he said.[29] McLaren's theory of magnetons was part of an ambitious electrodynamical theory of gravity which in scope and spirit was entirely different from Bohr's ideas. He regarded the magneton as "an inner limiting surface of the æther, formed like an anchor-ring," a notion which belonged to a different framework of thought than the one of Bohr's theory.[30] Bohr found McLaren's theory interesting, but denied that it had any connection to his own quantum theory of atomic structure, such as he mentioned in a letter to Rutherford of 16 October 1913.[31] Another consideration of the relationship between the magneton and Planck's constant was offered by S. D. Chalmers, who suggested to replace Bohr's hypothesis of a unit angular momentum $L = h/2\pi$

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27 Lodge 1913, p. 17.

28 McLaren 1913c. Bohr knew McLaren, with whom he had had "a long and nice conversation" about electron theory in the fall of 1911. Bohr to Oseen, 1 December 1911, in BCW I, p. 427.

29 McLaren 1913b, and also McLaren 1913a appearing in the 9 October issue of *Nature*.

30 McLaren 1913a. For his electrodynamical magneton theory of gravity, see McLaren 1913d.

31 BCW II, p. 588.

with $L = h/\pi$.[32] Chalmers gave an account of his magneton atomic model at the session on radiation theory of the British Association in 1913.

The magneton as a unit magnetic moment had been introduced by the French physicist Pierre Weiss in 1911, and in the final part of the trilogy Bohr suggested "a close relation" between his atomic theory and Weiss's magneton.[33] However, the exact connection eluded him and after several attempts to calculate Weiss's value for the magneton he left the matter.[34] What is today known as the Bohr magneton was only introduced by Pauli in 1920.

John Nicholson of King's College, University of London, responded to McLaren's association of the magneton with the quantization of angular momentum by offering a brief evaluation of Bohr's theory and its relation to his own atomic theory. Although he was generally positive, Nicholson was not all that impressed by Bohr's theory and its recent success in explaining the Pickering-Fowler lines. "The real test of his theory will lie in its capacity to account for the *usual* spectrum of helium," he said, thus agreeing with Fowler's evaluation.[35] This was just an ouverture for the more extensive critique against Bohr's theory that Nicholson would soon launch (see Section 4.3).

Bohr also listened to the papers given by Francis Aston and J. J. Thomson on isotopes and the recently developed methods of isotope separation. While working with positive rays in 1912, Thomson had found evidence in a hydrogen discharge tube of particles with a value of the mass-charge ratio m/e three times that of a hydrogen atom. He argued that the mysterious "X_3" particles were

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[32] Chalmers 1914.

[33] Bohr 1913d, p. 875.

[34] For Bohr's calculations and drafts on magnetism between 1913 and 1915, see BCW II, pp. 253-265. For the history of the Bohr magneton, see Okada 2002.

[35] Nicholson 1913a.

triatomic hydrogen, H_3, an ozone form of ordinary molecular hydrogen.[36] In the discussion following Thomson's talk on "X_3 and the Evolution of Helium" Bohr suggested that X_3 might possibly be a superheavy isotope of hydrogen of atomic weight 3, that is, what was later called tritium. In a letter to Rutherford, George von Hevesy told that Bohr's proposal was badly formulated and not properly understood: "So I felt bound to stick up for Bohr and explained the meaning of Bohr's suggestion in more concrete terms, saying that Bohr's suggestion is that X_3 is possibly a chemically non-separable element from Hydrogen … Of course it is not very probable, but still a very interesting suggestion, which should not be quickly dismissed."[37]

Rutherford, who had closely followed the development of Bohr's ideas of atomic structure, was of course much in favour of the the new atomic theory which complemented and justified his own earlier theory of the nuclear atom. But Rutherford's research interest was radioactivity and the atomic nucleus, not the electron system with which Bohr's theory was primarily about, and his early explicit support of Bohr's theory was consequently limited to a few general remarks. On the other hand, he left no doubt about his high opinion of the theory. Thus, in a paper with John M. Nuttall in the October issue of *Philosophical Magazine* he referred to the hydrogen and helium models "assumed by Bohr in a

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[36] Thomson 1913b, pp. 116-122. See also Stark 1913.

[37] Letter of 14 October 1913, reproduced in Eve 1939, p. 224. Later experiments by William Duane, Arthur Dempster and others confirmed the existence of a heavy and reactive form of hydrogen. While H_3 does not seem to agree with Bohr's theory of valency, in 1919 Bohr argued that such an unstable configuration might well exist (Bohr 1919, reprinted in BCW II, 472-488). See also Wendt and Landauer 1920.

recent interesting paper on the constitution of atoms, and [which have] been shown by him to yield very promising results."[38]

Together with Rutherford, Henry Moseley was the most important of the early supporters of Bohr's atomic theory. His experimental results became the best advertisement for the Bohr-Rutherford nuclear atom. Moseley reported his first series of experiments on X-ray spectroscopy in a letter to Bohr of 16 November, saying that the results "lend great weight to the general principles which you use."[39] Bohr's new theory, he concluded his letter, "is having a splendid effect on Physics, and I believe that when we really know what an atom is, as we must within a few years, your theory even if wrong in detail will deserve much of the credit." When Moseley's paper appeared in the December issue of *Philosophical Magazine*, it included references to all of Bohr's three papers. The results, he said, "strongly support the views of Rutherford and of Bohr." He emphasized that they amounted to an experimental verification of "the principle of the constancy of angular momentum which was first used by Nicholson, and is the basis of Bohr's theory of the atom."[40]

As Moseley saw it, there were three competing atomic models that incorporated Planck's quantum of action, namely Thomson's, Nicholson's and Bohr's. Of these he much favoured the latter, as he told Rutherford in early 1914: "I feel myself convinced that what I have called the *h* hypothesis is true, that is to say one will ne able to build atoms out of *e*, *m* and *h* and nothing else besides. Of the 3 varieties of this hypothesis now going Bohr's has far and away the most to

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[38] Rutherford and Nuttall 1913, p. 712. A similar reference appeared in Rutherford 1913. For Rutherford's appreciation of Bohr's theory – positive but not enthusiastic – see Wilson 1983, pp. 318-338.
[39] See Heilbron 1974, pp. 211-213.
[40] Moseley 1913, p. 1025 and p. 1033.

recommend it."[41] On the other hand, Moseley was careful not to link his work too closely to the Bohr atom. In his famous paper of 1914, including the first version of the Moseley diagram, he did not refer to Bohr's theory, but only concluded "from the evidence of the X-ray spectra alone, without using any theory of atomic structure" that the chemical elements must be characterized by an integer, that is, the atomic number.[42]

The responses from German physicists were fewer and later than those of their British colleagues. Shortly after the meeting of the British Association in Birmingham a corresponding meeting of the German Association of Science and Medicine (Gesellschaft deutscher Naturforscher und Ärzte) took place in Vienna. Speakers included Einstein, James Franck, Max von Laue, Johannes Stark and Max Born. None of the published addresses referred to Bohr's theory, but there is little doubt that it was discussed informally.[43] Hevesy was present and in a letter written during the congress he told Bohr that he had just had a conversation with Einstein and told him that it was now certain that the Pickering-Fowler spectrum belonged to helium, in agreement with Bohr's theory. "When he heard this he was extremely astonished and told me: 'Than the frequency of light does not depand at all on the frequency of the electron' – (I understood him so??) And this is an *enormous achiewement*. The theory of Bohr must be then wright.' I can hardly tell you how pleased I have been and indeed hardly anything else could make

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[41] Moseley to Rutherford, 5 January 1914, in Heilbron 1974, p. 218.
[42] Moseley 1914b, p. 714.
[43] For the papers presented at the Vienna meeting, see *Physikalische Zeitschrift* 14 (1913), 1073-1180. See also Hermann and Benz 1972 according to whom "time was not yet ripe" for a discussion of Bohr's theory. The meeting took place 21-28 September. There is no documentation for Nancy Greenspan's claim that "Discussion at the meeting focused on the recent work of twenty-eight-year-old Danish physicist Niels Bohr on the quantum theory of the atom" (Greenspan 2005, p. 60).

me such a pleasure than this spontaneous judgement of Einstein."[44] A few weeks later, Hevesy reported his conversation with Einstein in a letter to Rutherford, essentially making the same point: "When I told him about the Fowler spectrum the big eyes of Einstein looked still bigger and he told me 'Then it is one of the greatest discoveries'."[45]

That Bohr's theory was known in Germany is also indicated by a letter of October in which Hans Geiger congratulated Bohr with his new work and asked for preprints for a colleague at the Physikalisch-Technische Reichanstalt in Berlin.[46] Later the same month, from October 27 to 31, the second Solvay conference on physics convened in Brussels. While the theme of the first conference had been radiation theory and quanta, the theme of the second was the structure of matter. Several of the talks dealt with atomic and molecular physics, but Bohr's new theory – which in a sense integrated the themes of the two conferences – was not mentioned in the published version of the talks and discussions.[47] Among the participants were Jeans, Sommerfeld, Einstein, Lorentz, J. J. Thomson and Rutherford, all of whom were acquainted with or at least knew about Bohr's quantum atom. While Thomson in his address on the structure of

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[44] Hevesy to Bohr, 23 September 1913, in BCW II, p. 531. See also Hevesy to Bohr, 6 August 1913, as reproduced in BCW II, pp. 531-532: "I look forward with very much interest to the result of your more elaborated calculations, so far everything is so clear, the behaviour of hydrogen and helium as described by the theorie, so truefull that nobody can avoid to be struck by reading it." The letters are in Hevesy's spelling.

[45] Hevesy to Rutherford, 14 October 1913, in Eve 1939, p. 226. That Einstein found Bohr's theory interesting and valuable is supported by Franz Tank's recollection of a colloquium in Zurich at about the same time (see Section 5.1). However, in spite of his sympathy for the theory, Einstein did not refer to it in his publications until 1916 (Section 5.3).

[46] Geiger to Bohr, 12 October 1913 (Heilbron 1964, p. 295). Bohr knew Geiger from his stay in Manchester.

[47] Goldschmidt, de Broglie and Lindemann 1921. See also Mehra 1975, pp. 75-94 and Marage and Wallenborn 1995, pp. 133-160.

atoms did refer to Bohr, it was not to his atomic theory but to an earlier work on the collision between charged particles and atomic electrons. Likewise, when Rutherford referred to the ideas of "van den Broek and Bohr," it was to the new notion of atomic number.[48]

4. The reception among English-speaking scientists

4.1 Positive receptions, but cautious

Already by the end of 1913, Bohr's theory of the structure of atoms was well known in the British physics community and widely appreciated as interesting and promising. Its solution of the problem of the Pickering-Fowler lines was an important factor, and so was the continuing support of Rutherford and Moseley. X-ray spectroscopy and related research provided evidence in favour of Bohr's theory, although (as we shall see below) this was a somewhat controversial question. George Shearer, a student of Charles Barkla in Edinburgh, examined in 1915 the ionization of hydrogen by X-rays, concluding that the X-ray data supported Bohr's theory: "If we extrapolate from experimental results on the K-radiations of the elements, we find that the K-radiation of hydrogen would have a wave-length of the order of magnitude of that which Bohr's theory suggests."[49]

Although advocating Bohr's theory, Rutherford was somewhat cautious and reluctant to comment on the central parts of the theory dealing with spectroscopy and the quantum postulates. In Rutherford's arguments for the advantages of the nuclear model over the Thomson model, Bohr's theory was not of primary importance. It merely supplemented and completed the nuclear model by turning it into a proper model of the atom as a whole.

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[48] Thomson 1921, p. 20 and p. 50. Bohr 1913a. On Thomson and Bohr's theory, see Section 4.2.

[49] Shearer 1915, p. 657.

In a paper of February 1914 Rutherford again referred to the theory, but mostly to Bohr's ideas of the nucleus and the nuclear origin of beta rays.[50] Only at the end of the paper did he refer to Bohr's atomic theory. There can be no doubt, he concluded, "that the theories of Bohr are of great interest and importance to all physicists as the first definite attempt to construct simple atoms and molecules and to explain their spectra."[51] In another paper from the same time he expressed the feeling of a growing number of physicists: "There no doubt will be much difference of opinion as to the validity of the assumptions made by Bohr in his theory of the constitution of atoms and molecules, but a very promising beginning has been made on the attack of this most fundamental of problems, which lies at the basis of Physics and Chemistry."[52]

Bohr's theory of the emission of light and X-rays from atoms was also favourably mentioned by the father-and-son scientists William Henry Bragg and William Lawrence Bragg. In one of their papers, published the same year they were awarded the Nobel Prize in physics, they referred to "the very remarkable and ingenious hypothesis [which] has lately been advanced by N. Bohr."[53] In a letter to his father, the younger Bragg told about his first meeting with Bohr, whom he had happened to have met after they had both attended a lecture by Jeans on radiation theory: "I got an awful lot from a Dane who had seen me asking Jeans questions, and after the lecture came up to me and talked over the

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[50] Rutherford 1914a. Also the Dutch amateur scientist Antonius Van den Broek, who was first to introduce the notion of atomic number, referred to Bohr's argument that beta rays have their origin in the atomic nucleus (Van den Broek 1914).
[51] Rutherford 1914a, p. 498, and similarly in Rutherford 1914b and in a Royal Society meeting of 19 March 1914 on "Discussion on the Structure of the Atom" (see Heilbron 1974, p. 105 and Wilson 1983, p. 338).
[52] Rutherford 1914b, p. 351.
[53] Bragg and Bragg 1915, p. 82.

whole thing. He was awfully sound on it, and most interesting, his name was Böhr, or something that sounds like it."[54]

The main content of Bohr's sequel of papers in *Philosophical Magazine* was disseminated to wider circles of English-speaking scientists through abstracts appearing in *Science Abstracts*, the abstract journal issued by the Institution of Electrical Engineers, and also in the abstract section of the *Journal of the Chemical Society*. The first two of Bohr's papers were extensively abstracted in *Science Abstracts* by George De Tunzelmann, a London physicist and engineer, who summarized Bohr's theory as follows: "The author's primary aim is to show that the introduction of Planck's constant, the elementary quantum of action, will serve, in Rutherford's model, to take the place of the radius of the positive sphere [in Thomson's model], and so make stability possible." Curiously, the third of the papers received only a single line abstract, saying that it "deals with systems containing several nuclei, on the same lines as in the earlier papers."[55] The detailed abstracts in the *Journal of the Chemical Society*, appearing in the section on "General and Physical Chemistry," emphasized the spectroscopic and chemical aspects of the theory, including its picture of the hydrogen molecule.[56]

Bohr's theory was of interest not only to physicists and chemists, but also, in its capacity of a theory of spectra, to astronomers. In a report on line spectra to the 94th meeting of the Royal Astronomical Society the theory appeared prominently. As it was noted, Bohr "has given a remarkable theory of the hydrogen spectrum, which has led to a considerable amount of discussion."[57]

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54 Undated letter quoted in Caroe 1978, p. 70. The meeting between Bohr and W. L. Bragg may have taken place during the British Association meeting in Birmingham.

55 Tunzelmann 1913 and 1914; Walter 1914.

56 Spencer 1913; Dawson 1913.

57 Fowler and Nicholson 1914, p. 359. Neither of the two authors was at the time happy about Bohr's atomic theory.

The quantum theory of the atom played only a very limited astronomical role at the time. Only from about 1920 did it move to the forefront of theoretical astrophysics.

Although chemists were reluctant to take up Bohr's ideas, many were aware of them. In papers of early 1914 on radioactive elements Hevesy referred to Bohr's argument that the radii of isotopic ions are the same.[58] The radiochemist Frederick Soddy, Rutherford's former collaborator and a co-inventor of the concept of isotopy (and a future Nobel laureate), introduced the Bohr-Rutherford atom to the chemists in his series of annual reports on radioactivity compiled at the request of the London Chemical Society. In the report for 1913, published in 1914, Soddy adopted the Bohr-Rutherford model, noting that the laws of electrodynamics did not apply to the interior of the atom. He said that "The model has been used with very considerable success, in conjunction with Planck's theory of quanta," leading to results "with the series relationships of the hydrogen and helium spectra, in striking accord with experimental determination."[59]

Although Bohr's work focused on electron configurations and spectroscopy, it also included aspects of radioactivity and nuclear physics. For example, at the 1915 meeting of the British Association, taking place in Manchester under the shadow of the war, Bohr participated in a discussion on "Radio-Active Elements and the Periodic Law" opened by Soddy. According to the summary account in *Nature*, "Dr. N. Bohr pointed out that … properties depending on the outer rings of electrons would be the same for all isotopes." He further predicted the existence of what became known as the isotope effect or

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58 Hevesy 1914a and 1914b, p. 600, which were almost identical.

59 Soddy 1914, p. 271, reprinted in Trenn 1985, p. 341. Soddy did not actually mention Bohr, but referred to the atomic model only by Rutherford's name. In his report for 1911 Soddy dealt in some detail with Nicholson's atomic model (Trenn 1985, pp. 255-258).

isotopic spectral shift: "In the case of spectral vibrations, there occurs a small term depending on the mass of the central nucleus, and accordingly we ought to look out for a small but perceptible difference between the spectra of two isotopes."[60] Other speakers in the section of mathematical and physical science included Soddy, Rutherford, Lindemann, Nicholson, Fowler, Richardson, Eddington and W. H. Bragg.

Owen W. Richardson, a Cavendish physicist who in 1906 was appointed professor of physics at Princeton University, was a specialist in electron theory and the emission of electrons from hot bodies. (In 1928 he would receive the Nobel Prize for his work in this area.) He was acquainted with Bohr's theory of atomic structure not only from the papers in *Philosophical Magazine* but also from a conversation he had had with Bohr in Cambridge in July 1913, just at the time when the theory appeared.[61] In a book of 1914 on electron theory based on a series of lectures given in Princeton, *Electron Theory of Matter*, Richardson included Bohr's new atomic theory, although in much less detail than he gave to the classical Thomson model. This was probably the earliest treatment of Bohr's theory in a regular textbook.

The book was positively reviewed by Bohr, who used the occasion to contrast the current development in atomic physics with the older but still surviving electromagnetic world view:

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[60] *Nature* 96 (1915), p. 240. Bohr prepared a paper on isotopes but for unknown reasons it remained a draft (BCW II, pp. 418-425). It is not generally known that Bohr predicted the isotope effect, which was only established experimentally in 1932 when Harold Urey, Ferdinand Brickwedde and George Murphy identified the heavy hydrogen isotope (deuterium) by detecting a slightly different wavelength for the spectral lines of hydrogen. See Brickwedde 1982.

[61] Niels Bohr to Harald Bohr, 30 July 1913, in BCW I, p. 563.

> In text-books only a few years old one finds great enthusiasm over what was called the future programme of the electromagnetic theory. It was believed that this theory constituted a final accomplishment of ordinary mechanics, and there appeared to be no limit to the application of the general principles of the theory. … If at present we may speak of a programme for the future development, it would, perhaps, be to examine the constitution of the special atomic systems actually existing, and then, by means of the directly observable properties of matter, possibly to deduce the general principles. If so, the evolution would be exactly the reverse of that anticipated.[62]

Richardson was impressed by the agreement of Bohr's theory with spectra, and noted that although Bohr's ideas "frankly discards dynamical principles" they were nonetheless successful and promising. There is no doubt, he said, "that this theory has been much more successful in accounting quantitatively for the numerical relationships between the frequencies of spectral lines than any other method of attack which has yet been tried." Moreover:

> Although the assumptions conflict with dynamical ideas they are of a very simple and elementary character. The fact that they conflict with dynamics does not appear to be a valid objection to them, as there are a number of other phenomena, the temperature radiation for example, which show that dynamics is inadequate as a basis for complete explanation of atomic behaviour.[63]

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[62] Review of Richardson 1914, in *Nature* 95 (1915), 420-421.
[63] Richardson 1914, p. 587. Preface dated May 1914.

In the second edition of 1916, prefaced 11 January 1916, he dealt in much more detail with Bohr's theory, if still presenting it as merely an alternative to the Thomson model. He did not mention Sommerfeld's recent elaboration, which was probably unknown to him because of the war. Richardson rated the theory highly and dealt in considerable detail not only with the hydrogen atom, but also with many-electron atoms, the H_2 molecule, X-ray spectra etc. Yet he also covered J. J. Thomson's earlier theory, and that in even greater detail, carefully avoiding to confront the two theories.[64] Having presented the two theories, he left it to the reader to decide between the two alternatives. Although Richardson clearly valued Bohr's theory, apparently he did not fully realize its non-classical features and its disagreement with the classical electron theory on which most of the book was based.[65]

In the 1916 edition of his book, Richardson referred to Bohr's model of the hydrogen molecule and his calculation of its heat of formation as given in the last part of the trilogy. Although Bohr got the right order of magnitude (~60 kcal/mole), his result did not agree convincingly with the American chemist Irving Langmuir's experimentally determined value of about 130 kcal/mole. In a paper appearing in the January 1914 issue of *Philosophical Magazine*, Langmuir, praising Bohr's "recent valuable and wonderfully suggestive paper," pointed out that the discrepancy was less drastic than thought.[66] New experiments indicated

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[64] Given that Thomson and other physicists had abandoned the original Thomson model of 1904 several years ago it is surprising that Richardson gave so much emphasis to it. He only dealt briefly with Thomson's recent view of the structure of atoms and chemical combination.

[65] See Knudsen 2001, according to whom Richardson "gave no indication that he saw a fundamental conflict between this [frequency] postulate and the many applications of Lorentz' electrodynamics in the electron theory of matter" (p. 248).

[66] Langmuir 1914, p. 188. See Kragh 1977 and Hoyer 1974, pp. 212-214. Langmuir informed Bohr of his new measurements in a letter of 2 December 1913 (BCW II, p. 539). Trained as a physical chemist and employed by General Electrics, Langmuir was

a value of about 78 kcal/mole, still too high but perhaps not damagingly so. Only after more elaborate and precise experiments did Langmuir conclude that "it now becomes impossible to reconcile our experiments with the value q = 63000 [cal/mole] calculated according to the method of Bohr."[67]

Richardson's *Electron Theory of Matter* may have been the first textbook that treated Bohr's theory, but it was not the first book that referred to it. George W. C. Kaye, a physicist at the National Physical Laboratory in London and a former collaborator of J. J. Thomson, published in early 1914 a book on X-rays and their use in which he included two references to Bohr's theory. Relegating Thomson's atomic theory to a footnote, he adopted the Bohr-Rutherford model according to which "The outer electrons, by their number and arrangement, are responsible for the chemical and physical properties of the atom: the inner electrons have influence only on the phenomena of radioactivity."[68] Kaye further mentioned Moseley's "important deductions … bearing on Rutherford's and Bohr's theories of the structure of the atom."[69]

Very few British physicists (if any) accepted Bohr's theory *in toto*, including the two quantum postulates. It was more common to use the theory eclectically, to accept parts of it while ignoring or rejecting other parts. As an example, consider Herbert Stanley Allen, a physicist af King's College, University of London, who in a series of works in 1914-1915 investigated theoretically the effect of a magnetic force arising from the nucleus of an atom of the Bohr-Rutherford type. Allen apparently supported Bohr's atomic model: "The success of Bohr's theory in explaining the ordinary Balmer's series in the spectrum of

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awarded the Nobel Prize in chemistry in 1932, the first corporate scientist to receive the honour.

[67] Langmuir 1915, p. 452, referring to Bohr 1913d.

[68] Kaye 1914, p. 18, preface dated February 1914.

[69] Ibid., p. 200.

hydrogen, and especially in obtaining close agreement between the observed and the calculated values of Rydberg's constant, raises a strong presumption in its favour."[70] However, the way he used parts of Bohr's theory to construct an atomic theory of his own was hardly in agreement with the views of Bohr. In reality Allen's atomic model differed markedly from the one proposed by Bohr, which he thought failed in the case of atoms more complex than hydrogen and therefore was in need of modification. Not only did Allen assume that the magnetic field of the nucleus played an active part in the emission of radiation, he also suggested a picture of the atomic nucleus that differed from and was much more complex than the one proposed by Rutherford.[71]

Allen suggested that his magnetic-core model of the atom received support from recent experiments by William E. Curtis, an astrophysicist and student of Fowler at the Imperial College, London. According to Curtis's precision measurements of the Balmer spectrum the wavelengths of the lines deviated slightly from those predicted by the Balmer-Bohr formula.[72] Bohr read Allen's paper with interest, but gently pointed out that "some of the deductions made by

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[70] Allen 1915c, p. 720. Other of Allen's papers that included references to and (sometimes unorthodox) use of Bohr's theory were Allen 1914, 1915a, and 1915b.

[71] In the case of heavy atoms Allen pictured the nucleus as a conglomerate of orbiting protons, alpha particles and beta particles that gave rise to an extranuclear magnetic field. He thought that the radius of the nucleus was much larger than the ~10^{-15} m found by Rutherford, perhaps 1000 times as large. Allen's model was only one of several speculative nuclear models proposed in the second half of the 1910s. For these, see Stuewer 1983.

[72] Curtis 1914, who referred to Bohr's theory and also to his still unpublished revision of the simple Balmer formula due to the electron's velocity. Some of the assumptions of Allen's explanation were criticized by the young Norwegian physicist Lars Vegard (1915), who a few years later would investigate the Bohr atom on the basis of X-ray data. In this early paper Vegard did not explicitly refer to Bohr's theory.

Dr. Allen are hard to justify."[73] Instead of "introducing new assumptions as to a complicated internal structure of the hydrogen nucleus," he suggested to explain the observed deviations from the Balmer formula by taking into regard the variation of the electron's mass with its velocity. Although Bohr did not quite succeed, this was the first attempt to explain the fine structure of the hydrogen spectrum on the basis of the Bohr-Rutherford atomic model. Only Sommerfeld's extension of 1916 solved the problem (Section 5.3).

Few British physicists realized how drastically Bohr's theory departed from conventional physics, for example that it denied the applicability of the principles of mechanics to systems of atomic dimensions. And many of those who did realize it, opposed the theory precisely for this reason. The Cavendish physicist Norman Campbell recognized more clearly than most the radical nature of Bohr's atomic model. "To attempt to explain Bohr's theory in terms of those principles [of classical physics] is useless," he pointed out in a review of January 1914.[74] Campbell praised the assumptions of the theory which he saw as "simple, plausible, and easily amenable to mathematical treatment; from them all the properties of any atomic system which does not contain more than one electron can be deduced uniquely." As to more complex atomic systems Campbell admitted that the power of the theory was limited, but instead of

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[73] Bohr 1915c, p. 332. In private he described Allen's paper in less gentle terms, namely as "a little foolish paper." Niels Bohr to Harald Bohr, 2 March 1915, in BCW I, p. 573. Bohr first made his suggestion of a mass correction due to the swiftly moving electron in a letter to Fowler of 15 April 1914. See the Bohr-Fowler correspondence in BCW II, pp. 504-507.

[74] Campbell 1914, p. 587. The second edition of Campbell's *Modern Electrical Theory* (Cambridge University Press) appeared in 1913 with a substantial chapter on the structure of atoms; however, the book was published too early to include Bohr's theory of atoms and molecules.

regarding it a serious flaw he thought it was "owing to the mathematical difficulties involved."[75]

Contrary to other British physicists, William Wilson at King's College, London, was interested in the formal quantum aspects of Bohr's theory, which he sought to present in a general way that covered also some of the results obtained by Planck. Wilson's theory, which he formulated as certain quantum conditions, "while formally distinct from Bohr's theory, leads to the same results when applied to the Rutherford type of atom in which an electron travels in a circular orbit round a positively charged nucleus."[76] Wilson rested content with having expressed Bohr's theory in a new way and did not apply his dynamical formulation to the calculation of atomic spectra or to other problems of physics.

Independently of Wilson, the Japanese physicist Jun Ishiwara presented in the spring of 1915 a paper with a proposal somewhat similar to the one of Wilson (and also to the one proposed a little later by Sommerfeld).[77] As Ishiwara demonstrated in his paper, his formulation of the quantum conditions yielded the results of Bohr's theory of atomic structure. His work may have been the first reference to Bohr's theory by a non-Western physicist. However, Ishiwara was to some extent influenced by Nicholson's view of atomic constitution and for this reason he assumed that the neutral hydrogen atom contained two electrons rather than one.

One of the more curious references to Bohr's theory came from the Englishman P. E. Shaw, who in a paper read to the Royal Society in late 1915

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[75] Campbell 1914, p. 587. Campbell prophecied, quite wrongly, that "theories of atomic structure will probably never be very interesting to chemists" because of the mathematical difficulties of calculations dealing with complex atoms and molecules. He could not foresee the later computational chemistry that emerged in the 1950s.

[76] Wilson 1915.

[77] Ishiwara 1915. For Ishiwara's work on the quantum atom, see Mehra and Rechenberg 1982, pp. 210-211 and Nishio 2000.

investigated a possible temperature variation of the constant of gravitation. He apparently thought that Bohr's view of the atom was relevant in this context as it "assumes that gravitation like radio-activity is unaffected by all physical and chemical agencies."[78] In fact, Bohr did not mention gravitation anywhere in his trilogy.

4.2 Thomson's silence

Still by 1913 J. J. Thomson, once a pioneer of electron and atomic physics, was considered the recognized authority in atomic structure and his ideas taken very seriously especially in the United Kingdom. His earlier "plumcake model" of the atom, which he had presented in quantitative details in 1904 and which in some respects inspired Bohr, was for a brief period of time the best offer of a theory of atomic structure.[79] However, at the time of Rutherford's announcement of the atomic nucleus it had been abandoned by Thomson himself and most other physicists, if not yet replaced by Rutherford's alternative conception of the atom. Interest in the nuclear atom was at first very limited, nearly absent. Even Rutherford, realizing that it was only half an atomic theory, did not press his new nuclear theory.[80]

Resisting quantum theory as well as the nuclear model, Thomson proposed a new model of the atom which had only few similarities with the old one. This was the model he presented to the British Association in September 1913 and in

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[78] Shaw 1916, p. 350.

[79] On Bohr's indebtedness to parts of the classical Thomson atom, see Heilbron 1977 and Heilbron 1981, who goes as far as concluding that "Bohr's atomic theory belongs to the program of semiliteral model making initiated by J. J. Thomson and based on the methods of mid-Victorian Cambridge physics" (Heilbron 1981, p. 230).

[80] The reception of Rutherford's atom is considered in Heilbron 1968, pp. 300-305. The indifference with which it was met contrasts with the reception of the Bohr atom.

even greater detail to the Solvay congress the following month.[81] A main feature of this second Thomson model was that the atom consisted of negative electrons bound together in stable equilibrium positions with positive particles in the form of hydrogen ions (protons) and alpha particles. The charged particles within the atom were assumed to be subject to two kinds of forces, a radial repulsive force varying inversely as the cube of the distance from the atomic centre and an inverse-square radial attractive force. Contrary to the ordinary Coulomb force, Thomson hypothesized that the attractive force was directive, namely, confined to a number of radial tubes in the atom.

Making use of these and other assumptions Thomson succeeded, to his own satisfaction, to reproduce Einstein's equation for the photoelectric effect, including Planck's constant which he characteristically expressed by atomic constants. Thomson found that $h^2 = \pi^2 Cem$, where C was a force constant of such a value that it secured the right value for h (e and m refer to the charge and mass of the electron). His model also provided an explanation of the production of X-rays and some of the data from X-ray spectroscopy. Moreover, Thomson and others applied models of this kind to throw light on the nature of valency and other chemical phenomena, which for a period made the model popular among chemists. According to Thomson, many chemical properties could be understood

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[81] Thomson 1913a; Thomson 1921. An extended English abstract of Thomson's Solvay lecture appears in Mehra 1975, pp. 77-81. According to the account in *Nature* (vol. 92, p. 305), Thomson's paper read in Birmingham was "a brilliant attempt to construct an atom which would account for some of the evidence for the quantum theory of energy… [and] it will be long before his illustration of the quantum theory by pin-pots is forgotten." In fact, it did not take long.

on his model as due to a dipole-dipole interaction caused by the mobility of the atomic electrons.[82]

Although Thomson's model of 1913 was very different from Bohr's, the two models addressed many of the same problems and were therefore, in a sense, rival conceptions of atomic structure. For example, Thomson found electron configurations for the simpler atoms that corresponded to the known periodicity of the elements, much like Bohr had done in the second part of his trilogy.[83] From this point of view it may be considered surprising that Thomson simply chose to ignore Bohr's theory, which he did not mention in any of his works of 1913 or the following years. Increasingly isolated from mainstream physics, he consistently kept to his classical picture of the atom, modifying it from time to time in ways which were conspicuously *ad hoc*. Only in 1919 did he confront Bohr's atom, which at the time enjoyed general acceptance among experts in atomic and quantum theory.

Thomson's late objections to the quantum atom were methodological rather than technical and presumably reflected his opinion when he first read Bohr's papers. Referring to Bohr's principle of discrete orbits or energy states characterized by quantum conditions, he said:

> This, however, is not the consequence of dynamical considerations; it is arithmetical rather than dynamical, and if it is true it must be the result of the action of forces whose existence has not been demonstrated. The investigation of such forces would be a problem of the highest interest and importance. By the use of this principle and a further one, that when an

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82 Thomson 1914. On the application of the Thomson atom to problems of chemistry, see Stranges 1982. These problems included the structure of the H_3 molecule (Thomson 1914, p. 783).

83 For early electron explanations of the periodic system, see Kragh 2001.

> electron passes from one orbit to another it gives out radiation whose frequency is proportional to the difference of the energy of the electron in the two orbits, Mr. Bohr obtains an expression which gives with quite remarkable accuracy the frequencies of the lines in the four-line spectrum of hydrogen. It is, I think, however, not unfair to say that to many minds the arithmetical basis of the theory seems much more satisfactory than the physical.[84]

Thomson further objected, as others had done, that "The vibrations which give rise to the spectrum do not on this theory correspond in frequency with any rotation or vibration in the atom when in the steady and normal state." According to Thomson there was convincing experimental evidence, especially based on absorption spectra, that an electron in an unexcited state of the atom vibrated with the frequencies of its spectral lines. In short, as he saw it, Bohr's quantum atom was a mathematical construct with no basis in established physics. He kept to this view throughout his life, although eventually admitting that Bohr's theory had "in some departments of spectroscopy changed chaos into order."[85]

Bohr was not impressed by Thomson's new model of the atom, but he realized that it could be seen as an alternative to his own and therefore contemplated a response. A month after the meeting of the British Association, and after Thomson's paper had been published in *Philosophical Magazine*, he wrote to Rutherford: "As to the theory of the structure of atoms of Sir J. J. Thomson, I did not realise in Birmingham how similar many of his results are to those I had obtained," adding that "this agreement has no foundation in the

[84] Thomson 1919, p. 420.
[85] Thomson 1936, p. 425.

special atom-model used by Thomson but will follow from any theory which considers electrons and nuclei and makes use of Planck's relation $E = h\nu$."[86] Bohr drafted a letter, apparently meant for *Nature*, in which he said as much, but did not send it.[87] In his letter to Rutherford of 16 October he elaborated his objections to Thomson's model as follows:

> Thus – quite apart from the fact that the assumption of repulsive forces varying inversely as the third power of the distance is in most striking disagreement with experiments on scattering of α–rays, – Thomson finds a value for the fundamental frequency of the hydrogen-atom which is 4 times too small, and a value for the ionization-potential of the hydrogen atom which is about half that experimentally found by himself. Besides Thomson's theory apparently gives no indication of an explanation of the laws of the line-spectra, and – making the atom a mechanical system – offers no possibility of evading the well-known difficulties of black-radiation and of specific heat.[88]

Rutherford was less diplomatic. In a letter to the American radiochemist Bertram Boltwood he characterized the Thomson atom as "only fitted for a museum of scientific curiosities."[89] To Arthur Schuster, at the time secretary of the Royal Society, he wrote: "I believe he [Thomson] knows in his heart that his own atom

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[86] Bohr to Rutherford, 16 October 1913, in BCW II, pp. 587-589.
[87] The unpublished letter is reproduced in BCW II, p. 268.
[88] BCW II, pp. 588-589.
[89] "J. J. T. … knows that I think his atom is only fitted for a museum of scientific curiosities. The idea of a nucleus atom is really working out exceedingly well. You will have seen the work of Bohr and Moseley." Rutherford to Boltwood, 17 March 1914, in Badash 1969, p. 292.

is not worth a damn and will not do the things it has got to do."[90] No conflict arose between Bohr and Thomson who largely cultivated their separate lines of work without bothering too much about the other's theory. Bohr was convinced that Thomson's theory belonged to the past, while his own belonged to the future.

Also another of the highly respected physics professors of the old guard, Joseph Larmor of Cambridge University, chose to ignore the Bohr atom. A celebrated pioneer of electron theory, Larmor had dealt extensively with atomic theory in his Wilde Lecture of 1908, but when the theories of Rutherford and Bohr appeared he remained silent. Only in 1928, in a postscript to a paper of 1921 on non-radiating atoms, did he briefly refer to the Bohr-Rutherford model of the atom.[91] The case of H. A. Lorentz is somewhat similar. Although he included a reference to Bohr's theory in the second edition of his *Theory of Electrons* of 1915, Lorentz showed no interest in the Bohr atom until several years later.[92] This was not an area of physics that appealed to him or where he felt home.

4.3. The British opposition

The British atom-building tradition in the style of Thomson did not collapse overnight with the advent of Bohr's new model of atomic structure. It continued for some years, in most cases with the atom builders devising models that incorporated limited features of quantum theory, as in the works of Nicholson and Thomson. Some of these classical models referred to and were inspired by,

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[90] Rutherford to Schuster, 2 February 1914, quoted in Wilson 1983, p. 338.

[91] Larmor 1929, pp. 344-372 ("The physical aspect of the atomic theory") and pp. 630-633 ("On non-radiating atoms").

[92] See Nersessian and Cohen 1987, which includes the second edition with the reference to Bohr on p. 107. Lorentz 1927, based on a lecture course of 1922, contained discussions of various aspects of the Bohr atom.

or were critical responses to, Bohr's theory. They were all short-lived. Only a few physicists, notably Nicholson and Lindemann, campaigned actively against Bohr and his supporters.

Arthur W. Conway, professor of mathematical physics at University College, Dublin, proposed in December 1913 an atomic model based on classical mechanics and electromagnetism with the aim of explaining – or rather illustrating – some of the properties of spectral series.[93] "The atom considered is a 'Thomson' atom rotating with a constant angular velocity," he said, and his chosen model was further modified in such a way that the positive sphere was capable of executing elastic vibrations. He found that "in every steady motion the angular momentum of the negative electron has the same constant value," which he identified with h/π or twice that obtained by Bohr. Conway's h was not really Planck's quantum constant but a quantity deduced from spectroscopy and his atomic model which happened to be very close to the quantum of action. His attempt to clarify the connection between Bohr's theory and his own – "two theories so very different from one another" – was unconvincing and revealed a lack of understanding of the meaning of Bohr's atomic theory.[94] In a note of 1914 Conway argued that his model, if supplied with certain assumptions, was able to reproduce Fowler's spectrum and thus provided an alternative to Bohr's explanation.[95] The implication was that Bohr's atom was not necessary.

Two months later, again in the pages of *Philosophical Magazine*, another and more elaborate atomic theory was proposed, this time by William Peddie, a

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[93] Conway 1913. A. W. Conway (1875-1950) had done important work in theoretical spectroscopy and also worked on mathematical formulations of electrodynamics and special relativity theory. See Whittaker 1951.

[94] For Conway's misunderstandings, see Heilbron 1964, pp. 299-301.

[95] Conway 1914.

professor of physics at St. Andrews in Dundee.[96] Peddie's atom was a "spherical counterpart of the tubular atom of Sir J. J. Thomson," consisting of a series of negatively charged shells surrounding a positive core and constructed in such a way as to give the desired results. Disregarding technical details, Peddie managed to obtain from his model atom Balmer's spectral formula, account for the law of photoelectricity, and come up with a qualitative explanation of radioactivity. His general idea was to derive optical and other phenomena from "a complicated structure of the atom itself" – and Peddie's spherical atom was indeed complicated. Bohr had deduced his results in a "beautifully direct manner," he said, but unfortunately in a way that could not be reconciled with the known laws of dynamics and electromagnetism. As Peddie saw it, for this reason the Bohr atom could not be a model of the real constitution of atoms. He spelled out his critique as follows:

> The value of the new ideas [of Bohr] as a working hypothesis cannot be denied. But behind all this procedure there lies the root question whether or not the peculiarities, so readily explained on the new ideas, cannot be explained in terms of the ideas of the older physics as consequences of structural conditions.[97]

Peddie thought this could be done: "It does not seem to me that we are yet under compulsion to forsake the laws of ordinary dynamics in connexion with atomic

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[96] Peddie 1914. W. Peddie (1861-1946) was a student and later an assistant of Peter G. Tait. In 1887 he was elected a Fellow of the Royal Society of Edinburgh and since 1907 he served as Professor of Physics at University College, Dundee, in the University of St. Andrews. His main work was in colour theory, molecular magnetism, and dynamics. See Smart 1947 for an obituary.

[97] Peddie 1914, p. 258.

properties, or the doctrine of a continuous wave-front in æther, or even, apart from magnetic action, the notion of central symmetry in atomic motion."[98]

As a third and last example of a classical alternative to the Bohr atom, consider a work by the American physicist and engineer Albert Cushing Crehore. Following Bohr's first paper on atomic theory in the July 1913 issue of *Philosophical Magazine* there appeared a 60-page long paper by Crehore on atomic and molecular structure.[99] The author adopted the classical atomic model of Thomson which he developed in different ways and extended into an elaborate theory of molecules, crystals and more. It is informative to compare Crehore's paper with the preceding one of Bohr – two theories dealing with the same subject matter, the structure of atoms and molecules, and yet so very different in both substance and method. By February 1915 Crehore had modified the Thomson model into a "corpuscular-ring gyroscopic theory," in part in an attempt to introduce Planck's constant and take into regard the works of Moseley and Bohr.

Noting that "The present tendency among atomic theorists is to favour with Rutherford an atom with a central positive nucleus having electrons circulating in orbits," Crehore devised a theory which eclectically included features of both the Thomson model and the Bohr-Rutherford model.[100] In what he thought was in agreement with Bohr, he assumed that undisturbed electrons

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[98] Ibid., p. 259.

[99] Crehore 1913. A. C. Crehore, a former assistant professor of physics and electrical engineering at Dartmouth College, was an independent scientist and inventor. Known as the inventor of a printing telegraph, he excelled in ambitious "theories of everything" that included an electromagnetic theory of gravitation (*New York Times*, 15 february 1912). In papers published in *Physical Review* and *Philosophical Magazine*, Crehore continued until the late 1920s to construct atomic models on an electromagnetic basis and to interprete quantum theory in terms of electrodynamics.

[100] Crehore 1915, p. 310.

describing circular orbits did not emit radiation. On the other hand, while on the Bohr-Rutherford model beta particles had their origin in the nucleus, in Crehore's theory they might come from any electron in the atom. The American physicist used his speculative theory to offer an alternative explanation of X-ray spectra, to account for photoelectricity, to suggest the existence of positive electrons, and to predict an upper limit of atomic weight corresponding to the weight of uranium. Borrowing a few features from Bohr's theory did not make him accept the theory: "Although Bohr has in a brilliant manner given an explanation of some of the series of spectral lines, notably those of H and He, yet it may fairly be said that luminous spectra have not been explained by any atomic theory." As evidence he cited Nicholson, who had "shown in a seemingly conclusive manner that these spectra are not really accounted for on Bohr's hypothesis."[101]

At the meeting of the British Association held in 1914 in Australia, Bohr's theory was discussed in a joint meeting in Melbourne of section A (mathematics and physics) and section B (chemistry). While Rutherford did not mention Bohr's ideas, they were critically addressed by William M. Hicks and John W. Nicholson. Hicks, who had studied under Maxwell and in 1883 advanced to a professorship at Firth College in Sheffield, had been a leading proponent of the vortex theory of atoms, a research programme which in a general sense continued to appeal to him (as it did to J. J. Thomson).[102] His view of atomic theory may be illustrated by his praise of Conway's recent and "most suggestive" paper offering an electrodynamic explanation of the origin of

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[101] Ibid., p. 324. On Nicholson, see below.
[102] On Hicks, see Milner 1935.

spectra. "We want more of a similar nature," he said.[103] That Hicks was not in the vantguard of physics is further illustrated by his dismissal of the Bohr-Rutherford picture of helium as the element of atomic number 2. He argued that the atomic number was more probably 4, implying the existence of at least one unknown element between hydrogen and helium.[104]

Admitting that Planck's constant had a role to play in atomic theory, Hicks discussed the theory of Bohr – or "Böhr" as he was spelled in the proceedings of the British Association – which he praised for its "ingenuity and great suggestiveness." However, ingenious and suggestive as it was, he dismissed it on both methodological and empirical grounds. As to the latter, he objected that it was valid for hydrogen only and thus not really a theory of atoms and spectra. Although the theory had "caught the scientific imagination," it failed to offer a true explanation, meaning a mechanism for the emission of light. "It is based on the Rutherford atom, but throws no further light on the structure of the atom itself, as the mechanism of radiation is totally unexplained, and it is this which we are in search of."[105] Hick's had more confidence in the rival atomic theory of

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[103] Hicks 1914c, p. 298. Walker 1915 might be an example of what Hicks wanted more of. Without referring to Bohr's theory, George Walker examined an atomic model similar to the one of Conway and from electrodynamical calculations he obtained series formulae of the Balmer type. He thought that Conway's theory was "most important."

[104] See also Hicks 1914b, where he appealed to the periodic system proposed by the Swedish spectroscopist Johannes (Janne) Rydberg. From the point of view of the Bohr-Rutherford theory, two intermediary elements were impossible, but this is nonetheless what Rydberg and a few other scientists (including Hicks and Nicholson) held. Rydberg argued that the ordinals of elements were two units greater than the atomic numbers adopted by Moseley and Bohr. Thus, in the first group there should be four elements rather than just hydrogen and helium, lithium should be element number 5, etc. See Rydberg 1914.

[105] Hicks 1914c, p. 299. See also Heilbron 1974, pp. 114-115. Disgarding the Bohr atom, Hick's continued investigating the origin of spectra in great mathematical and numerical details. For an example, see Hicks 1914a, a paper of more than 90 pages.

Nicholson which he thought was generally correct and "stands alone as a first satisfactory theory of one type of spectra."

Bohr could afford to ignore the alternatives and objections of scientists like Conway, Peddie, Hicks and Crehore, whose ideas were so clearly out of tune with mainstream physics. The opposition of Nicholson was a different matter, for not only had Nicholson proposed a kind of quantum atomic model before Bohr, his views also enjoyed considerable respect among British physicists and astronomers. For example, Jeans's report on radiation and quanta of 1914 included not only an account of Bohr's theory of the structure of atoms but also of Nicholson's theory. Only in the case of Nicholson did Bohr become involved in something that was close to a controversy over atomic structure. However, the disagreement did not evolve into a proper controversy.

Nicholson's model of 1911 – proposed the same year as Rutherford's nuclear atom – consisted of a tiny centre of positive electricity around which electrons revolved in rings.[106] It was mainly concerned with problems of speculative astrochemistry, including primary atoms and the constitution of hypothetical elements (which he named "coronium," "protofluorine" and "nebulium"). In order to explain the line spectra, including those found by astronomers and not known from the laboratory, in papers of 1912 he made use of Planck's constant, suggesting that the angular momentum of simple atoms was quantized according to $L = nh/2\pi$ ($n = 1, 2, 3,\ldots$). The similarity to the Bohr-Rutherford atom was to some extent apparent only, for Nicholson held that an atom needed to have more than a single orbital electron. His hydrogen atom

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[106] For details on Nicholson and his atomic theory, see McCormmach 1966 and Maier 1965, pp. 448-461. J. W. Nicholson (1881-1955) was lecturer at the Cavendish Laboratory until 1912, when he was appointed Professor of Mathematics in the University of London, King's College. He was elected a Fellow of the Royal Society in 1917. For biography, see Wilson 1956.

carried three electrons around the nucleus and he argued that the only four-electron atom was the hypothetical nebulium.

Bohr had first met Nicholson in Cambridge in late 1911, at a time when both were interested in the electron theory of metals. Nicholson had written a paper on the subject which Bohr found to be "perfectly crazy," as he told in a letter to his Swedish friend Carl Oseen. "I have also had a discussion with Nicholson; he was extremely kind, but with him I shall hardly be able to agree about very much."[107] At that time Bohr was unaware of Nicholson's atomic theory, which he first referred to in a postcard about a year later, emphasizing that Nicholson's ideas of the structure of atoms were incompatible with his own ideas.[108] Well aware of Nicholson's ring model and eager to distance his own model from it, Bohr referred extensively to it in the first part of his trilogy. He emphasized that his own theory rested on a very different basis. "In Nicholson's calculations the frequency of lines in a line-spectrum is identified with the frequency of vibration of a mechanical system in a distinctly indicated state of equilibrium," Bohr wrote.[109] He further pointed out that Nicholson's theory, contrary to his own, was unable to account for the spectral regularities of Balmer and Rydberg.

Recognizing the threat from Bohr's rival theory of atoms, Nicholson responded critically to it in a series of papers from 1913 to about 1917. His aim was not primarily to defend his own model, but rather to demonstrate irreparable weaknesses in Bohr's theory by examining it from its own premises, or what he thought was its premises. Contrary to most other critics, he had a deep knowledge of Bohr's theory, which he developed into great details, often

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107 Bohr to Oseen, 1 December 1911, in BCW I, p. 427. See also Heilbron and Kuhn 1969, p. 258.

108 Postcard to Harald Bohr, 23 December 1912, in BCW I, p. 563.

109 Bohr 1913b, p. 7.

greater than those considered by Bohr himself. At times he indicated that his own theory and Bohr's were not necessarily in conflict and might perhaps both be valid descriptions – complementary in some sense . "The two theories give the same constitution for the atom of hydrogen," he claimed, "except that the dynamical one [Nicholson's] is somewhat more specific."[110] As mentioned above, Nicholson first responded to Bohr's theory in a note of 16 October 1913 in which he pointed out that the theory was apparently unable to acount for the spectrum of ordinary helium. He repeated his criticism at the discussion meeting in Melbourne, where he said that in order to go further than hydrogen, "we must abandon at least one of Böhr's premises which is vital to the deduction of the hydrogen formula."[111]

Rather than going through all of Nicholson's many comments and arguments, I shall only mention some of his main objections which I group together in four classes:

(*a*) According to Nicholson's analysis, two or more coplanar rings of electrons could not exist, neither on the view of the dynamical theory nor on Bohr's theory. Either the electrons must move in different planes, or they must all lie on the same circle. This implied that Bohr's explanation of Moseley's results of X-rays was necessarily incorrect. "If Bohr's theory is to remain," he said (calling it "so attractive that its retention is desirable") – "we must give up the idea of concentric rings in the atom, with X-radiation coming from an inner ring."[112] As Nicholson concluded in another paper of 1914, "Moseley's observations have

[110] Nicholson 1914e, p. 487.
[111] Nicholson 1914f, p. 300.
[112] Nicholson 1914a, p. 583.

shown no relation to Bohr's theory."[113] Of course, Bohr disagreed and so did Moseley.

(*b*) It was a recurrent theme in Nicholson's criticism that what he called "van den Broek's hypothesis" of the atomic number was in conflict with Bohr's theory. Since Bohr's theory for atoms more complex than helium was founded on the notion that the nuclear charge was the ordinal number for the periodic system, this was a serious charge. "If we are to retain Bohr's theory of such complex atoms, that theory must give up van den Broek's hypothesis in its present form."[114] Nicholson further argued that Bohr's theory of valency and the structure of complex atoms led to results that were grossly inconsistent with chemical knowledge. For example, lithium should be an inert element and nitrogen a divalent metal. So much for Bohr's chemistry!

(*c*) Since Nicholson had concluded that Bohr atoms could only have a single ring, Bohr's model of lithium, as he had presented it in the second part of his trilogy, had to be wrong: "It is not possible for three electrons and a nucleus to form a lithium atom with a unit valency, after the manner of Bohr's model."[115] In early 1913 Nicholson had suggested that lines in the spectra of certain stars were due to a new hydrogen series, but according to Bohr they were really due to doubly ionized lithium, Li^{2+}, just as the Pickering-Fowler lines had their origin in He^{+}.[116] If Bohr's lithium model was wrong, so was his reinterpretation of the stellar lines.

(*d*) Bohr's theory was singularly successful when applied to the simplest elements, hydrogen and ionized helium, but according to Nicholson the success was only partially deserved. In detailed analyses he concluded that except for the

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[113] Nicholson 1914d, p. 564.
[114] Nicholson 1914d, p. 543 and also Nicholson 1914b.
[115] Nicholson 1914g, and see also Nicholson 1914b and 1914d.
[116] Bohr 1913c, pp. 490-491; Nicholson 1913b.

neutral hydrogen atom the model failed even for simple systems such as He^+, H_2, He and H^-. Nicholson admitted that "the theory is definitely successful when there is only one electron, – and also, at the same time, when there is only one nucleus," but for all other atomic and molecular systems "it rests on a slender foundation."[117] Of what worth was an atomic theory which was valid only for a single element? The theory, he said, must "stand or fall according to its capacity to take account more completely of the spectra of these two elements," namely hydrogen and helium. Especially with regard to helium he was convinced that Bohr's theory failed to live up to its promises. Having investigated various ways to generalize and modify the theory so as to explain the helium spectra, "we must conclude that it cannot develop in the manner which its earlier success appeared to foreshadow."[118]

Some of Nicholson's objections to Bohr's theory, and especially as they related to X-ray spectroscopy, were independently argued by Frederick A. Lindemann, the later Viscount Cherwell.[119] While the general view was that Moseley's data provided strong support for Bohr's theory, Lindemann argued that this was not the case and that the data merely supported the hypothesis of an atomic number as suggested by Van den Broek and Rutherford. "The agreement of Bohr's constant with experimental data is not convincing to my mind in view of the

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[117] Nicholson 1914c, p. 441, and see also Nicholson 1915 and 1914g.
[118] Nicholson 1914g, p. 103. Similarly at the discussion in Melbourne: "The balance of experimental evidence is against Böhr's theory at present" (Nicholson 1914f, p. 300).
[119] F. A. Lindemann (1866-1957) was born and trained in Germany, where he started his scientific career as a protégé of Nernst, doing research on the new quantum theory. He acted as a co-secretary to the first Solvay congress in 1911 and much later became a controversial scientific adviser to Churchill during World War II.

large number of arbitrary assumptions in his derivation."[120] By means of elaborate dimensional analyses he suggested that there were many ways in which results equivalent to Bohr's could obtained, including some that avoided reference to quantum theory. Lindemann denied that experiments, whether in the X-ray or the optical region, provided unambiguous support for "Dr. Bohr's special assumptions." Bohr immediately penned a brief reply in which he criticized the procedure adopted by Lindemann, and also Moseley responded, repeating that his experiments did confirm Bohr's theory.[121]

The objections raised by Nicholson was a more serious matter and Bohr intended to reply to them. He drafted a letter to *Nature* and a longer one to *Philosophical Magazine*, but none of them were mailed. Although "I admit most readily the importance of the difficulties discussed by Prof. Nicholson," he wrote in the longer reply, "I cannot, on the other hand, feel convinced that the basis for his calculations is sufficiently self-contained to justify his conclusions."[122] Bohr's replies came in the form of two papers of 1915, the first on the hydrogen and helium spectra and the second a general development of his theory of atoms and radiation. "I am unable to agree with Nicholson's conclusions," he stated, apparently unwilling to face these conclusions in details.[123] He did however take care to repudiate the argument of Nicholson that the 4686 line and the new series discovered by Evans were no evidence for Bohr's theory as they might well be due to hydrogen rather than ionized helium. Bohr concluded that "at present there is scarcely sufficient theoretical evidence to justify us in disregarding the

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[120] Lindemann 1914a, p. 501 and also Lindemann 1914b. For the details of Lindemann's arguments, published in the transactions of the German Physical Society, see Lindemann 1914c.

[121] Bohr 1914b; Moseley 1914a. See also Hoyer 1974, pp. 196-202.

[122] The two draft letters are reproduced in BCW II, pp. 270-271 and pp. 312-316.

[123] Bohr 1915b, p. 399. The earlier paper was Bohr 1915a.

direct evidence as to the chemical origin of the lines given by Evans's experiments."[124]

There might be no "theoretical evidence" to doubt that the 4686 line was due to ionized helium, but at Imperial College doubts remained as to the empirical evidence. In a paper from March 1915 the spectroscopist Thomas Merton observed that Bohr's theory "has given rise to a considerable amount of theoretical discussion."[125] Spectroscopic experiments based on a new interference method suggested to him that the evidence provided by Evans was inconclusive and that the mass of the atom from which the 4686 line originated was much smaller than that of the helium atom. He found that it was only about one-tenth of the mass of a hydrogen atom and thus "due to systems of subatomic mass." What these systems might be, Merton did not say. Nor did he spell out the theoretical significance of his conclusion, although it obviously contradicted Bohr's explanation. Further work on the 4686 line focused on its complex structure which in 1916 came to be seen as evidence for Sommerfeld's relativistic extension of Bohr's model.[126]

It seems that Bohr convinced himself that it was not worth entering a dispute with Nicholson, whose premises and way of thinking differed too much from his own to make it worthwhile. "His whole point of view is so foreign to me," he wrote to Oseen in September 1914, adding that "by a departure from

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[124] Bohr 1915a, p. 7, a reply to Nicholson 1915. Bohr's reply to *Nature* was first returned and only appeared after "Rutherford took care of it in a hurry." See letter to Harald Bohr of 2 March 1915, in BCW I, p. 573.

[125] Merton 1915b, p. 383, with a preliminary announcement in Merton 1915a. Bohr 1915b responded briefly to Merton's experiments, for which he suggested a different explanation.

[126] Evans and Croxton 1916; Paschen 1916. The interpretation in favour of the Bohr-Sommerfeld model was questioned by some physicists, as detailed in Kragh 1985 and Robotti 1986.

mechanics I understand something much more radical than he does."[127] To Hans M. Hansen, his friend and colleague in Copenhagen, Bohr expressed himself in a similar way: "You have probably seen quite a bit of criticism, which has appeared; especially from Nicholson. I do not think it has any foundation. I feel that Nicholson treats the question not as a physical, but as a purely literary one."[128] He soon came to see the critique from Nicholson, Lindemann and others as insignificant and not worth worrying about. "I don't think that any of it means anything," he said in a letter to his brother Harald.[129]

4.4 Limited American interest

While Bohr's theory made a very considerable impact on physics in the United Kingdom, it was received later and with less interest by American scientists. Some of those who did mention the theory in the first years after 1913 (such as Langmuir and Crehore) published in British scientific journals. As mentioned, Richardson's textbook, which included an introduction to Bohr's theory, was based on a course at Princeton University. Although the theory was undoubtedly known by many American physicists, it did not make an impression in *Physical Review*, since 1913 the journal of the American Physical Society. The structure of atoms was not what occupied the minds of most American physicists, the large majority of whom worked on experimental rather than theoretical subjects. Until the beginning of 1916, *Physical Review* contained no papers on or references to Bohr's theory of atomic structure and almost no papers that can be classified as atomic theory.[130]

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[127] Bohr to Oseen, 28 September 1914, in BCW II, p. 562.
[128] Bohr to Hansen, 12 May 1915, in BCW II, pp. 517-518.
[129] Niels Bohr to Harald Bohr, 15 April 1915, in BCW I, p. 579.
[130] The only sign of interest was a symposium of the American Physical Society of 27 September 1914 on "Spectroscopic Evidence Regarding Atomic Structure" which

Several of the papers in *Physical Review* and *Astrophysical Journal* dealt with spectroscopy, an area of research which was of equal interest to physicists and astronomers and which American researchers cultivated as actively as their colleagues in Europe. Indeed, astrospectroscopy was something of an American specialty. In a study of the 4686 line and other lines in the spectra of planetary nebulae, William Wright at the Lick Observatory referred to the role of the 4686 line in "certain theories of the constitution of the atom." He singled out "the interesting theory of radiation proposed by Bohr … [which] predicts lines separated by about two angstroms from the members of the Balmer series."[131]

In 1914 the experienced spectroscopist Theodore Lyman at the Jefferson Laboratory of Harvard University reported the observation of two new hydrogen lines in the ultraviolet region.[132] The lines had been suspected by Bohr in his 1913 generalization of Balmer's formula, but the American physicist made no mention of Bohr and may at the time have been unaware of his theory. Even after Bohr had become aware of the lines and referred to "the series in the ultra-violet recently discovered by Lyman" as further confirmation of his theory,[133] Lyman refrained from considering the theoretical relevance of his discovery.

Whereas Lyman did not refer to atomic theory in 1914, in papers of 1915 and 1916 he briefly mentioned that "The relations of the spectra of hydrogen and helium have recently come into prominence through the theoretical researches of

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included a paper on Nicholson's atomic theory. Bohr's theory may have been mentioned, but the papers of the symposium were not published. See *Physical Review* 5 (1914), 72. Speculative atomic theories were not foreign to the Americans. Apart from Crehore's model, in 1915 Alfred L. Parson published a "magneton theory" of the atom which attracted some attention among chemists and will be considered below. Although not an American citizen, Parson worked in the United States and published his theory under the patronage of an American institution.

131 Wright 1915, p. 269, read to the National Academy of Sciences on 9 December 1914.

132 Lyman 1914.

133 Bohr 1915a.

Bohr, Nicholson and others."[134] In his paper in *Astrophysical Journal* he called the relations between the spectra of the two elements "a fascinating subject for speculation" and said: "In connection with Bohr's speculations it is important to observe that λ 1217, which forms the first member of the Ritz [Lyman] series, occupies exactly the same position when obtained from helium as when it is produced in hydrogen."[135] In none of his papers did he give credence to Bohr's formula and he ignored his atomic theory. He preferred to deal with experimental facts rather than "speculations."

Readers of *Nature* and *Philosophical Magazine*, which included many American physicists, could hardly avoid to come across Bohr's theory and the Bohr-Rutherford atom, subjects which also appeared in the pages of *Science*, the journal of the American Association of the Advancement of Science. For example, the July 1914 issue of the journal included a survey article by Arthur S. Eve based on a meeting of the Royal Society of Canada on the structure of the atom. Eve, a former assistant of Rutherford and since 1903 professor of physics in Montreal, presented the ideas of the "brilliant young Dane, Bohr" whose work "is remarkable as leading to excellent numerical verification."[136] He also referred to Bohr's models of water and other molecules, which, although "somewhat speculative," he found to be "refreshing."

Half a year later *Science* brought another survey article which praised the Bohr-Rutherford model of the atom as a great advance, even one that "will probably remain, suffering but little change in the future."[137] The author, G.

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134 Lyman 1915, p. 370 and Lyman 1916, p. 91. The second paper was an extended version of the first. Lyman was guided by a formula of Walther Ritz from 1908, not by Bohr's formula of 1913. For details on the works of Ritz and Lyman, see Konno 2002.
135 Lyman 1916, p. 100.
136 Eve 1914. The paper also appeared in the *Journal of the Franklin Institution* of 1915.
137 Stewart 1914.

Walter Stewart of the University of Iowa City, recognized the critique of Nicholson but did not find it damaging that Bohr's theory had difficulties with the more complex atoms. "When one contemplates the narrow scope of even this brilliant theory, what a limitless field for research seems ahead!" Another American physicist, Gordon Scott Fulcher of the University of Wisconsin, was less impressed by Bohr's theory which he, in agreement with Stark, thought was contradicted by Stark's series of experiments with canal rays and their spectra. According to Fulcher, "his [Bohr's] assumption that the series lines are emitted by the single rotating electron of the hydrogen neutral atom is directly contrary to Stark's experimental result." The correct interpretation of the experiments was rather that "The Balmer series is emitted by electrons in the nucleus, vibrating about positions of static rather than dynamic equilibrium."[138]

Although his first public comments on Bohr's theory only date from 1916, there is reason to mention the response of the eminent American chemist Gilbert Newton Lewis, who in 1912 had moved from a professorship in physical chemistry at Massachusetts Institute of Technology to become dean of the College of Chemistry at the University of California, Berkeley. Lewis had for long nourished an interest in atomic structure, but it took until 1916 before he published his ideas of what he called the "cubical atom."[139] It was essential to Lewis's cubic model that the electrons stayed in fixed positions and for this reason alone he had to deny the validity of Bohr's dynamic atom. Concerning Bohr's postulate that electrons moving in a stationary orbit produce no radiation

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[138] Fulcher 1915, p. 371. This was one of very few American research papers before 1916 which discussed Bohr's theory. See also Fulcher 1913, in which he critically assessed Rutherford's nuclear atom and defended Stark's view that the hydrogen series lines are emitted by singly charged hydrogen atoms. This paper was written shortly before the appearance of the Bohr atom. On Stark versus Bohr, see Section 5.2 below.

[139] Lewis' ideas went back to 1902. For a full historical account of his early conception of atoms and molecules, see Kohler 1971.

or other effect, he said: "Now this is not only inconsistent with the accepted laws of electromagnetics but, I may add, is logically objectionable, for the state of motion which produces no physical effect whatsoever may better be called a state of rest."[140] In spite of his critical attitude to Bohr's theory, Lewis was greatly interested in the ideas of the Danish physicist with whom he wanted to establish connections. In February 1916 he invited Bohr to come to Berkeley, but although Bohr was tempted to go nothing came of it.[141]

Lewis's theory of valence and atomic structure was to some degree stimulated by a work that the English chemist Alfred Lauck Parson published in a series issued by the Smithsonian Institution. After studies in Oxford, Parson moved to the United States where he worked at Harvard and Berkeley and came to know Lewis. In 1915 he published an ambitious and rather speculative "magneton theory" of the atom (not to be confused with McLaren's) which he applied to valency, affinity and a variety of other chemical problems. His work was much closer to the tradition of Thomson than the theory of Bohr, and he explicitly dismissed the Bohr-Rutherford atom as useless from a chemical point of view. "Bohr's theory, based upon the conceptions of the nuclear positive charge, gives a interesting treatment of the problem of spectrum series, but its chemical application is very meager indeed."[142] This was not an unfair

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140 Lewis 1916, p. 773. At a symposium on "The Structure of Matter" at the meeting of the American Association for the Advancement of Science held on 27 December 1916, Lewis amplified his critique of Bohr's theory, now adding the objection that on this theory the revolving electrons would continue their motion even down to the absolute zero of temperature. He apparently believed that at $T = 0$ all motion would cease, including the motion of intra-atomic particles. Lewis 1917.

141 Niels Bohr to Harald Bohr, 14 March 1916, in BCW I, p. 585. The index in BCW I refers to "E. P. Lewis", which is undoubtedly a mistake.

142 Parson 1915, p. 3. On Parson's theory, see Kohler 1971, pp. 364-370 and Stranges 1982, pp. 220-223.

characterization. Whether in the version of Thomson, Nicholson or Bohr, Parson argued that the ring atom "is experimentally shown to be untenable."[143]

Parson had obviously studied Bohr's papers, including the third and often ignored part of the trilogy, for he referred critically to Bohr's tentative model of the tetrahedral four-valence carbon atom. This part of Bohr's theory, admittedly tentative, failed to impress the chemists, and it certainly did not impress Parson: "We see there that the theory comes to a complete halt when confronted with the problems of 'Chemistry in Space'."[144] The young Englishman further referred approvingly to Nicholson's objections to Bohr's models of lithium and other complex atoms. Like Nicholson, he did not believe in the hypothesis of atomic numbers, which was an integral part of the Bohr-Rutherford atom. While Bohr's atom was necessarily dynamic, the chemists needed a static one, either in the version of Lewis (or, for that matter, Parson) or some other version. In spite of several attempts to reconcile the two kinds of model, the Bohr atom played only an insignificant role in chemistry and none in the crucial problem of the nature of the covalent bond.[145]

One of the very few chemists who reacted positively Bohr's theory was E. H. Buchner at the Chemical Laboratory of the University of Amsterdam, who received inspiration from Part II of Bohr's trilogy. In a paper of 1915 he suggested that Bohr's ideas of the electron configuration of the elements might explain some of the chemical analogies known from inorganic chemistry, such as the analogy between the ammonium ion and the alkali ions.[146]

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143 Parson 1915, p. 7.

144 Ibid., p. 11.

145 On the static and dynamic traditions in early atomic chemistry and physics, see Kragh 1985a and Nishio 1967.

146 Buchner 1915. Bohr drafted a letter of response to *Nature*, but did not send it (BCW II, p. 630).

By 1916 American scientists were warming up to adopt the new theory of the atom based on the works of Rutherford, Bohr and Moseley. Robert Millikan, recognized as the leading American physicist at the time, first referred to Bohr's theory in a couple of papers presented in December 1916. As president of the American Physical Society he delivered an address on the new physics of radiation and atoms in which he extolled the "extraordinary success of the Bohr atom."[147] According to Millikan's inductivist understanding of Bohr's theory, it was "guided solely by the known character of the line spectra of hydrogen and helium" and even the postulate of non-radiationg stationary orbits was "merely the statement of the existing *experimental* situation." The success of the theory, he said, was not least due to "its adaptability to the explanation of deviations from the behaviour predicted by its most elementary form," such as illustrated by the Fowler anomaly and Sommerfeld's recent explanation of the fine structure. Millikan was aware of the standard objection that Bohr's atomic theory "gives us no picture of the mechanism of the production of the frequency," but considered it a strength rather than a weakness. In this regard he likened the theory to the fundamental laws of thermodynamics, which "are true irrespective of a mechanism."

5. The German scene

5.1 The early reception in Germany

Bohr stayed most of the period 1912-1915 in England; he was well connected to several leading British physicists; he participated in two of the meetings of the British Association; and with one exception all of his papers appeared in British

[147] Millikan 1917a, p. 326. The other quotations are from the same paper.

journals, either *Nature* or *Philosophical Magazine*.[148] Moreover, his theory relied on and was closely related to works of British physicists, in particular Rutherford, Fowler, Barkla and Moseley. It is therefore natural that his theory of atoms and molecules attracted more and earlier attention in Britain than in Germany, the other of the major powers in physics at the time.

There most likely was another reason, namely that the atom-building tradition was strong and had long roots in the United Kingdom, in contrast to the situation in Germany where this kind of physics was not highly regarded. When Sommerfeld told Bohr in September 1913 that he was "rather sceptical about atomic models in general," he spoke for a majority of his German colleagues. The difference in attitude was described by Rutherford in a letter to W. H. Bragg of late 1911 in which he said about the first Solvay meeting:

> I was rather struck in Brussels by the fact that the continental people do not seem to be in the least interested to form a physical idea of the basis of Planck's theory. They are quite content to explain everything on a certain assumption, and do not worry their heads about the real cause of the thing. I must, I think, say that the English point of view is much more physical and much to be preferred.[149]

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148 The exception was a paper in *Fysisk Tidsskrift* based on an address to the Physical Society in Copenhagen on 20 December 1913. The paper appeared in an English translation in 1922 (BCW II, pp. 283-301).

149 Eve 1939, p. 208. Nishio (1973, p. 56) observes that at the time Bohr proposed his atomic theory, "in Germany problems concerning the real structure of the atom received little interest."

Although Bohr did not attempt to "form a physical idea of the basis of Planck's theory," his theory clearly belonged to the English tradition and not the continental one.

In the fall of 1913 the mathematician Harald Bohr, the two years younger brother of Niels, stayed in Göttingen where he met and cooperated with Richard Courant, Constantin Carathéodory, David Hilbert, Hermann Weyl and other leading mathematicians. He wrote back to his brother, then in Copenhagen, that "People here are still exceedingly interested in your papers, but I have the impression that most of them – except Hilbert, however – and in particular, among the youngest, Born, Madelung, etc., do not dare to believe that they can be objectively right; they found the assumptions too 'bold' and 'fantastic'."[150] The somewhat reserved attitude is confirmed by later recollections. Thus, Max Born recalled that Bohr's papers of 1913 "made a deep impression on us and were thoroughly discussed," but also that "the whole atmosphere of the physics department in Göttingen was, in spite of Debye, not favourable to such revolutionary ideas."[151] Yet, latest by the fall of 1914 Born had become "an ardent follower of Bohr," preparing a report on the stability of the Bohr atom for Hilbert's seminar in the winter semester 1914-1915.[152] The famous mathematician met with Bohr, but according to Courant "Hilbert could not learn anything from Niels Bohr – it was a problem in itself to communicate mutually with Niels

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150 Harald Bohr to Niels Bohr, undated but most likely October 1913, in BCW I, p. 567. Harald further wrote: "There are so many of the younger people here that have asked me for reprints of your papers; if you send me, e.g., 2 of the first and 3 of the second (if you have enough of them) I could give them to some who really will study them." Although Hilbert would later describe Bohr as the "Newton of atomic theory" and lecture on the quantum theory of atoms and spectra, at the time he did not take up Bohr's theory. On Hilbert's characterization of Bohr, see Sauer and Majer 2009, p. 509.

151 Born 1978, p. 157.

152 Ibid. On Born's report, see Greenspan 2005, p. 66.

Bohr."[153] Only later did Hilbert become seriously interested in Bohr's atomic theory, on which he gave lectures in the early 1920s.

In an interview of 1962 Courant said that the reception in Göttingen was cool and that the eminent spectroscopist and mathematician Carl Runge was particularly antagonistic:

> Carl Runge was between physics and mathematics. He was the great spectroscopist. He knew more about the spectra than anybody else. … Then Niels came with his model. And I remember that Runge was completely upset. He said, "Well, such a nice man, and so intelligent. But this man has become completely crazy. This is the shearest nonsense." It was a violent criticism and opposition.[154]

In another of Courant's recollections he said that "The reception in Göttingen was cool and sceptical" and that Runge characterized the first part of Bohr's trilogy as "a strange if not crazy stunt."[155] On the other hand, Courant, who had first met Bohr in Cambridge in 1913, found at once Bohr's theory convincing. He later wrote to Niels Bohr that "When I reported these things here in Göttingen, they laughed at me that I should not take such fantasies seriously."[156]

Although Runge conceded that Bohr's theory agreed surprisingly well with spectroscopic data, he considered it to be nothing more than a collection of rules for calculation. It provided no understanding of either atoms or radiation.

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[153] Courant 1981, p. 162.
[154] Interview with R. Courant, 9 May 1962, by Thomas S. Kuhn and M. Kac. Niels Bohr Library & Archives. http://www.aip.org/history/ohilist/4562.html.
[155] Courant 1967, p. 302. On the reaction to Bohr's talk in Göttingen, see also Alfred Landé's recollections of 1962, as quoted in Pais 1991, p. 155.
[156] Reid 1976, p. 45.

Runge's dislike of Bohr's theory of spectra did not vanish easily. In a letter of September 1916 Sommerfeld wrote him that he "had the impression that you are still somewhat foreign to Bohr's theory." But Sommerfeld assured him that there was no reason for scepticism: "One can no longer doubt the absolute correctness of this theory."[157]

In Zurich, another of the German-speaking centres of physics, Bohr's atomic model was discussed at a colloquium in the fall of 1913. According to Franz Tank, who attended the colloquium, Max von Laue objected vehemently to the theory: "That's all nonsense; Maxwell's equations are correct under all circumstances, and an electron orbiting around a positive nucleus is bound to radiate." Tank further recalled that Einstein, in opposition to von Laue, declared his support to Bohr's model: "Very remarkable – there must then be something behind it; I do not believe that the derivation of the absolute value of the Rydberg constant is purely fortuitous."[158] Einstein referred to Bohr's identification $R = 2\pi^2 me^4/h^3c$, which some physicists considered significant and impressive whereas critics tended to see it as a piece of numerology.

Only in the summer of 1914 did Bohr meet with German physicists, namely when he gave talks in Göttingen and Munich in front of Born, Debye, Wien, Sommerfeld and others. "I had never met any German physicists before and had much pleasure in talking with them," he wrote in a letter after his return to Denmark – just in time to avoid the complications caused by the war. "I gave a couple of small talks in the seminars in Göttingen and Munich and had many lively discussions. I especially enjoyed talking with Wien and hearing about

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157 Sommerfeld to Runge, 6 September 1916, in Eckert and Märker 2000, p. 566. See also the nice description in McCormmach 1982, pp. 73-75.
158 Letter of 11 March 1964 to Max Jammer, as quoted in Jammer 1966, p. 86.

some experiments going on in his institute."[159] At the Rydberg Centennial Conference held in Lund, Sweden, in 1954 Bohr confirmed what Courant said about Runge's antagonism. Referring to the explanation of the Pickering-Fowler lines in terms of Bohr's theory, he said: "I especially recall the warning, given by the latter [Runge] at a colloquium in Göttingen, against such apparently arbitrary use of spectral evidence by theoreticians who did not seem properly to appreciate the beauty and harmony of the general pattern of series spectra, revealed about all by the ingenuity of Rydberg."[160]

Whether through formal or informal channels, latest by the spring of 1914 Bohr's works were well known and discussed in German-speaking Europe. The Austrian physicist Arthur E. Haas, who in a work of 1910 as the first one had introduced Planck's constant in the architecture of atoms, had studied Bohr's papers with great interest. He wrote him that "I … shall at the Physical Society of Leipzig this very January render an account of your papers, which of course will also meet with great interest there."[161] At the same time Bohr's theory and applications of it began to appear frequently and prominently in the programme of the Munich physics colloquia. On 26 January 1914 Paul Epstein reported on Bohr's publications on atomic theory, and four months later Sommerfeld and his former student Wilhelm Lenz discussed Bohr's new work on the Stark effect.[162] As mentioned, on 15 July Bohr was himself a *Mittwoch* colloquium speaker in Munich.

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[159] Bohr to Oseen, 28 September 1914, in BCW II, p. 557.
[160] Bohr 1955, reprinted in BCW X, pp. 371-379 (p. 378).
[161] Haas to Bohr, 6 January 1914, in BSC II, p. 513. On Haas and his early atomic model, see Hermann 1965.
[162] Nishio 1973, pp. 59-60. On the Munich *Mittwoch* colloquia 1913-1915, see also Eckert and Märker 2000, pp. 434-439.

The trilogy was extensively abstracted in the *Beiblätter* of the *Annalen der Physik*, although the abstracts only appeared collectively in 1914 and then placed under the optics section.[163] Bohr's theory was also abstracted in the *Chemische Central-Blatt*, but without paying attention to its relevance for problems of chemistry. The *Beiblätter* reviewer was Rudolf Seeliger, a young physicist who had taken his doctorate under Sommerfeld and at the time worked at the Physikalisch-Technische Reichanstalt in Berlin. Seeliger stressed the axiomatic structure of Bohr's theory and its success in explaining the Balmer and Pickering-Fowler series, and also that it was a modification of Rutherford's nuclear model. "In their last consequences," he said, "the postulates of Bohr go beyond the assumptions of quantum theory, and they also have rather little connection to the former views of physics; on the other hand, the great heuristic value of Bohr's considerations cannot be belied."

In a detailed and generally positive review in *Naturwissenschaften* of March 1914, Seeliger mentioned the objections of Nicholson, Lindemann and others. "One can reasonably ask the question if the postulates of the theory are the only possible ones … and if these and the associated deductions are really consistent." Without answering the question he concluded: "Even though we may be sceptical with respect to the details, I think we have in Bohr's considerations an important and fundamental advance in the knowledge of the origin of spectral lines and series."[164] Another review, even more detailed and positive, appeared the following year in *Physikalische Zeitschrift*, where Eduard Riecke, the 70-year old professor of experimental physics in Göttingen, gave particular attention to the spectroscopic evidence in favour of Bohr's theory. "The further development of science may still change much in Bohr's theory, yet it is certain that it has

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163 Seeliger 1914a.
164 Seeliger 1914b, p. 313.

already lead to highly valuable information and is of fundamental importance in the area of spectroscopy."[165] Not only did the spectra of hydrogen and helium agree beautifully with the theory, Riecke also concluded that its importance was of a more general and fundamental kind and not limited to the field of spectroscopy. The positive reviews of Seeliger and Riecke were instrumental in disseminating Bohr's theory among German physicists.

5.2 Objections and developments

The reception of Bohr's atomic theory in the German physical community at the summer of 1915, two years after it was introduced, may be judged from the volume on physics that under the editorship of Emil Warburg was published in the book series *Die Kultur der Gegenwart*.[166] The volume, with contributions from leading German and German-speaking physicists, gave a general overview of the state of physics aimed at a general audience. It is evident from the content of the book that atomic structure was not seen as a subject of high priority. Bohr's theory entered briefly in the chapters on spectrum analysis and magneto-optics, written by Franz Exner and Pieter Zeeman, respectively, but only alongside Thomson's model which was given more attention than Bohr's. None of the chapters dealt with the structure of the atom. Bohr's theory was only given more than brief notice in Wilhelm Wien's chapter on heat radiation, where he emphasized the remarkable reproduction of Balmer's formula that followed from the theory. But Wien, who a decade earlier had helped to pioneer the electromagnetic world view, also pointed out that "the theory is not yet self-

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[165] Riecke 1915, p. 222. Riecke died the same year, and the paper was published posthumously.
[166] Warburg 1915.

consistent and it contradicts the electromagnetic theory by assuming that the revolving electrons do not emit energy."[167]

Wien's objection that Bohr's model of the atom contradicted the well established theory of electromagnetism was common at the time. It was, of course, a feature that Bohr was well aware of and which was a central postulate in his introduction of stationary states. The contradiction was built into the theory from the very beginning. Oseen, Bohr's Swedish friend and colleague, raised the question in a letter to Bohr of 11 November 1913 in which he congratulated Bohr with his second paper. Now Bohr had developed his theory "beyond the region of hypotheses and theories and into that of truth itself." Praise apart, Oseen was curious to know "how the Maxwell-Lorentz theory should be modified to allow for the existence of an atom of your type."[168] Oseen (contrary to Bohr) continued to worry about the problem, and in a detailed analysis in *Physikalische Zeitschrift* he reached the following, unequivocal conclusion: "Bohr's atom model can in no way be reconciled with the fundamental assumptions of Lorentz's electron theory. We have to make our choice between these two theories. One of them may be correct, but not both of them."[169] Although greatly attracted by the electron theory based on the Maxwell-Lorentz equations, Oseen refrained from concluding that Bohr's model was in serious trouble. He was careful to stress that his paper should not be considered a "a work of controversy against the theory of Bohr."

The concern expressed by Oseen and others, that Bohr's theory was incompatible with the laws of electrodynamics, was also raised by another

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[167] Wien 1915, p. 222.

[168] Oseen to Bohr, 11 November 1913, in BCW II, p. 553. Bohr was not interested in the question of a modification of the Maxwell-Lorentz equations.

[169] Oseen 1915, p. 404. Stark used Oseen's analysis as ammunition in his critique of the Bohr atom (Stark 1916, p. 76).

Scandinavian physicist, the Norwegian Thorstein Wereide. In a book of 1915, entitled *Statistical Theory of Energy and Matter*, he included a summary account of Bohr's "recent and surprising researches" concerning atomic structure and the mechanism of light emission. However, Wereide did not accept either Bohr's assumptions or other aspects of the non-classical quantum discontinuity. On the contrary, he argued that "the quanta may exclusively be considered as invented mathematical quantities that seem to exist because they lead to a true result."[170] Manipulations of the Maxwell-Lorentz theory made him claim that he had reproduced Bohr's stationarity postulate on a classical basis, namely that an electron moving in a circular orbit would not emit radiation (he thought that elliptic orbits would be unstable).[171] His calculations, published in *Annalen der Physik* at a time when the Bohr-Sommerfeld theory was widely accepted in Germany, were ignored by most physicists.

Although German physicists may have been "exceedingly interested" in Bohr's papers, such as Harald Bohr said in his letter from Göttingen, for a while the interest did not materialize in scientific papers related to the new theory. Most readers of the 1913-1915 volumes of *Annalen der Physik*, the main journal of the German physics community and at the time coedited by Wien and Planck, would not perceive that a new revolution in atomic physics was under way or otherwise come across Bohr and his theory. The journal contained very few papers on quantum and atomic theory, and none at all that dealt with Bohr's new theory of the structure of atoms. The few articles on atomic theory were speculative and quite different from Bohr's in both style and content.[172] Only a

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[170] Wereide 1915, p. 162. On Wereide and his critique of the quantum atom, see Kragh 2006.

[171] Wereide 1917.

[172] For example, Byk 1913 introduced a speculative and ambitious atomic theory vaguely related to the Thomson atom. The theory made use of quantum theory and also,

couple of papers carried references to Bohr's 1913 papers, among them a paper on the spectrum of He^+ written by Jens Koch, a Swedish physicist who worked under Stark at the Technische Hochschule in Aachen.[173]

Another of the few *Annalen* papers that referred to Bohr's theory, and that at a relatively early date, appeared in January 1914 and was written by Hans Marius Hansen, a young Danish physicist and close friend of Bohr. In a long experimental paper on the inverse Zeeman effect Hansen pointed out that some of the spectroscopic details had probably escaped explanation because of the inadequate knowledge of atomic structure. In this context he referred in a note to "the very important results which N. Bohr has obtained from Rutherford's atomic model."[174]

The near absence of Bohr from the pages of *Annalen der Physik* did not imply a lack of interest from German physicists, as there were other outlets for publication, for example the proceedings (*Verhandlungen* or *Berichte*) of the German Physical Society. Thus, it was in the *Verhandlungen* that Emil Warburg in December 1913 published what was probably the first German research paper relating to Bohr's theory (see the following section). The next time Bohr's theory appeared in the publications of the society was in May 1914, when Stark reported experiments that confirmed that the 4686 line belonged to the helium spectrum.[175] However, Stark did not see the identification as a confirmation of Bohr's model of atomic structure. On the contrary, concluding that the line was due to the *doubly* charged helium ion (He^{2+}) he implicitly denied the validity of

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and remarkably, applied non-Euclidean geometry to atomic architecture. According to Byk, his theory had wide applications to molecular and chemical phenomena.

173 Koch 1915.

174 Hansen 1914, p. 235. The paper, a condensed version of Hansen's doctoral dissertation at the University of Copenhagen, was dated 31 October 1913 and appeared in the issue of 23 January 1914.

175 Stark 1914a.

the Bohr-Rutherford conception of the atom. He would soon do the same explicitly. On the other hand, to Friedrich Paschen in Tübingen the resolution of the puzzle of the hydrogen and helium lines was convincing evidence for Bohr's theory. "Only now do I see that Bohr's theory is exactly right," he wrote to Sommerfeld. "There is no doubt that Bohr's final formula is as accurate as the measurements can be made."[176]

In his Bakerian Lecture of April 1914, Fowler pointed out that "some of the conclusions drawn by Stark from his experiments on canal-ray spectra are inconsistent with the views of Bohr," and that "In the case of helium it does not seem possible to reconcile Stark's conclusions with those of Bohr."[177] However, the disagreement mentioned by Fowler did not refer to the electric effect, but to Stark's research on canal rays and the consequences for atomic constitution that Stark drew from them. Stark's view on the structure of atoms was indeed irreconcilable with Bohr's theory and the Bohr-Rutherford model. Concentrating on his extensive experimental work and pursuing his own line of research, Stark showed little interest in the theoretical discussions concerning Bohr's theory, but he did intervene in the discussion.[178] In a book of 1914 in which he summarized his work on "electric spectral analysis" he criticized Bohr's theory in general and confronted it with the canal-ray results and the Stark effect in particular. Although admitting that Bohr's recent theory of the electric effect agreed with some of his experiments, he argued that the agreement was apparent only and in any case annulled by serious disagreements with other of the experimental results:

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176 Paschen to Sommerfeld, 24 February 1915, in Eckert and Märker 2000, p. 500.
177 Fowler 1914, pp. 260-261 and similarly in Fulcher 1915, p. 371.
178 Mehra and Rechenberg (1982, p. 202) write that Stark "stayed away from the discussion," but this is not quite correct.

> The mentioned quantitative agreement between Bohr's theory and observations loses completely its significance in regard of the fact that the theory is unable to reproduce correctly, and not even qualitatively, essential features of the electric splitting of the lines of the H-series. … Bohr's theory is unacceptable, and that alone for the reason that there is an unsolvable contradiction between it and the observations regarding the splitting of consecutive series terms.[179]

In an extensive paper of 1916 Stark confronted Bohr's theory of the hydrogen atom with his own, very different model according to which the Balmer spectrum was caused by the H^+ ion and not a neutral hydrogen atom (Starks's H^+ contained several electrons in motion). He proudly emphasized that his model agreed with "Newtonian dynamics and Maxwell's theory … [and] it makes no use of the quantum hypothesis."[180] The following year Stark launched a frontal attack on the Bohr quantum atom, but at the time of the attack Bohr's theory was so well established that Bohr and most other physicists chose to ignore him.[181]

From a scientific point of view, the most important of the Bohr-related papers published in the *Verhandlungen* were two papers by Walther Kossel, a student of Lenard and Sommerfeld. Appearing in the fall of 1914 they offered a new and promising analysis of the absorption of X-rays. Kossel's second paper

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179 Stark 1914b (preface June 1914), p. 119.
180 Stark 1916, p. 56.
181 Stark 1917. Most of Stark's objections were experimental, but he also argued that Bohr's theory violated ordinary causality as it allowed events to be influenced by later events. In his Nobel Lecture of 1919, Stark admitted that the results from Bohr's theory "agree surprisingly well with observed facts." But this was not enough for him: "In spite of my high estimation of this achievement by the theory, nevertheless I am unable to accept it as definitive." http://nobelprize.org/nobel_prizes/physics/laureates/1919/stark-lecture.html.

was based on the presupposition that "as far as systems with one nucleus are concerned, Bohr's model is totally correct and well known."[182] Kossel, who at the time worked as an assistant at the Technische Hochschule in Munich, only studied Bohr's theory at a rather late date, perhaps in the late spring of 1914. In July he met the Danish physicist in Munich, where Bohr gave a presentation of his work.[183] "Have you seen a paper by Kossel in Verh. d. phys. Ges. 1914," Bohr asked H. M. Hansen. "I think he has got hold of something very important."[184] And indeed he had, for it was only with Kossel's work that Bohr's theory became truly reconciled with the results of Moseley, making it clear that X-rays had their origin in atoms which had lost an electron from an inner ring. Kossel's work was also important in stimulating Sommerfeld's interest in Bohr's theory.

Understandably, Bohr much welcomed the work of Kossel which he dealt with in his papers of 1915.[185] So he did with another piece of experimental evidence, which was obtained in Würzburg by Heinrich Rau, a student of Wien. Rau investigated the excitation of hydrogen and helium lines by collisions with electrons and interpreted his results as being in satisfactory agreement with Bohr's theory.[186]

A few more papers dealing with or referring to Bohr's theory appeared in the German physics literature. Ludwig Föppl, another physicist at the University of Würzburg, had earlier examined in great mathematical detail the stability of

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182 Kossel 1914b, p. 953. The paper was preceded by another paper with the same title, Kossel 1914a, in which he briefly referred to the "Bohr-Moseley" relation but without realizing how his results fitted into the picture.

183 On 15 July Bohr and Kossel shared the programme of the university's *Mittwoch* colloquium. See Heilbron 1967, who gives details about Kossel's work and career. In a letter to Bohr of 1921, quoted in Heilbron 1967, Kossel said that he only became acquainted with Bohr's works a short time before they met.

184 BCW II, p. 517.

185 Bohr 1915a and Bohr 1915b.

186 Rau 1914, to which Bohr referred in the same two papers of 1915.

electron configurations in a Thomson atom; in a paper of 1914 he did the same with the Bohr atom, where many of the calculations were similar to those of the Thomson case.[187] Based on dispersion and refraction experiments the physical chemist Adolf Heydweiller, at the University of Rostock, investigated the electrons in the hydrogen molecule, and in this context he referred briefly to Bohr's model of the molecule.[188] However, his result owed nothing to and was entirely different from Bohr's.

An attempt to establish an alternative to Bohr's theory, or to translate it into more classical terms, was made by Ernst Gehrcke at the Physikalisch-Technische Reichanstalt. Without using Bohr's postulates, Gehrcke derived the Balmer-Bohr formula for hydrogen and suggested an explanation of both the Zeeman effect and the Stark effect on the basis of his atomic model. Gehrcke did not dismiss quantum theory, but he preferred to do without it. Contrary to Bohr's theory, "In my model of emission of light the assumption of energy quanta is admissible but not necessary."[189] The expression that Gehrcke found for the change in frequency caused by an electric field (the Stark effect) differed by a factor 4/3 from the one published a little later by Bohr.

The Danish chemist Niels Bjerrum was not only a close friend of Bohr but also a pioneer in the use of quantum theory to problems of molecular structure, work he did while staying with Walther Nernst in Berlin 1910-1911. In a later work on infrared spectra of gases Bjerrum investigated carbon dioxide and other simple molecules based on the assumption that their vibration and rotation

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187 Föppl 1912 (Thomson atom) and Föppl 1914 (Bohr atom). Föppl's interest in the Bohr atom was more mathematical than physical.

188 Heydweiller 1915.

189 Gehrcke 1914a, p. 839. Gehrcke would later join forces with anti-relativists and oppose the Bohr-Sommerfeld quantum theory of atomic structure (Kragh 1985b). In a footnote in Bohr 1914a (p. 511) he commented on Gehrcke's theory and its difference from his own.

energies were quantized. It might be necessary to go further, he said, and "to resort to similar revolutionary intuitions for explaining the radiation connected with the vibrations and rotations of molecules as has been done recently by N. Bohr in the case of electronic radiation."[190]

5.3 Stark effect, dispersion, and ionization

A few physicists, both in Germany and elsewhere, found it tempting to apply ideas of quantum theory to radioactivity and the enigmatic atomic nucleus. If the orbital electrons were governed by Bohr's equations, why not the nuclear electrons? The German physicist Heinrich Rausch von Traubenberg used in 1915 Bohr's atomic theory to calculate not only the speed of beta particles but also of alpha particles. Hans Wolff, a physicist from Dresden, developed a somewhat similar idea based on a speculative model of the nucleus.[191] Suggestions like these were followed up by a few later physicists, but Bohr and most other mainstream physicists ignored them.[192] Of much greater importance than such speculations was the application of Bohr's theory to the Stark effect.

The electric splitting of the spectral lines of hydrogen and helium that Stark discovered in November 1913 was an important factor in the increased interest that met Bohr's theory in Germany in particular. Having acquainted himself with

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190 Bjerrum 1914, p. 749. English translation in Bjerrum 1949, pp. 41-55. On Bjerrum, Nernst and the role of molecular spectroscopy in early quantum theory, see Assmus 1992. Bohr referred to Bjerrum's works in the third part of his trilogy (Bohr 1913d, p. 866).

191 Traubenberg 1915; Wolff 1915. For some other attempts of the period to explain radioactivity in terms of the dynamics of the nucleus, see Stuewer 1983.

192 Although these attempts were not highly regarded, it made sense to apply Bohr's theory to the calculation of the orbits of beta electrons emitted by the nucleus. Epstein 1916 was a serious attempt to understand the beta spectrum in terms of the Bohr-Sommerfeld theory. On the theories of Wolff and Epstein, see Jensen 2000, pp. 47-50.

Stark's work, Bohr immediately too up the challenge and on the last day of 1913 he wrote to Rutherford that "I have succeeded in accounting, at least partly, for the experiments of Stark on the basis of my theory."[193] Bohr's paper in which he introduced his theory of what quickly became known as the Stark effect appeared in the March 1914 issue of *Philosophical Magazine*. However, he was not the first to offer an explanation of the electric effect on the basis of his new model of the atom. As mentioned, as early as 5 December 1913 Warburg read a paper to the German Physical Society in which he attempted to explain the Stark effect on the basis of Bohr's theory of the hydrogen atom. Since this theory was new and not generally known to German physicists, he included a condensed account of it.[194] According to Robert Pohl, who attended the meeting in Berlin, Warburg gave a report "on a very important paper, that was Bohr's paper, … He explained … that this was a real advance, and I believe that the few hundred listeners at once understood [that] … 'Planck's *h* proves to be the key for understanding the atom'."[195]

Warburg, who mistakenly seems to have considered Bohr's theory as belonging to the tradition of the electromagnetic matter theories, clearly found the theory appealing. But he was less happy about its foundation in Bohr's postulates, which invited "weighty misgivings." Although Warburg obtained a broadening of the right order of magnitude, he did not succeed in reproducing the distinct line patterns observed by Stark, from which he concluded that Bohr's theory was unable to give more than a partial explanation: "It does in no way

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193 Bohr to Rutherford, 31 December 1913, in BCW II, pp. 591.
194 Warburg 1913. Heilbron (1964, p. 322) says about Warburg's paper that it was "the first time Bohr's work was presented in detail in Berlin, and it is almost certainly the first application of Bohr's atom not made by its originator."
195 Interview with Thomas S. Kuhn and John L. Heilbron of 5 March 1962, as quoted in Pais 1991, p. 154. It is doubtful if the German physicists listening to Warburg "at once understood" the meaning and range of Bohr's theory.

explain it [the Stark effect] completely and for this reason a modification or extension of it is in any case needed."[196] Bohr disagreed. In a letter to Warburg of early 1914 he remarked that his own as yet unpublished calculations resulted in separate lines with the right separation in wavelength.[197]

Two weeks after Warburg's address in Berlin, the astronomer Karl Schwarzschild suggested a classical model for the Stark effect which was inspired by the methods of celestial mechanics.[198] Although Schwarzschild referred to Bohr's atomic theory he did not make use of either it or other concepts from quantum theory. On the other hand, a slightly later preliminary calculation made by the Italian physicist Antonio Garbasso, professor in Florence, was closely based on Bohr's "marvelous theory of spectral analysis."[199] Garbasso proposed his interpretation of what he called the Stark-Lo Surdo phenomenon in terms of Bohr's theory at a session of the Accademia dei Lincei on 21 December 1913, which was the first reference to Bohr's theory in Italy.[200]

Bohr's own and superior analysis of the Stark effect appeared in the March issue of *Philosophical Magazine* where he calculated a frequency shift due to an external electric field that agreed reasonably well with the observations of Stark. Realizing that his theory did not cover all the experimental details, Bohr cautiously concluded that "it seems possible to account for some of the general features of the effect of magnetic and electric fields on spectral lines discovered

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196 Warburg 1913, p. 1266.
197 Bohr to Warburg, 8 January 1914, in BCW II, p. 608.
198 Schwarzschild 1914, read at meeting of the German Physical Society 19 December 1913. Bohr to Schwarzschild, 23 February 1914, in BCW II, pp. 600-601.
199 Garbasso 1914a. For his praise of Bohr's marvelous theory, see Garbasso to Bohr, 19 January 1914, in BCW II, p. 511. In Garbasso 1914b he reported new details of the Stark effect which made him comment that "perhaps Bohr's theory, in its present form, is unable to account for the phenomenon."
200 The discovery of the electric splitting of spectral lines was made independently by Garbasso's compatriot Antonino Lo Surdo. See Leone, Paoletti and Robotti 2004.

by Zeeman and Stark."[201] By the spring of 1914 Bohr was confident that his theory agreed with, or could be developed to agree with, the Stark effect. This optimistic view was shared by many other physicists, but not by all. For example, Gehrcke maintained that his own atomic theory offered a better explanation of the experimental details than any of the theories that built on Bohr's model, whether Warburg's, Garbasso's or Bohr's.[202]

The work of Stark was influential in another case that soon came to be seen as strong support of Bohr's theory, namely the Nobel Prize rewarded electron collision experiments of James Franck and Gustav Hertz.[203] That experiments of this kind might yield information about the energy levels of the stationary states of atoms was already suggested by Bohr in his trilogy, but the Franck-Hertz series of experiments were quite unrelated to Bohr's theory. Rather than thinking in terms of spectra and atomic structure, in agreement with an earlier idea of Stark they interpreted their results as due to ionization processes. In none of the publications of Franck and Hertz until 1916 did they refer to Bohr's theory or his alternative interpretation, and when they did so in a paper of 1916 it was only to reject it.[204] Franck and Hertz were not alone in interpreting ionization experiments as a problem for Bohr's theory. For example, the Canadian physicist John McLennan, professor at the University of Toronto, concluded in 1916 that

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[201] Bohr 1914a, p. 524. Bohr also applied his theory to the normal Zeeman effect, a topic that a little earlier had been discussed by Karl Herzfeld on the basis of Bohr's theory (Herzfeld 1914). On the role of the Stark effect in the reception of Bohr's atomic theory, see Hoyer 1974, pp. 215- 241.

[202] Gehrcke 1914b.

[203] See the sources in Hermann 1967. For a historical perspective, see Hon 2003.

[204] Franck and Hertz 1916. They acknowledged Bohr's interpretation in 1919 and received the Nobel Prize in 1925, in large measure because of the connection to the theory of Bohr.

the results of experiments with mercury vapour "would indicate that the theory [of Bohr] is invalid."[205]

In an interview many years later Franck recalled that he and Hertz were unaware of Bohr's theory when they made their key experiments in 1914:

> We had neither read nor heard about it. We had not read it because we were negligent to read the literature well enough … On the other hand, one would think that other people would have told us about it. For instance we had a colloquium at that time in Berlin at which all the important papers were discussed. Nobody discussed Bohr's theory.[206]

When the experiments nevertheless played a role in the reception of Bohr's theory, it was not due to the two German physicists but to Bohr, who in 1915 pointed out how the data obtained in Berlin could be understood as a confirmation of his theory of stationary states.[207]

As mentioned in Section 3, Sommerfeld had aquainted himself with Bohr's theory at an early date. The French physicist Léon Brillouin later recalled:

> When Bohr's theory on the hydrogen atom was published in 1913, Sommerfeld immediately saw the importance of this new idea. I happened to be in his office when he opened the issue of the *Philosophical*

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205 McLennan and Keys 1916, p. 607. On the other hand, the two Canadians found that their experiments with cadmium vapour supported the theory and ended up with the mixed conclusion that the combined results "neither conclusively support nor definitely tend to invalidate Bohr's theory of atomic structure." Millikan 1917b, presented at a meeting of the American Physical Society on 1 December 1916, argued in favour of Bohr's explanation of the electron collision experiments.

206 Quoted in Holton 1961, p. 808.

207 Bohr 1915b, p. 411.

> *Magazine*, which had just arrived; he glanced through it and told me, "There is a most important paper here by N. Bohr, it will mark a date in theoretical physics." And soon after, Sommerfeld started applying his own "quantum of action method" to rebuild a consistent theory of Bohr's atom.[208]

There is little doubt that the recollection, told 36 years after the event, is to some extent a reconstruction. If Sommerfeld had really considered Bohr's paper to be of revolutionary importance, it is hard to understand why it took him so long to make the theory part of his own work and the works of his assistants and students in Munich.

Sommerfeld's growing interest in the Bohr atom was in part indebted to the Stark effect and its relation to Bohr's theory, a subject he dealt with in the Munich colloquium of 27 May 1914.[209] A few days later he wrote in a letter to the French physicist Paul Langevin, this time relating to the Zeeman effect, that "in the atom a number-theoretical symmetry and harmony appears to rule, as from another side Bohr has shown." Then he expressed his reservations with regard to the state of Bohr's theory: "Clearly a great deal is true in Bohr's model and yet I think that it must be fundamentally reinterpreted in order to satisfy. In particular, I am presently disturbed that it gives a wrong value for the magneton."[210] Sommerfeld was warming up, but even after having met with Bohr in Munich, he was not ready to adopt the new atomic theory as a basis for

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208 Brillouin 1960, p. v, foreword dated September 1959.

209 For the importance of the Stark effect for Sommerfeld's growing interest in Bohr's theory, see Eckert and Märker 2000, p. 437.

210 Sommerfeld to Langevin, 1 June 1914, in Eckert and Märker 2000, p. 484-485.

his further research. For example, he continued to investigate the Zeeman effect without taking into regard Bohr's model.[211]

Only in 1915 did Sommerfeld publish his first research works on the new theory, on dispersion theory and on a generalization of Bohr's theory of the hydrogen spectrum. Sommerfeld's theory of dispersion based on Bohr's model of simple molecules was not the first of its kind, for Peter Debye had a little earlier developed a somewhat similar theory that worked well for molecular hydrogen but not for helium.[212] The success of Sommerfeld's more elaborate theory was also limited to hydrogen. Bohr was pleased that Debye and Sommerfeld were interested in the same questions as himself, but "I don't think I agree with them at all," as he wrote to his brother Harald. "I look upon the entire problem of dispersion in quite a different way."[213] The Debye-Sommerfeld theory (as the two theories were collectively known) assumed classical electrodynamics to apply to the perturbations of stationary orbits caused by external radiation, and thus described the interaction between radiation and the orbiting electron in classical terms. This Bohr found to be objectionable. "If the theory of the Hydrogen atom has but the slightest connection with truth," he wrote to Oseen, "the dispersion (at least in gases) must be a phenomenon of quite a different nature from that assumed by Debye and Sommerfeld."[214]

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211 Eckert and Märker 2000, p. 435. On Sommerfeld's reworking of Bohr's theory in 1915-1916, see Seth 2010, pp. 162-171.

212 Debye 1915a; Sommerfeld 1915a. For Debye's model of the hydrogen molecule, a refinement of Bohr's model of 1913, see also Wolfke 1916.

213 Niels Bohr to Harald Bohr, 10 November 1915, in BCW I, p. 581.

214 Bohr to Oseen, 20 December 1915, in BCW II, p. 565. The Debye-Sommerfeld theory caused Oseen to conclude that Bohr's theory was irreconcilable with the validity of Lorentz-Maxwell electrodynamics within atoms (Oseen 1915). Knudsen (2001, p. 248) suggests that O. W. Richardson's conception of the Bohr atom was similar in spirit to the one of Debye and Sommerfeld.

Under the impact of the new progress in quantum and atomic theory, even Planck (who was generally hostile to atomic models) took up Bohr's theory of atoms and spectra. His work in this area was, as he expressed it in a letter to Sommerfeld, "just a little excursion into for me unfamiliar territory."[215] In one of these excursions he derived the Bohr-Balmer formula in a way which he considered more satisfactory than the original one of Bohr. As he explained: "The idea developed here is different from Bohr's in so far that here the emission process is not necessarily connected with a jump of the oscillating electron from one stationary orbit to another stationary orbit, but instead it can be followed without some significant change in the elliptic orbit."[216] Having dealt with aspects of Bohr's theory in communications from late 1915, Planck decided to leave the further development of the theory to his colleague in Munich.

Einstein's interest in the Bohr atom came a little later. In spite of his possible early interest, as reported by Hevesy in the fall of 1913, it seems that Einstein only "discovered" Bohr's theory in 1916, in connection with his own work on emission and absorption of radiation. It is also only from that time that Bohr's name begins to enter Einstein's correspondence. In a paper from the summer of 1916, Einstein referred to "Bohr's theory of the spectra" and derived from statistical considerations Bohr's frequency condition.[217] In his autobiographical notes written thirty years later, Einstein characterized Bohr's atomic theory as a "miracle" and "the highest form of musicality in the sphere of thought." It was, he said, due to Bohr's "unique instinct and tact" that he had

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215 Planck to Sommerfeld, 30 January 1916, in Eckert and Märker 2000, p. 445.
216 Planck 1915a, p. 913. Planck's other excursion was Planck 1915b.
217 Einstein 1916. See also the comments in Kox, Klein, and Schumann 1996, p. 147.

been able to "discover the major laws of the spectral lines and of the electron-shells of the atoms together with their significance for chemistry."[218]

The generalization of Bohr's theory of the Balmer series which Sommerfeld presented to the Bavarian Academy of Sciences in late 1915 included that the electron of the hydrogen atom moved in elliptic orbits (rather than circular), something which Bohr had previously suggested but without developing the idea.[219] He started with praising Bohr's theory:

> The theory of the Balmer hydrogen spectrum appears at first glance to have been brought to a conclusion through the wonderful investigations of N. Bohr. Bohr could explain not only the general form of the series law, but even the numerical value of the constants detailed therein, and their refinement when the motion of the nucleus is taken into consideration.[220]

Yet Sommerfeld also pointed out, as Nicholson and others had done previously, that the Bohr model was essentially a model for the hydrogen atom: "One might even say that the capability of the Bohr theory is for the time limited to this hydrogen series and to the hydrogenic series (ionized helium, X-ray spectra, series ends of the visible spectra)."[221] In his alternative formulation Sommerfeld made use of two quantum numbers instead of one, and, applying a different and more general method than Bohr, derived the Bohr-Balmer formula.

In the other of the papers presented to the Bavarian Academy Sommerfeld introduced Einstein's special theory of relativity into the motion of the electron,

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[218] Schilpp 1949, p. 47.
[219] Bohr 1915a and 1915b.
[220] Sommerfeld 1915b, p. 425.
[221] Ibid.

thereby creating a new and more powerful version of the Bohr theory able to account in quantitative details for the observed fine structure of the hydrogen spectrum.[222] With this generalization Bohr's theory of the atom, at the time not yet three years old, entered a new chapter. Bohr received the news from Munich with enthusiasm. "I do not think that I have ever enjoyed the reading of anything more than I enjoyed the study of them," he said, referring to Sommerfeld's two papers of late 1915.[223] Einstein's reaction was no less enthusiastical: "Your investigation of the spectra belongs among my most beautiful experiences in physics. Only through it do Bohr's ideas become completely convincing. If I only knew, what little bolts the Lord had used for it!"[224]

Debye's work on the hydrogen molecule, presented to the Bavarian Academy on 9 January 1915, was not the only work in which he developed ideas related to Bohr's theory. At about the same time he investigated the scattering of X-rays on atoms within the framework of the Bohr ring model. Fully realizing that this model of the atom contradicted the laws of electrodynamics, Debye stated that "The more recent development in our conceptions of atomic structure has forced us to recognize the possibility of electrons in motion which, in spite of very large accelerations, do not emit energy. We have to assume, for instance, the presence of two electrons in a hydrogen molecule, situated opposite one another on a circle 1.05×10^{-8} cm in diameter and revolving with an angular velocity $\omega =$

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222 Sommerfeld 1915c. On Sommerfeld's theory of fine structure and its relation to experiments, see Nishio 1973, Kragh 1985 and Robotti 1986.
223 Bohr to Sommerfeld, 19 March 1916, in BCW II, p. 603. The two papers were Sommerfeld 1915b and Sommerfeld 1915b.
224 Einstein to Sommerfeld, 3 August 1916, in Eckert and Märker 2000, p. 563. Sommerfeld's work was not only an important generalization of Bohr's atomic theory but also an argument for the use of relativity theory in the interior of atoms, which undoubtedly contributed to Einstein's enthusiasm.

4.21×10^{16} sec^{-1}."[225] He thought that the electron ring structure might reveal itself by X-ray interference patterns, which was the original motive for what became the celebrated Debye-Scherrer method of X-ray diffraction.[226] Paul Scherrer, who had come to Göttingen in 1913 at the age of 24, recalled about Debye's idea that the regular spacing of electrons on circular orbits should produce diffraction effects:

> One tried hard to become convinced of the reality of Bohr's electron orbits in the atoms in spite of all the hesitations the physicist felt in accepting the hypothesis that the electron on its stationary orbit about the atomic nucleus does not radiate, – a flagrant contradiction to Maxwell's theory. The next job to be done was therefore to find a check on Bohr's hypothesis which worked so simply and directly in the case of spectral emission, by looking for a direct evidence of the reality of the electronic orbits.[227]

The work of Debye and Scherrer turned out to be eminently useful in the study of solids and liquids. Of course, it did not yield "direct evidence of the reality of the electronic orbits." As became known only after the advent of quantum mechanics, orbits in the semiclassical sense of Bohr do not exist.

6. Conclusions

Two years after Bohr had announced his theory of atoms and molecules, it was widely accepted or at least seriously considered by physicists working with

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[225] Debye 1915b, p. 809. Although Debye clearly had Bohr's theory in mind, he did not refer to the theory or Bohr's name.

[226] Debye and Scherrer 1916, presented at a meeting of the Göttingen scientific society of 4 December 1915. The paper included a reference to Bohr's atomic theory.

[227] Scherrer 1962, p. 642.

quantum theory and the structure of matter. Given the radical nature of the postulates on which the theory rested, Bohr could be satisfied with how it was received in the physics communities in England and Germany, the two leading countries of physics at the time. Of course its victory was not complete, for many physicists resisted it and even more were indifferent or just ignorant of it. Yet, by the end of 1915 the majority of physicists doing research in atomic physics and related areas recognized that Bohr's theory constituted an important advance that might well define the course of future research. There was no way back.

Bohr's model of the atom was successful in both a social and a scientific sense. What swayed otherwise sceptical physicists to accept it was primarily its *empirical successes*, that is, its remarkable ability to account for or predict phenomena that other theories failed to explain. In his publications of 1913-1915, Bohr emphasized the explanatory power of his theory rather than its foundation in the admittedly strange quantum postulates. It was this ability to explain known facts and predict new ones that attracted most attention and forced physicists to take the theory seriously. The theory got its best possible start by explaining the Pickering-Fowler lines and defending its explanation of the hydrogen and helium spectra against the objections of spectroscopists. At about the same time the theory received unexpected support from Moseley's and later Kossel's analysis of X-ray spectroscopy. By the end of 1915 the successes weighed more heavily than the few failures or inadequacies. Over the following years the balance would fluctuate, but gradually with an increasing awareness of the problems (such as the helium spectrum, the anomalous Zeeman effect and the spectra of many-electron systems). However, this later development is not part of the present essay.[228]

To many physicists the empirical successes of the Bohr theory overshadowed its problematic conceptual framework. Jeans's evaluation of 1913, that the only justification for the postulates was "the very weighty one of success," was shared by many of his colleagues. It might be the only justification, but even so it was enough. In his review of 1917 Millikan stressed that the success criterion included adaptability to take care of unexpected difficulties. This adaptability of Bohr's theory was not a result of *ad hoc* hypotheses grafted upon the original theory, but was rooted in the theory itself. By taking into account the finite mass of the nucleus, or the high velocity of the revolving electron, Bohr could explain phenomena that threatened the theory, and he could do it without the addition of new auxiliary hypotheses. No wonder that Imre Lakatos used Bohr's theory to illustrate the notion of "monster-adjustment," meaning "turning a counterexample, in the light of some new theory, into an example."[229] Moreover, discoveries that were only announced after the theory had appeared, most notably the Stark effect, could be understood within the framework of the theory. This kind of fertility, robustness and explanatory breadth impressed theorists and experimentalists alike.

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[228] For a checklist of the empirical problems and successes of the Bohr theory since its emergence in 1923 until its demise about 1924, see Kragh 2002.

[229] Lakatos 1970, pp. 148-149. What Lakatos called monster-adjustment was for Dudley Shapere a general "principle of nonrejection of theories," namely this: "When a discrepancy is found between the predictions of a theory and the results of observation or experiment, do not reject the theory as fundamentally incorrect before examining areas of the theory in which simplifications have been made which might be responsible for the discrepancy." Shapere 1977, p. 563. For an interesting and historically informed refinement of Lakatos's rational reconstruction of Bohr's research programme, see Hettema 1995.

It much helped the dissemination of Bohr's theory that there was no widely favoured alternative theory of atomic structure with which it had to compete. Various versions of the Thomson atom continued to be popular among British physicists, but they possessed neither the unitary structure nor the empirical fertility of the Bohr atom. In particular, they were largely unsuccessful in accounting for the Balmer series and other spectral regularities; besides, the way they incorporated the quantum of action (if they did) was half-hearted and unconvincing. After 1913 the Nicholson ring atom enjoyed little support and was not even pressed by Nicholson himself, although he continued to develop it until 1919. In Germany the only atom-builder of significance was Stark, and his theory of atoms and molecules was in a tradition entirely different from Bohr's in so far that it disregarded the internal structure of the atom and focused on the atom's surface structure.[230] Stark's electroatomic theory attracted some attention among chemists but was ignored by German and most other physicists.

If the empirical power of Bohr's theory made it attractive, its theoretical basis in the quantum postulates made it, in the minds of many physicists, unattractive. The opposition to the Bohr atom was in part empirically based and in part of a conceptual nature. Only few physicists concluded that the empirical arguments against the theory were reason enough to dismiss it *in toto*. But some, such as Nicholson and Hicks, found it suspicious that the theory could only claim success for the hydrogen atom and perhaps for other one-electron systems. As they saw it, Bohr's theory promised much more than it could actually deliver. Other physicists, including Stark, Fulcher and Fowler, believed for a time that the spectra of canal rays falsified the theory. And the chemists Parson and Lewis, soon followed by other chemists, found Bohr's theory unacceptable because of its

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[230] Stark 1910-15; Stark 1914b. For a summary account in English, see Stranges 1982, pp. 192-200.

inability to account for valency and the structure of molecules. The “chemical anomaly” was real enough, and became even more real in the years following 1915, but it was ignored by the physicists who did not consider it their problem. In general the opposition against Bohr’s theory was scattered and there never was a united front against it.

Of no less importance than the empirical objections were the arguments of a conceptual and methodological nature that related to the theoretical core of Bohr’s theory. One class of these arguments objected to the postulate of stationary orbits which so obviously contradicted the authoritative Maxwell-Lorentz theory of electromagnetism. Some physicists felt, not without justification, that the theory’s basic architecture was messy, a patchy combination of incomparable elements. This was a feeling that would only grow stronger with time. As Henry Margenau later phrased it, “Bohr’s atom sat like a baroque tower upon the Gothic base of classical electrodynamics.”[231] Bohr himself was well aware of the apparent inconsistency, but he saw it as a necessity and resource rather than a weakness. In the end, the inconsistency became the basis of quantum mechanics.

The irreconcilability between Bohr’s theory and the principles of electrodynamics made von Laue reject the theory (at least according to Tank’s recollections) and caused Wien to view it as inconsistent. It was highlighted in different ways by Stark in Germany, Oseen in Sweden, and Wereide in Norway, and it laid behind the opposition of Peddie, Hicks and some other British physicists. On the other hand, both Richardson and Campbell supposed that the conflict was somehow necessary and not a valid objection against the stationarity postulate. As Richardson pointed out, this was not the first time that quantum

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[231] Margenau 1950, p. 311.

phenomena contradicted electrodynamics. Indeed, by the time it was generally accepted that the quantum theory was incompatible with classical electron theory, such as argued by Lorentz and others.[232] Many physicists supposed that the Maxwell-Lorentz equations would need to be modified at intra-atomic distances.

It was an important element in the British tradition of physics in particular that it should be possible to form a physical picture or idea of a theory or model. For a theory to be truly convincing it had to include a *dynamical mechanism* that caused the phenomenon in question. However, Bohr's theory provided neither a mechanism nor a physical picture of the radiation process, it merely postulated discontinuous and apparently uncaused quantum jumps. To Runge in Germany this scarcely qualified as a physical explanation but was at best a computational recipe. The theory was, in the words of Thomson, "arithmetical rather than dynamical." Hick's agreed, complaining that the theory "throws no further light on the structure of the atom itself, as the mechanism of radiation is totally unexplained." A somewhat similar attitude may have been behind Ehrenfest's dismissal of the theory. Objections of this kind were fairly common, both in England and elsewhere, but they did not constitute a serious hindrance for the acceptance of the Bohr atom. Most physicists were willing to disregard them in the light of the theory's empirical successes.

As mentioned, Bohr's atom attracted interest among British physicists at an earlier date than in Germany. This was not only because it appeared in an English journal and was closely connected to Rutherford's nuclear atom, but also because the theory was to a large extent in the tradition of British atom-building. Its *style* was British, not German. Up to 1915 Bohr was almost alone in

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[232] For the relationship between quantum theory and electromagnetism, see Seth 2004.

developing his ideas, which he did with great energy and ingenuity. W. Wilson was about the only British physicist who contributed to the theory, and his contribution was mathematical rather than physical. Although the Germans came later, in this respect they were more active, at first inspired by the Stark effect which Warburg analyzed on the basis of Bohr's theory at an early date. Later contributions to and extensions of Bohr's works were made by Kossel, Debye, Epstein, Schwarzschild and – most importantly – Sommerfeld. This kind of innovative work was not pursued by British physicists who tended to see the Bohr model as a theory of spectra rather than a general quantum theory of atomic structure and phenomena.

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Appendix I: Physicists' attitude to Bohr's theory, 1913-1916

Below I list a number of physicists (and a few chemists) who referred to or dealt with Bohr's theory during its early phase. Some of them did not express their view in publications, and we know of their attitude only from letters or later reminiscences. The names of these scientists are italized. In addition to those who expressed an opinion of Bohr's theory, or just referred to it, there was of course a large number of scientists who ignored the theory at least initially (examples are Thomson, Larmor, Rayleigh, Mie, and von Laue). The scientists given in the second group expressed some interest in Bohr's theory, and sometimes used parts of it (often in unorthodox ways), but did not clearly endorse it. The year of birth is added after the names of the scientists.

<u>Opponents or critics of the theory</u>

A. W. Conway (1875); A. C. Crehore (1868); *P. Ehrenfest* (1880); G. S. Fulcher (1884); W. M. Hicks (1850); *M. von Laue* (1879); G. N. Lewis (1875); F. A. Lindemann (1866); J. W. Nicholson (1881); A. L. Parson (1889); W. Peddie (1861); *C. Runge* (1856); J. Stark (1874); T. Wereide (1882).

<u>Interested in the theory</u>

H. S. Allen (1873); N. Bjerrum (1879); *M. Born* (1882); W. H. Bragg (1862); W. L. Bragg (1890); S. D. Chalmers (?); *A. Einstein* (1879); P. S. Epstein (1883); L. Föppl (1887); A. Fowler (1868); E. Gehrcke (1878); H. Geiger (1882); *A. E. Haas* (1884); J. Ishiwara (1881); S. B. McLaren (1876); I. Langmuir (1881); O. Lodge (1851); C. W. Oseen (1879); M. Planck (1858); O. Richardson (1879); R. Seeliger (1886); E. Warburg (1846); W. Wien (1864).

<u>Supporters of the theory</u>

E. H. Buchner (1880); N. R. Campbell (1880); P. Debye (1884); E. J. Evans (1882); A. S. Eve (1862); A. Garbasso (1871); H. M. Hansen (1886); K. F. Herzfeld (1892); G. von Hevesy (1885); J. Jeans (1877); W. Kossel (1888); R. A. Millikan (1868); H. G. Moseley (1887); F. Paschen (1865); E. Riecke (1845); E. Rutherford (1871); F. Soddy (1877); A. Sommerfeld (1868); G. W. Stewart (1876); W. Wilson (1875).

Appendix II: A generational gap?

One might believe that the younger scientists tended to support Bohr's theory, while the older generation opposed it. Lord Rayleigh, who in 1913 had just passed 70 years, may be a case in point. Bohr recalled that when Rayleigh at the

British Association meeting in 1913 was requested to offer his opinion about the recent developments in atomic and radiation theory, he declined to do so. He is to have said: "In my young days I took many views very strongly and among them that a man who had passed his sixtieth year ought not to express himself about modern ideas. Although I must confess that today I do not take this view quite so strongly, I keep it strongly enough not to take part in this discussion."[233]

If there were a generational gap in the reactions to Bohr's theory, it was only small. The average age in 1913 of the 14 opponents listed above was 40 years, while the average of those supporting the theory was 37 years. There is no clear picture.[234] Some of the critics (such as Fulcher and Parson) were quite young and several of the supporters, such as Eve, Paschen and Riecke, were quite old. Nicholson, the most persistent of Bohr's critics, was only his senior by four years. If Hicks and Runge confirm the generation hypothesis, what to do with Riecke, who at the age of 70 came out strongly in support of Bohr's theory?

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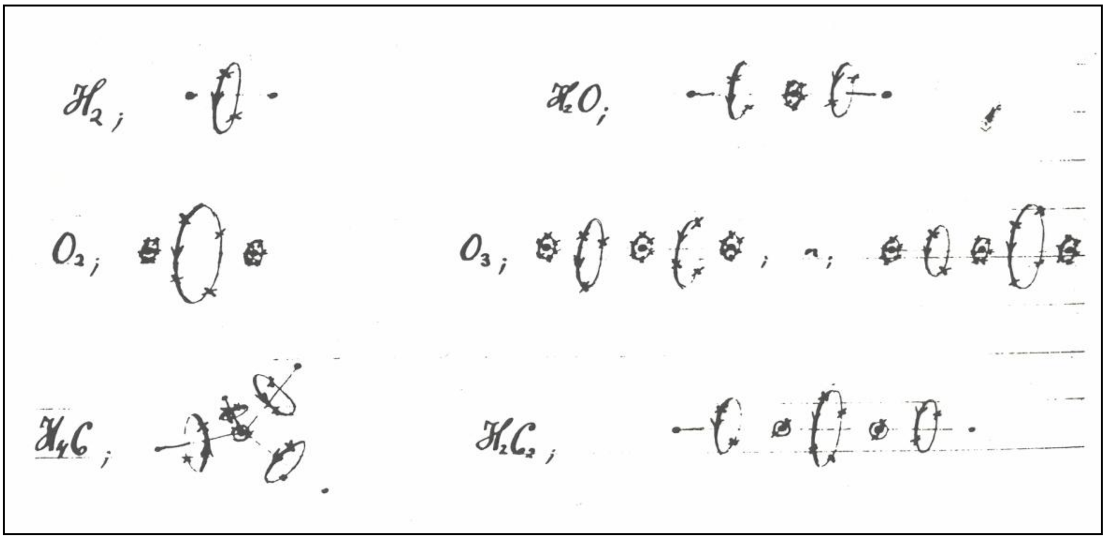
[233] Rutherford Memorial Lecture of 1958, in BCW X, p. 393. According to Robert John Strutt, Lord Rayleigh's son and biographer, in 1913 he asked his father if he had seen Bohr's paper on the hydrogen atom. The third Lord Rayleigh replied, "Yes, I have looked at it, but I saw it was no use to me. I do not say that discoveries may not be made in that sort of way. I think very likely they may be. But it does not suit me." Strutt 1968, p. 357.

[234] The lists are to some extent arbitrary and should not be given much weight. For example, one might reasonably add physicists like Thomson (1856) and Larmor (1857) to the first group, although they did not in fact refer to or comment on the theory in the period. In that case the average age of the opponents would increase to 42 years.



Bohr's three papers of 1913 contained no illustrations, but there are several visual models in his manuscript notes. This one shows his ideas, as described in Part III of his work, concerning the covalent bond in molecules. Notice that he incorrectly described water (H_2O) and ozone (O_3) as linear molecules.

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Abbreviations:

BCW I = Rud Nielsen (1972); BCW II = Hoyer (1981); BCW X = Favrholdt (1999).

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