Complementarity before uncertainty
Author(s): Sandro Petruccioli
Source: *Archive for History of Exact Sciences*, Vol. 65, No. 6 (November 2011), pp. 591-624
Published by: Springer
Stable URL: https://www.jstor.org/stable/41287710

Arch. Hist. Exact Sci. (2011) 65:591–624
DOI 10.1007/s00407-011-0087-0

Complementarity before uncertainty

Sandro Petruccioli

Received: 30 March 2011 / Published online: 10 August 2011

Abstract This article argues that a manuscript dated to the summer of 1927 by the editors of Bohr's *Collected Works* was written a year earlier. The re-dating allows the conclusion that Bohr was well on his way to complementarity before his famous fight with Heisenberg over the uncertainty principle early in 1927. The literature that assumes that complementarity was Bohr's response to Heisenberg is therefore in error. The editors of the *Collected Works* assigned the document the date of 1927 because it refers to electron-diffraction experiments by Davisson and Germer, which were published in 1927. But as the article points out, Bohr and other leading physicists such as Max Born met Davisson in Britain in 1926 and discussed the experiments with him then. That demolished the basis of the earlier dating. Finally, the article argues that when the document takes its place between the Bohr–Kramers–Slater theory (1924) and the 1927 drafts of complementarity, everything falls neatly into place.

1 Introduction

Volume 6 of Niels Bohr's *Collected Works*, entitled "Foundations of quantum physics I (1926–1930)" is entirely devoted to the genesis, statement, and affirmation of the 'principle' of complementarity.[1] By this concept, Bohr believed he was providing a consistent interpretation of the foundations of atomic physics, thus recomposing the serious break created with the formulation of two formally equivalent theories conveying two quite different images of reality: Heisenberg, Born and Jordan's quantum

[1] Bohr (1985) [henceforth CW6].

Communicated by: Jed Buchwald.

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

mechanics and de Broglie and Schrödinger's wave theory. In addition to some articles and a selection of Bohr's scientific correspondence, Jørgen Kalckar, the editor of the book, offers readers and scholars some manuscripts that document with remarkable accuracy the path that led the Danish physicist to develop his interpretative hypothesis. There are four texts, all of them unpublished, accounting for 31 of the 195 pages contained in the "Como Lecture II", 1927 folder of the Niels Bohr Archive in Copenhagen.[2]

Only one manuscript, entitled "Philosophical foundations of the quantum theory" is undated, but its drafting can be placed with certainty in the weeks following the end of May 1927.[3] The first of the three documents carrying a date is an eight-page manuscript entitled "Fundamental problems of the quantum theory" dated September 13, probably the notes for the conference that Bohr held a few days later at Como for the celebrations of the 100th anniversary of the death of Alessandro Volta.[4] The second one is a comprehensive text composed of twelve typewritten pages, dated 12 and 13 October, written by Bohr[5] just before going to Brussels to attend the Fifth Solvay Council, where he had the opportunity to present his theory to the most authoritative group of physicists of the time and–more importantly–to discuss his ideas directly with Albert Einstein.[6] The title of the manuscript, "The quantum postulate and the

[2] Many of the texts collected in this folder are dated and with a single exception, they were all written between July 2 and 13 September 1927. Here's how Kalckar describes the contents of the folder: "The folder "Como Lecture II", 1927, contains a large number of drafts and notes, comprising 181 sheets, a few of which have notes on the reverse, making a total of 195 written pages. Most of them are handwritten, in pencil or ink, Klein's, Bohr's, Mrs Margrethe Bohr's and an unidentified hand. There are few typed pages and carbon copies. The languages are English, Danish and German. Many sheets are dated, carrying dates between 2 July 1927 and 13 September 1927 (except for 2 pages, dated 1926, apparently by mistake). The main titles are: "Atomteori og Bølgemekanik" ("Atomic Theory and Wave Mechanics"), "Philosophical Foundations of the Quantum Theory", "Fundamental Problems of the Quantum Theory", "Über die Wellentheorie des Lichts und der Materie" ("On the Wave Theory of Light and Matter"), and "Zur Frage des begrifflichen Aufbaus der Quantentheorie" ("On the Question of the Conceptual Structure of the Quantum Theory")", CW6 58.

[3] CW6 69–71. The text was actually written by Oskar Klein, Bohr's assistant at Institute for Theoretical Physics in Copenhagen at the time, and it has a few corrections by Bohr. *Nature* in the number of 28 May 1927 had published a note from the British physicist and philosopher of science Norman R. Campbell (1927) and accepted a brief reply by Pascual Jordan. Evidently stimulated by this discussion, Bohr decided to intervene with a paper that opens with these words: "In connection with the discussion between Drs. Norman R. Campbell and P. Jordan, published under this headline in the issue of *Nature* May 29, I should be glad to get space in these columns for the following general remarks" CW6 69. The note was never sent to *Nature*. See Interview to Oskar Klein by John L. Heilbron and L. Rosenfeld on 28 February 1963, AIP.

[4] Manuscript and transcription in CW6 75–88. The Como Conference, organized in memory of the work of Alessandro Volta on the centenary of his death, was held from 11 to 20 September 1927. Bohr presented his report, in which he introduced the notion of complementarity, on 16 September. The extensively revised text of the conference was published in Bohr (1928a). Also speaking at the debate were Max Born, Hendrik Kramers, Werner Heisenberg, Enrico Fermi, and Wolfgang Pauli, ibid., pp. 589–598.

[5] CW6 91–98. Bohr sent the manuscript to Charles G. Darwin, his friend since the years spent in Manchester, with a letter dated 16 October 1927.

[6] The Fifth Solvay Conference was held in Brussels from 23 to 29 October 1927 and the theme chosen by Lorentz for the meeting was 'electrons and photons'. The level of participants was extraordinarily high, perhaps unparalleled in the history of twentieth century physics.

recent development of atomic theory", is the same of the paper that a few weeks later Bohr was to send to *Nature*.[7]

The last dated document, which Kalckar himself defines as "extremely interesting",[8] is completely different from the previous ones, a set of notes lacking an explicit logical–conceptual path. A text "evidently written with great haste"[9] which probably arose from the need to put the complex steps of an argument on paper, to be developed further at a later stage. The manuscript reproduced in the *Collected Works* volume[10] (henceforth referred to as mss26) has no title and is 'composed' of three sheets; only the first two are numbered (in Roman) and carry the date 7.10.1926 (10 July 1926) that the editor corrects into 1927, adding a brief footnote explaining that "on the manuscript the year is erroneously given as 1926".[11] According to Kalckar the evidence of the error that Bohr would have perpetrated twice with his own hand, lies in the fact that in the second sheet of mss26 the name of the American physicist Davisson is mentioned as a "confirmation" of the de Broglie wave theory of matter. In fact, on 3 March, 1927 Clinton Davisson had sent a letter to *Nature* containing the first results of "a series of experiments now in progress", carried out together with Lester Germer at Bell Laboratories, on electron diffraction by crystals that represented a strong empirical evidence in favor of the wave interpretation of material particles.[12] Hence his—I think rather hasty—decision to shift the dating of mss26 up by one year because, according to Kalckar, this manuscript was undoubtedly subsequent to the publication of Davisson's letter.

In this article I will demonstrate the following theses:

(a) According to an analysis of content and to some remarks of formal nature it can be accurately stated that the third page of the document is not an organic part of mss26 and was written by Bohr at a later date.
(b) The fact that the Davisson's name appears in mss26 cannot be used as evidence of the document not having been written in July 1926. On the contrary, some historical data allow us to say that it would be entirely plausible that in July 1926 Bohr was referring to the results of Davisson's experimental investigations, which he had started in 1919, as a confirmation of the de Broglie theory.
(c) The point above is further supported by the results of a comparative analysis of the conceptual and theoretical framework of mss26 and of the other three documents published by Kalckar written by Bohr between June and October 1927.

[7] Bohr (1928b). The article was also published in German and Danish; for a comparison between the different versions of the text please refer to the notes by Jørgen Kalckar in CW6.

[8] CW6 27.

[9] Ibid., p. 58.

[10] Manuscript, transcription and translation, ibid., pp. 59–65.

[11] Ibid., p. 27, n. 20. Below, in illustrating this document, Kalckar reaffirms: "The first two pages are numbered and are dated 10 July 1926 [1927, as is clear, i.a., from the reference to Davisson]", ibid., p. 58.

[12] Davisson and Germer (1927a). In particular, the letter reads: "These results are highly suggestive, of course, of the ideas underlying the theory of wave mechanics, and we naturally inquire if the wave-length of the X-ray beam which we thus associate with a beam of electrons is in fact the *h*/*mv* of L. de Broglie", ibid., p. 559.

So it would be quite possible to argue that Kalckar's decision to change the date on mss26 was not only hasty, but quite likely also wrong; Bohr's notes actually date back to the summer of 1926, before he invited Erwin Schrödinger to Copenhagen to discuss his wave theory,[13] and above all many months before Werner Heisenberg sent his famous article on uncertainty relations to *Zeitschrift für Physik*.

This is much more than an archival nicety since, as we shall see, were my theses to prove correct it would change the interpretative perspective making it possible to achieve the historically correct reconstruction of one of the most troubled periods of early twentieth century physics. It would in fact constitute documentary evidence to the hypothesis that Bohr had already grasped the concept of complementarity when he started that intense, lively and sometimes harsh exchange of ideas with Schrödinger and Heisenberg during the fall and winter of 1926–1927. We could also then, finally, get rid of the established interpretative hypothesis that relegates Bohr's contribution to the foundation of quantum mechanics to a cunning ploy aimed at rebuilding a shattered philosophical–theoretical framework and as a means for a come-back after his resounding defeat in 1924.[14]

But there's more. The second page of mss26 ends with a statement regarding the outcome of the measurement of energy and momentum, which surprisingly contains the core of the reasoning underlying the Heisenberg uncertainty paper, or, more precisely, the way in which Bohr would have interpreted the 'discovery' of Heisenberg in his paper on complementarity.

2 The historical context

2.1 The Addendum

The issue of the *Zeitschrift für Physik* containing Werner Heisenberg's article „Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik" was published on 31 May 1927.[15] Heisenberg had sent his paper to the journal in mid-March from the Teoretisk Institut for Fysik in Copenhagen, where he was professor Niels Bohr's assistant since spring 1926. The publication put an end to a long and sometimes dramatic contrast between Bohr and his young co-worker about the contents of the article. Eventually, it was published without any substantial change to the text sent in March, but with a short addendum by Heisenberg on the draft (*Nachtrag*

[13] Also as a result of the report made by Heisenberg, who had attended the conferences held by Schrödinger in Munich on July 21st and 23rd, with a letter dated 11 September 1926 Bohr had invited Schrödinger to Copenhagen to discuss the open questions of atomic theory in detail. Schrödinger, who met Bohr for the first time on this occasion, arrived at the Copenhagen train station on 1 October.

[14] See for example the well-known and influential view of Lakatos (1970, p. 145): "In the new theory Bohr's notorious 'complementarity principle' enthroned [weak] inconsistency as a basic ultimate feature of nature, and merged subjectivist positivism and antilogical dialectic and even ordinary language of the philosophy into one unholy alliance".

[15] Heisenberg (1927).

bei der Korrektur) containing some corrections that the author attributes directly to Bohr and what had emerged "in the course of several discussions on this paper".[16]

Fortunately, the accuracy standards of scientific journals at the time had not prevented the diffusion of an article in which the author himself drew the reader's attention to the errors it contained; this allowed science to acquire one of its most important laws for understanding the microscopic world. Even historians have reason to rejoice that those standards were far removed from the criteria used by today's referees, gaining an opportunity to learn from the protagonists themselves what was, in those months, the nature and extent of the conflict behind the definition of conceptual and epistemological foundations of quantum theory.

The addendum ends with the author's formal thanks to professor Bohr and the announcement of a forthcoming article in which the results of the more recent investigations on the conceptual structure of quantum theory are to be presented. In fact, Heisenberg refers the reader to a future article of Bohr's for a final clarification on the controversial and not fully exhaustive topics of his work. As we shall see in detail later, Heisenberg believed that in quantum theory discontinuities and particles were the only essential items. Furthermore, he believed that Schrödinger's sole merit consisted in the introduction of a mathematical model for quantum physics that made calculations particularly easy compared to the more complex matrix algebra; on that fact alone Schrödinger's theory had found favor with the majority of physicists. Bohr, on the other hand, was convinced that it was not possible to dispense with waves; only by assigning the same validity to the experiments that can be explained consistently by corpuscular theory or by wave theory could the real significance of the discovery of Heisenberg on the unavoidable uncertainty of physical quantities during measurement be grasped.

"Bohr and I tried from different angles and therefore it was difficult to agree. Whenever Bohr could give an example in which I couldn't find the answer, then it was clear that we had understood what the actual situation was. [...] in the end, shortly after Christmas, we both were in a kind of despair. In some way we couldn't agree and so we were a bit angry about it. So Bohr went away to Norway to ski. [...] So I was alone in Copenhagen and then within a few days I thought that this thing with the uncertainty relations would be the right answer".[17] In this way, during the interview granted to Thomas Kuhn on 23 February 1963 at the Max Planck Institute in Munich, Heisenberg was to reconstruct one of the crucial moments of the affair, at the peak of despair, to use his own words. Left alone in Copenhagen, Heisenberg had thus found the right climate and the concentration he needed to organize his ideas and shape his ambitious project. Already 1 week after Bohr's departure on 23 February, he sent Wolfgang Pauli a 14–pages long letter containing the core of his uncertainty paper.[18] asking for his opinion before Bohr's return to Copenhagen, expected for mid-March.[19] We know there was a letter Bohr sent to Heisenberg from Norway, which

[16] Ibid., p. 197.

[17] Interview of Werner Heisenberg by Thomas S. Kuhn on 25 February, 1963, AIP

[18] Heisenberg to Pauli, 23 February 1927, in Pauli (1970, pp. 376–381) [henceforth WB].

[19] There are no sure data about Bohr's return to Copenhagen. At the end of a letter sent to Pauli on 14 March, Heisenberg commented thus: "Bohr should (dicitur!) return tonight", WB 387–388.

would probably have helped to clarify the real terms of their disagreement, but it has been lost. In his reply, dated March 10, Heisenberg only informed Bohr in general terms that "I managed to deal with the case where both p and q are determined with some degree of accuracy" and that he had sent a text to Pauli asking for his opinion.[20] Actually, Heisenberg already knew Pauli's judgment, since in a letter dated March 9 he wrote thanking him for his lenient criticism (nachsichtigen Kritik).[21]

Since in the printed article the date on which *Zeitschrift für Physik* received the manuscript is reported as 23 March, it seems quite likely that Heisenberg sent his work without waiting for Bohr's influential endorsement, just returned from Norway. "One may venture the guess – claims in this regard Jørgen Kalckar–that after the many months of preceding discussions, and knowing as he did Bohr's thorough method of working, Heisenberg was eager to get the manuscript off before every paragraph of it would be overhauled by Bohr".[22] Thus Heisenberg meant to avoid further discussion with Bohr or rather, he did not want the publication of his work to depend on comments and criticism that he knew full well and which sprung from a point of view he did not share. It was enough for him to have received Pauli's support, who enjoyed undisputed prestige among the main schools of theoretical physics of the time—Munich, Copenhagen, Göttingen and Berlin—and whose opinion had proven crucial on many occasions.[23] In fact, as Heisenberg himself noted, he sought to know "Pauli's reaction before Bohr was back because I felt again that when Bohr comes back he will be angry about my interpretation"; besides, Pauli's reaction was"extremely enthusiastic".[24]

Heisenberg's concerns were well founded. Not only Bohr "found some trouble in my paper", but as soon as they had the opportunity to speak about the article, in the presence of Oskar Klein, there was a very lively discussion that"ended with the general impression that now Bohr again has shown that my interpretation is not correct".[25] If the discussion had left Heisenberg "a bit furious", Bohr was "rather angry" and over the next days he was to try to explain to his young assistant the reasons for

[20] Heisenberg to Bohr, 10 March 1927.

[21] Heisenberg to Pauli, 9 March, WB 383–384. In fact, at the end of his letter/article of 23 February, Heisenberg had requested a severe critique (unnachsichtige Kritik). On 2 March, with a postcard Heisenberg again asked for a response from Pauli, and demonstrating all his apprehension, advanced even the possibility of his not having received "my letter of 14 pages". Soon Heisenberg was to receive Pauli's overall positive judgment, along with some observations that he took into account when drafting the final manuscript. See in this regard Heisenberg to Pauli, 14 March 1927.

[22] CW6 16.

[23] Due to his undisputed authority Pauli was the negative protagonist of an episode involving the young American physicist Ralph Kronig, who had formulated the concept of electron spin, well before this hypothesis was published on *Nature* in 1926 by Uhlenbeck and Goudsmit. When Kronig had submitted to Pauli the hypothesis of the magnetic moment of the electron, he "ridiculed the idea", arguing that it had nothing to do with reality. In a letter to Kramers, Kronig observed: "To my distinct surprise and my greatest amusement I noticed in *Nature* of Feb. 20 than the electron with magnetic moment has found favor among the theoretical physicists"; concluding with a bitter comment: "In future I shall trust my own judgment more and that of others less", Kronig to Kramers, 26 March. Bohr later expressed his consternation and deep regret to Kronig "that you have not only, more than a year ago, had the same ideas as those published in autumn by Goudsmit and Uhlenbeck, but that you also from the beginning understood their bearing on the puzzle of the relativity doublets", Bohr to Kronig, 26 March 1926, in Bohr (1984, pp. 234–236:235) [henceforth CW5].

[24] Interview of Werner Heisenberg by Thomas S. Kuhn on 25 February, 1963, AIP.

[25] Ibid.

his disapproval, exercising such a strong pressure over him that, Heisenberg recalls, "I shouldn't publish the paper": this situation of great tension "ended with my breaking out in tears".[26] Thirty-five years later, however, Heisenberg managed to soften his recollection of the episode and, more importantly, to narrow it in time. In his memoirs he says it took a few days before "we could settle everything" and for them to agree to publish the article following some improvements that Heisenberg himself had acknowledged as "quite important" And prodded by Kuhn he stated: "I should say that by March in '27 we had already reached complete agreement. So there was only a short period of perhaps ten days or so in which we really disagreed rather strongly".[27]

But things did not go exactly like that as shown by the documentation and by some historical reconstructions. On the one hand, Bohr thought of asking Pauli's direct intervention to help him out of an increasingly embarrassing situation[28]; on the other, Heisenberg involved Pauli too by informing him, on April 4, that they had reached a stalemate regarding the attribution of the origin of the uncertainty relations to the discontinuous or wave aspects of quantum mechanics. "I argue[d] with Bohr over the extent to which the relation $p_1 q_1 \sim h$ has its origin in the wave- or the discontinuity-aspects of qu[antum] m[echanics]. Bohr emphasize[d] that, e.g., in the γ-ray microscope the diffraction of the wave is essential; I emphasize[d]–Heisenberg wrote – that the theory of light quanta and even the Geiger-Bothe experiment are essential. By exaggerating to one side as well as to the other one may argue at length without saying anything new".[29]

The divergences between the two physicists were bridged in mid-May following a new set of long discussions that begun when Heisenberg returned from Easter holidays. In fact, a compromise solution had been found. Heisenberg recognized, inter alia, the error in his γ-ray microscope *gedankenexperiments*, which could have been used to illustrate the validity of the uncertainty relations only when taking into account light diffraction and therefore applying the concepts of wave theory. The discovery of indeterminacy was saved, but Heisenberg acknowledged, "not quite in the manner I thought", and in general he admitted to Bohr that "certain points could be better expressed and discussed in every detail, if only one begins a quantitative discussion directly with the waves".[30] Heisenberg obtained not having to amend the text submitted to the journal and especially not having to delay its publication.[31] The result of the

[26] Ibid.

[27] Ibid.

[28] "I am sending you quickly a couple of lines to ask if you would care to take a short trip to Copenhagen in early April". Bohr did not explain the reasons of the invitation, even if the urgency was betrayed by the offer of providing for travel expenses (Bohr to Pauli, 25 March 1927, WB 388). Perhaps perceiving the real reasons, Pauli declined Bohr's invitation politely informing him that he had already "arranged a pleasure trip of fourteen days with a friend (and I really am in great need of rest), at the moment I truly need to relax and make some order among my thoughts. So do not take offense, please, if I propose of defer my visit to Copenhagen. Maybe I could come in the week of Pentecost, or even in September or October?" (Pauli to Bohr, 29 March 1927, WB 389).

[29] Heisenberg to Pauli, 4 April 1927, WB 390–391.

[30] Heisenberg to Pauli, 16 May 1927, WB 394–396.

[31] In his monumental biography of Heisenberg, Cassidy (1992) argues that the academic events of those months and the choices made by the faculty committees in choosing the candidates for vacant professorships

long debate was to be summarized in an addendum, which among other things would point towards the forthcoming publication by Bohr of "a general treatise on the 'conceptual constitution' of quantum theory from the point of view: 'There exist waves and corpuscles'". In this regard, Heisenberg pointed out that "if one starts right away with this, one can of course also make everything consistent".[32] A sarcastic comment that made it clear that for him this was an armed truce, not least because, he confided to Pauli, despite the outlined mistakes "my opinion is of course just as beforehand that in the quantum theory only the discontinuities are interesting and that one can never emphasize them enough".[33]

The drafts of Heisenberg's article arrived at the Institute for theoretical physics in Easter week and Bohr sent a copy to Albert Einstein, as Heisenberg had asked him to do before leaving for the Bavarian mountains. In the cover letter, a large text of about 1,200 words, Bohr, after presenting Heisenberg's work as "a very important contribution to the discussion of the general problems of quantum theory", entered directly into the merits of the matter, almost as if meaning to suggest an alternative interpretation to that of the author.[34]

2.2 Two paths to uncertainty

Heisenberg's reasoning started from the acknowledgment of quantum mechanics' crisis and in the introduction to his work he drew very clearly the route that he thought should be followed to eradicate those contradictions (Widerspruch) that had hitherto prevented a consistent physical interpretation from being found. First, it must be noted that no significant step in this direction was possible as long as the continuity/discontinuity and wave/particle dualism persisted. Too long had the coexistence with the paradoxes of dualism been accepted, in the hope of coming up with a theory that would include them within a single coherent conceptual and mathematical scheme; a scheme that could, for example, reconcile the idea of Einstein's light quanta with the wave image of radiation. Dualism and its paradoxical results had revealed unbridge-

Footnote 31 continued
at various German universities, Heisenberg's attitude in his dispute with Bohr had a non-negligible influence. The failure to publish another good article could have diminished Heisenberg's academic expectations, who after a complex situation, in October 1927, accepted the chair of theoretical physics in Leipzig. Not surprisingly, perhaps, in the same letter that announced the closure of the 'case', he hastened to ask Pauli: "Did you hear something on the chair of Halle? Write me, please, if you know something! I am very interested in all the rumors on the employment of professorships Berlin-Leipzig-Halle-(possibly Munich?)-Zurich, and so forth". Finishing with a quote from Horace's ode "Omnes eodem cogimur...". Heisenberg to Pauli, 16 May 1927, WB 395–396.

32 Ibid., p. 395. And to witness the state of tension that prevailed in those days in Copenhagen, Heisenberg continued as follows: "Unfortunately, discussions of the last times have created personal gross misunderstanding between Bohr-Klein and me, of which of course I have my share of guilt. Obviously I do not want to write anything about it, but I would ask you to send me the letter [...] that I wrote before 31.1.27, where I spoke of the work of Klein. In fact, Klein believes that I have systematically talked badly with you, Hund and others of his works and that of Bohr. [...] But now let us put aside these petty and unpleasant matters and concern ourselves with yours and Dirac's electrodynamics!".

33 Ibid.

34 Bohr to Einstein, 13 April 1927, CW6 21–24.

able inconsistencies and contradictions that demanded an ultimate choice, opting for one of two competing research programs. It is quite clear that Heisenberg considered only discontinuity and particles as truly essential, and that the only theoretical framework that he took into account was the matrix mechanics developed by the Göttingen school of physics. But if this was the first necessary step towards the definition of the theory's conceptual foundation, a further condition was dictated by the certainty that it was impossible to be content with the classical concepts of mechanics and kinematics of 'position', 'velocity', 'energy', and 'time'.

Heisenberg returned thus to the origins of quantum mechanics, to the summer of 2 years earlier when with a fortunate intuition he had made a synthesis of the Bohr correspondence principle and of Kramers' work on the quantum theory of dispersion, coming up with an original quantum–theoretical interpretation of the quantities of ordinary kinematics and mechanics.[35] But after almost two years, Heisenberg no longer considered observability as the criterion for an *a priori* selection of the quantities with which the mathematical structure of the theory would be able to describe microscopic physical processes. In short, he did not ask how we can describe nature by mathematical schemes. Rather, he thought the reasoning needed to be reversed. Thus, we have a mathematical scheme of quantum mechanics that works, satisfying for instance, conservation laws and Bohr's frequency condition, of significant success in explaining many experimental results (the Balmer series for hydrogen, the spin, the anomalous Zeeman effect, the helium spectrum, the allotropic forms of hydrogen[36]) and above all that needs no revision. This scheme was to indicate which quantities can be defined, if it is true that in nature we can "only find situations which can be described by quantum mechanics". This was an undoubtedly challenging assumption but, as Heisenberg was to recall later, he had simply put to good use the lesson learned from Einstein's theory of relativity.[37] It was precisely from Einstein's authority that he drew a formidable epistemological certainty, one that allowed him to see at once the need of "a revision of kinematical and mechanical concepts"[38] in those very same fundamental equations of quantum mechanics. He only had to consider what Max Born called the 'stronger quantum condition', the first fundamental law of quantum mechanics: $pq - qp = h/(2\pi i)$. While from a classical point of view or in "everyday physics" there is no ambiguity when we speak of the 'position' and 'velocity' of a 'mass' m, in quantum mechanics these words are associated with the symbols p and q that satisfy Born's quantum formula, so the dynamical variables do not obey the commutative law of multiplication. According to Heisenberg, this is enough to make us "suspicious every time uncritical use is made of the words 'position' and 'velocity'".[39]

In fact, it was not a very surprising result. For nearly three decades, since Planck's famous black-body radiation law and his discovery of the quantum of action, atomic physics had grown without being able to resolve the conflict between empirical

[35] Heisenberg (1925). Cf. Beller (1999).

[36] The Nobel Prize in Physics 1932 was awarded to Werner Heisenberg "for the creation of quantum mechanics, the application of which has, inter alia, led to the discovery of the allotropic forms of hydrogen".

[37] Interview of Werner Heisenberg by Thomas S. Kuhn, cit.

[38] Heisenberg (1927, p. 173).

[39] Ibid.

evidence and the classical laws of mechanics and electrodynamics. The reality of the microscopic world had shown that elementary physical processes are characterized by an element of discontinuity completely extraneous to the traditional description and representation model: atoms are stable and the interaction between radiation and matter is governed by so-called quantum jumps. Therefore, it was entirely possible that in areas where physical processes are characterized by an unavoidable discontinuity (Versagen) the ordinary concepts of 'position' and 'velocity' could fail. According to Heisenberg, however, in this context it was legitimate to ask whether a more detailed analysis of said concepts would put an end to those contradictions that had hitherto prevented any kind of interpretation of the new mechanics, finally yielding a visual comprehension (anschaulichen) of quantum formulae.

At this point, Heisenberg introduced a criterion setting out the conditions under which those words or concepts take on some significance. There is no need to renounce the use of terms such as 'position' or 'velocity' and to elaborate, as he had previously thought, a theory that deals only with observable physical quantities; we can certainly talk about the 'position' of an electron provided that the "definite experiments" that allow us to measure the position of the electron are specified. It is therefore the experimental apparatus set up to measure a given physical quantity to determine the conditions which may or may not assign an unambiguous meaning to the concept corresponding to that quantity. Beyond this specific experimental situation it is meaningless to use a given concept and therefore it is not possible to speak in general terms of the 'position of an electron', both before and after a measurement process.

However, that which appeared as a simple operational definition of the concepts resulting from radical empiricism, on further analysis of the experimental conditions was to reveal a fundamental discovery, a glimpse of the path that would finally bring freedom from the contradictions of dualism. If one considers actual experimental situations, it is clear that there are pairs of quantities that cannot be measured with the same accuracy, 'position' and 'velocity', 'energy' and 'time'. It is possible, for example, to measure the position of a particle with as much accuracy as one desires, but then its velocity remains completely undetermined. If we assign a meaning to the concept of 'position' of a particle at a given time, we are obliged to recognize that we can never say that particle, placed at a given point in space–time, has *also* a given velocity: on the basis of the criterion introduced by Heisenberg, the concept of 'velocity' is meaningless. All of this is expressed in symbols in the expression $p_1 q_1 \sim h$, where p_1 and q_1 represent respectively the precision with which the momentum and position of a particle can be determined.

In the formalism used in the article $p_1 q_1 \sim h$ was the well-known uncertainty relation between position and momentum that Heisenberg had obtained by developing the formalism of Dirac and Jordan's transformation theory.[40] The most striking aspect of the whole Heisenberg procedure was that the formula in question expressed exactly the same limitations faced by the quantities p and q when measured with a given experimental apparatus. At this point his famous thought experiment was summoned, the

[40] With this theory, in late 1926, Dirac (1927) and Jordan (1927), without providing any significant contribution to their physical interpretation, had shown that matrix mechanics and wave mechanics represented special cases of a more abstract formulation of quantum mechanics.

oft quoted gamma-ray microscope. Heisenberg's reasoning was as follows: in order to determine the position of an electron we must illuminate and observe it with a microscope; it is evident that, given the small size of the observed object, the accuracy of the measurement of its position will depend on the wave-length of light used. Hence, the smaller the wave-length of light illuminating the electron the greater the accuracy of the measurement will be; by using a gamma-quantum[41] the electron's position can be determined as accurately as desired.

One had, however, to bear in mind that this kind of interactions are subject to the photon–electron scattering that the American physicist Arthur H. Compton had studied years before and published in the *Physical Review* in May 1923.[42] In the so-called Compton effect, the interaction produces a quantum of light that is scattered at an angle θ from the direction of the incident radiation, as well as the diffusion (with an angle φ) of the electron subjected to a discontinuous change of momentum. The simple laws found by Compton, as an application of the theorems of conservation, allowed to affirm, in rigorously quantitative terms, that the smaller the wave-length of incident radiation—so if highly energetic quanta are used—the greater the momentum transferred to the electron. Hence Heisenberg drew the conclusion that "at the instant at which the position of the electron is known, its momentum can therefore be known up to magnitudes which correspond to the discontinuous change".[43] Now it was only left to translate the results of the thought experiment into the symbols corresponding to the various quantities under consideration. If q_1 is the uncertainty with which a quantity q can be determined, it must be exactly equal to the wave-length of incident gamma radiation λ_0; p_1 is but the discontinuous change of momentum of the recoiling electron. From the basic laws of Compton's effect it then turns out, quite trivially, that the product between p_1 and q_1 is equal to Planck's constant h, ignoring a numerically negligible term, which indicates the momentum associated with the scattered quantum of wave-length λ_θ, greater than λ_0.

Once it was shown that $p_1q_1 \sim h$ was "a straightforward mathematical consequence".[44] of Born's quantum condition $pq - qp = h/(2\pi i)$ *via* the transformation theory, Heisenberg also obtained confirmation of the assumption that was at the basis of his reasoning, namely that it was the mathematical scheme that would reveal which situations existing in nature one can describe. He was so convinced of this that he went as far as to claim that "if there existed experiments which allowed simultaneously a 'sharper' determination of p and q than the equation $[p_1q_1 \sim h]$ permits, then quantum mechanics would be impossible".[45] From Heisenberg's point of view it really seemed that his 'law' enjoyed within quantum mechanics the same epistemological status as the constancy of the speed of light within the theory of relativity. Contrary to what was somewhere suggested, the mathematical formalism of quantum mechanics did not require any change to the physical sense (Sinn Physikalische) of dynamic

[41] With wave-length of the order of picometers.

[42] Compton (1923).

[43] Heisenberg (1927, p. 175).

[44] Ibid.

[45] Ibid., pp. 179–180.

quantities p and q, because "*all concepts which can be used in classical theory for the description of a mechanical system can be defined exactly for atomic processes in analogy to classical concepts*".[46] For Heisenberg it was a very surprising result: one had only to abstain from applying those concepts in experimental situations in which their meaning was not defined with exactitude, and above all to respect the constraints imposed by the uncertainty relations. Once again we were faced with a situation not very different from relativity, in which, Heisenberg emphasized, "the word 'simultaneous' cannot be defined except through experiments in which the velocity of light enters in an essential way".[47]

Heisenberg had therefore good reason to be pleased with his achievements, especially for being able to show the way towards the long-awaited transition from microphysics to macro-physics within the framework of matrix mechanics, retaining the concepts of particle and discontinuity. Heisenberg's satisfaction was certainly increased by the fact that in his theory there were no waves, nor was there any mention of Schrödinger's 'popular' wave mechanics, for which he had reserved a contemptuous comment in a footnote.[48] But Heisenberg had not realized that the experiment contained a truly gross error that threatened to undermine the whole theoretical edifice; on this point he was to face a very strong challenge from Bohr.

Bohr objected that what made it impossible to determine the momentum of the electron accurately did not depend upon the discontinuous change of said quantity in the Compton effect. In fact, if it were possible to measure the scattering angle q of the secondary photon with any degree of precision it would become quite easy to obtain, from Compton's laws, the exact value of the discrete change in momentum of the electron. Therefore if our microscope were to allow us to detect the exact direction of the photon produced in the scattering process *à la* Compton, i.e., if it were possible to establish the exact point in which the photon was to enter the microscope, then Heisenberg's thought experiment would not prove the impossibility of simultaneously measuring the position and momentum of the electron with a higher precision than the constraint imposed by the equation $p_1 q_1 \sim h$. Paradoxically, Heisenberg's experiment would show instead precisely those requisites that would necessarily lead to the conclusion that "quantum mechanics would be impossible".

"As long as we only talk about particles and quantum jumps, it is difficult to find a simple presentation of the theory, which is based on a reference to the limitation in the possibilities of observation. This is because the uncertainty mentioned is not only connected to the presence of discontinuities but also to the very detailed description in accordance with those properties of material particles and light that find expression

---

46 Ibid., p. 179.

47 Ibid.

48 "Schrödinger describes quantum mechanics as a formal theory of frightening, indeed repulsive, abstractness and lack of visualizability. Certainly one cannot overestimate the value of the mathematical (and that extent physical) mastery of the quantum-mechanical laws that Schrödinger's theory has made possible. However, as regards questions of physical interpretation and principle, the popular view of wave mechanics, as I see it, has actually deflected us from exactly those roads which were pointed out by the papers of Einstein and de Broglie on the one hand and by papers of Bohr and by quantum mechanics on the other hand", ibid., p. 196.

in the wave theory".[49] This very severe comment was contained in a letter from Bohr to Einstein; surely of the same type were the remarks he was moving in those days to Heisenberg, in an attempt to persuade him that without the wave theory his result was nothing but a brilliant albeit sterile mathematical exercise.

In his experiment, Heisenberg did not take into account that the finite opening of the microscope limits its resolving power and therefore its capacity to exactly localize an object such as an electron. His argument was also flawed in that it ignored the wave properties of scattered quantum which, according to Bohr, were however essential to carry out a proper analysis of the thought experiment. Rather than a criticism, it was a veritable step back because, as revealed in his letter to Einstein, the simultaneous uncertainties of conjugate variables found by Heisenberg could be obtained by another path and precisely by looking at the wave properties of radiation and matter. He pointed out to Einstein that there is a perfect symmetry between them since "the representation of an electron by a group of de Broglie wave is of course closely analogous to the representation of a light quantum by group of electromagnetic waves".[50] Heisenberg had chosen to regard photons and electrons as point-like particles, thinking that in the instant of collision the quantum would transfer a discrete and uncontrollable amount of momentum to the electron.

According to the superposition principle a corpuscle of matter, as well as a quantum of light, can be adequately described by a train of monochromatic plane waves and the wave packet that represents it will be all the more localized the greater the variety of waves involved. The limitation of the extension of the wave field in space and time can always be seen as the result of the interference of a group of elementary harmonic waves. Then we can indicate with $\Delta\nu$ and $\Delta\lambda$, respectively, the differences between the frequencies and wave-lengths of the wave components by which we can assign a finite extension to our corpuscle in the direction of propagation. Due to this 'uncertainty', the geometrical description of waves–and hence the possibility of observing the light quantum – will be subject to the following space-time limitations: $\Delta t = 1/\Delta\nu$ and $\Delta x = \lambda^2/\Delta\lambda$. Taking into account the well-known quantum equations for energy, $E = h\nu$, and momentum, $I = h/\lambda$, we find that the uncertainties concerning the space-time coordination stand in a peculiarly inverse relationship with the quantities E and I that symbolize conservation laws and ensure the claim of causality. Very simple algebraic derivations therefore result in $\Delta E \Delta t = h$ and $\Delta I \Delta x = h$, "all in harmony – Bohr remarked – with the general relation between the simultaneous uncertainties of conjugate variables, which according to Heisenberg is a direct consequence of the mathematical laws of quantum mechanics".[51]

It follows that Heisenberg's important result could have been reached by a theoretically alternative path that did not need strong assumptions about the relationship between mathematics and physical reality, nor operational constraints on the use of physical concepts. From a theoretical point of view it was all written into the wave image of reality, generalized by de Broglie's idea of wave associated with moving

[49] Bohr to Einstein, 13 April 1927, CW6 23.

[50] Ibid.

[51] Ibid., p. 22.

material particles, as well as in the Planckian expression of the quantization of energy. Therefore it was necessary to explore the potential of wave-particle duality thoroughly and especially it needed to be understood, beyond any arising paradoxes, to what degree were the concepts of field theory essential. Yet, as he stressed in the article to *Nature*, everything was contained, according to Bohr, in the simple laws of quantum physics, where the Planck constant establishes a formal relationship between the concepts of energy and momentum—which symbolically express the corpuscular and discontinuous content of microscopic world—and the concepts of frequency and wave number—which are mainly connected with the continuous and wave image of that same world.[52] The paradoxes of dualism were therefore written in the basic formulae of quantum theory and in the 'power' of h to establish a quantitatively defined relationship between concept systems representing contradictory images of reality.

On the basis of the equation $p = h/\lambda$ we know that a single wave 'has' a definite momentum even if, contrary to all intuitive evidence and to the laws of classical physics, this quantity is associated with a non-localized physical object of infinite extension in space. We also know, as mentioned earlier, that from the superposition of a group of waves of different wave-lengths we can represent a body of finite extension which, however, as clearly outlined by the $\Delta p = h/\Delta\lambda$ relation, cannot have a defined momentum. In fact, the more accurately is the momentum of a wave packet defined, i.e., the smaller the uncertainty Δp, the lower will the number of its components be and, if $\Delta\lambda$ is large, the greater the dispersion of the wave packet in space. The precise knowledge of the momentum increases the uncertainty of position.

In his letter to Einstein, Bohr had therefore shown that the uncertainty relations could be obtained by a completely different way; but, in his view, Heisenberg had not even the right to propose an operational definition as a condition to use the classical concepts in a quantum context. In a paragraph of the 1927 Solvay conference dedicated to the interpretation of Heisenberg's uncertainty relations, Bohr apparently pointed out what was the limit, even in philosophical terms, of a theory developed around the belief of a world populated by microscopic particles with their discontinuous behavior. The idea that discontinuous changes of energy and momentum occur during an observation, argued Bohr, does "not prevent us from ascribing accurate values to the space–time co-ordinate, as well as to the momentum–energy components before and after the process".[53] In Heisenberg's view, apart from instruments and their unavoidable disturbance on the observed objects and discontinuous changes of dynamic variables, we could also imagine a world in which classical particles moved along defined trajectories and where the ordinary notion of causality is still valid. These would be legitimate curiosities dismissed however by Heisenberg as nothing but "fruitless and senseless" speculation. For Bohr this was instead the true core of

[52] "In these formulae $[E\tau = I\lambda = h]$ the two notions of light and also of matter enter in sharp contrast. While energy and momentum are associated with the concept of particles, and hence may be characterized according to the classical point of view by definite space–time co-ordinates, the period of vibration and the wave-length refer to a plane harmonic wave train of unlimited extent in space and time", Bohr 1928b, p. 581.

[53] Ibid., p. 583.

the problem of atomic physics, which he was to try to resolve with the notion of complementarity.

2.3 A lesson from a failure

'Anschaulichen'and 'Anchaulichkeit' are terms used often in the writings and discussions of those months. Anschaulichen was the requirement that Heisenberg intended to restore to quantum mechanics showing the limitations that mathematical formulae imposed on the use of 'classical' concepts. Moreover, said term of clear programmatic aim present in the title of *the Zeitschrift für Physik* paper, was a challenge to Schrödinger, who had built a large part of his scientific and academic success precisely on the "populäre Anchaulichkeit" of his wave mechanics. Beyond familiar mathematical concepts and computational tools that could be easily mastered by the vast majority of physicists, Heisenberg saw nothing physically interesting in Schrödinger's work.[54] On the other hand, Schrödinger was not to be outdone, stating proudly that his work had eliminated the irrationality of quantum jumps and wiped out matrix mechanics, a mathematical scheme that we saw he found alarming for his "Abstraktheit" and repulsive for his "Unanschaulichkeit". Always in relation to the word 'anschaulich' Heisenberg confided to Pauli that there still remained significant differences of taste (Geschmacksunterschiede) between him and Bohr, despite the agreement reached for the publication of the uncertainty paper.[55] Bohr did not think it a matter of different philosophical inclination, but rather something deeper on the theoretical and conceptual ground concerning the role that it was necessary to ascribe to Schrödinger's new theory; in fact he pointed out to Einstein that "... it seems to me instructive always to keep in mind how indispensable are the concepts of the continuous field theory in the present stage of science".[56]

The letter written to Einstein on 13 April 1927 marked in some way the Danish physicist's return on the main scene of theoretical debate: ultimately he should be grateful to Heisenberg for having offered him, albeit indirectly, that opportunity. Bohr had been absent from said scene for over 2 years, ever since–as he had learned from Born–news started circulating about the results of the Bothe–Geiger experiment "which has apparently turned out in favor of light quanta".[57] That experiment proved, beyond any reasonable doubt, that in the scattering *à la* Compton there is a coupling (or time coincidence) between the recoiling electron and the diffused gamma ray. An experimental result that dramatically contradicted the hypothesis put forth a few months earlier by Bohr, Kramers and Slater on the purely statistical validity of the

[54] Schrödinger's wave mechanics had caused immediate interest within the scientific community. Arnold Sommerfeld and Wilhelm Wien invited him to hold two conferences in Munich at the end of July 1926. Heisenberg would not miss the event and having listened to Schrödinger illustrating the new results of the wave mechanics intervened in the debate by declaring a deep skepticism for a theory that was not able to account for many experimental results. But in a letter a few days later (28 July) to Jordan he admitted that "the mathematics of Schrödinger represents great progress".

[55] Heisenberg to Pauli, 16 May 1927, cit.

[56] Bohr to Einstein, 13 April 1927, CW6 23.

[57] Born to Bohr, 15 January 1925, CW5 302–304.

conservation laws, or rather the non-applicability of said laws to individual physical processes. And justifiably, wrote Born, "Einstein was triumphant".[58]

In the article written in 1924 in collaboration with Kramers and the young American physicist John Slater, Bohr had introduced a new quantum theory of radiation that would allow "to arrive at a consistent description of optical phenomena by connecting the discontinuous effects occurring in atoms with the continuous radiation field in a somewhat different manner from what is usually done".[59] These few words contain the announcement of a breakthrough: the contrast between continuity and discontinuity was no longer an insurmountable obstacle to the formulation of a coherent theory and, most importantly, it seemed to open the way to finally getting "a picture as regards the time-spatial occurrence of the various transitions processes on which the observations of the optical phenomena ultimately depend".[60] However Bohr thought it was necessary to overcome the previous model-like approach to the atom in order to consider the connection between radiation field and atomic system in a totally different way from the one followed hitherto. Here the correspondence principle came into play since, according to the new formulation contained in the conference held by Bohr in September 1923 at the British Association Meeting in Liverpool,[61] it would allow "comparing the reaction of an atom on a field with the reaction on such a field which, according to the classical theory of electrodynamics, should be expected from a set of 'virtual' harmonic oscillators with those [concerning] the various possible transitions between stationary states". [62]

This kind of comparison was contained in two new hypotheses. First, it was stated that an atom, even when in a stationary state, is able to "communicate continually with other atoms through a time-spatial mechanism which is virtually equivalent with the field of radiation which on the classical theory would originate from the virtual harmonic oscillators corresponding to the various possible transitions to other stationary states".[63] Secondly, it was recognized that the transition processes in a given atom, as well as those that occur in atoms with which it is in mutual communication, are governed "by probability laws which are analogous to those which in Einstein's theory hold for the induced transitions between stationary states when illuminated by radiation".[64] There are two keywords that shed some light on propositions clearly distant from the usual theoretical standards, 'correspondence' and 'probability'. In the first

[58] Ibid.

[59] Bohr et al. (1924, p. 786).

[60] Ibid., p. 791.

[61] In light of the correspondence principle Bohr took the opportunity to reformulate the second postulate of the theory, and where it had so far been impossible to ascribe the nature of the radiation to motions within the atom, he now argued that "a process of transition between two stationary states can be accompanied by the emission of electromagnetic radiation, which will have the same properties as that which would be send out according to the classical theory from an electrified particle executing a harmonic vibration with constant frequency" Bohr (1923).

[62] Bohr et al. (1924, pp. 789–790).

[63] Ibid., p. 790.

[64] Ibid., p. 791. The second assumption referred explicitly to those probabilistic considerations with which Einstein, in 1917, had obtained a new derivation of the Planckian formula of thermal radiation from the definition of the probabilities associated with possible quantum transitions, Einstein (1917).

case, the concept of correspondence provides some logical relationship between two theoretical terms that, although both essential for the understanding of atomic processes, are in striking contrast with each other: on one hand, the 'virtual' oscillators, the systems of electric charges vibrating at the frequencies characteristic of the processes of emission and absorption of radiation and, on the other, the stationary states endowed with a hypothetical stability in which the atom is located both before and after each radiation process. In the second case, the concept of probability implies the impossibility in the individual processes of some causal connection between radiation and motion of charges within the atom, so that illumination would only 'act' on an atomic event through a probabilistic mechanism.

The model of virtual oscillators therefore contains the possibility of providing a 'classical' description of the interaction between radiation and matter; in fact there are systems of virtual charges in it which oscillate at the frequencies of the permitted transitions from a given state and associated virtual fields that ensure communication between an atomic system and the other, 'intervening' on the probability of occurrence of a given radiative process. But the price to be paid for this result is particularly high and likely to assail the very foundations of classical physics. The only way to reconcile this space–time description with the reality of individual processes governed by probability laws is in fact the abandonment of "any attempt at a causal connection between the transition in distant atoms, and especially a direct application of the principles of conservation of energy and momentum, so characteristic for the classical theories".[65] The validity of such principles, even while apparently supported by a very large experimental evidence, could only be, according to the authors, the outcome of a statistical average over a large number of individual events for each one of which, however, those principles are not strictly satisfied.

The model therefore implied the violation of the conservation laws and undermined the concept of causality, but, the article concluded, this seems to be "the only consistent way of describing the interaction between radiation and atoms by a theory involving probability considerations".[66] It was not an epistemological stretch, but the logical consequence of the unusual relationship of correspondence that Bohr had established between the new atomic model, founded on the virtual oscillator concept, and the observable properties of quantum objects. That model was based on the assumption that transitions, or, to use a more appropriate language to the model, the transition from a system of oscillators to another was a probabilistic process, and also that the fields generated by the oscillators were the ones that determined their probability; indeed, with such limitations, it is absolutely true that the model was able to provide a space-time picture of the interaction between radiation and matter.

Bohr had abandoned the mechanical model of the atom and newly formulated the correspondence principle, no longer associating the occurrence of a radiative process to the existence of a given harmonic frequency of the electron motion.[67] The concepts

[65] Bohr et al. (1924, p. 791).

[66] Ibid., pp. 792–793.

[67] The correspondence principle is perhaps one of the least clear and therefore most controversial aspects of Bohr's theoretical production between 1913 and 1927. Historians have interpreted it in different and often conflicting ways, but rarely have they provided convincing versions and above all ones able to clarify

of virtual oscillator and virtual field were not accompanied by a new, more effective model, able to provide a viewable image of the atomic system. The objective that Bohr wished to pursue with this new theoretical hypothesis was of a completely different nature. In other words, his was not some odd ploy to salvage a continuous image of the radiation field in the presence of a phenomenal reality marked by an unavoidable quantum discontinuity. It was necessary to deal with the inherent weakness of the quantum theory, facing the fact that despite encouraging progress and experimental confirmations it was still burdened with severe interpretative limits. For example, the theory was unable to explain any part of the mechanism underlying the interaction between radiation and matter and moreover the meaning of the very concept of stationary state remained shrouded in the thickest of mysteries—a sort of 'waiting place' between one process of interaction and another, to use Bohr's own words. The new theory seemed to disclose an important perspective by allowing to state that if in reality there were a mechanism corresponding to an electromagnetic field, or in other words if it were possible to give a space–time description of the interaction between radiation and matter, then the model of virtual oscillators and the principle of correspondence would show, even if in total ignorance of the nature of that mechanism, which principles of conservation must be violated and also that we should impose strong restrictions on the use of the category of causality. This is a scientific statement that had a clearly falsifiable content for Bohr and not just for him.

Bohr was right to consider it thus, but perhaps at first he expected the results of the experiments not to be so ungenerous with his theory by showing, as both Bothe–Geiger and Compton–Simon had done, the absolute lack of grounds of the expected violation of the conservation principles.[68] The great majority of physicists read the results of those experiments as a final sentence against the continuous image of radiation, which Bohr would persist in defending even at the cost of renouncing the conservation laws, also as strong evidence in support of Einstein's idea of light quanta. The undisputed authority of the Danish physicist within the international scientific community seemed to be faltering under the weight of a sensational theoretical failure. But as it turned out, in his answer to Geiger Bohr preferred to emphasize that the aim of this work—certainly not a "completed theory"—was mainly an endeavor to attain the greatest possible applicability of classical concepts. To Geiger these words may likely have seemed as a humanly enough attempt to mitigate the extent of his defeat. And so must have thought those physicists who had gone through the frustration of trying to decipher the meaning of a theory speaking of virtual entities, of systems that exchange information without energy transport, of strange probabilistic mechanisms and of generic correspondence relationships.[69]

Footnote 67 continued
its fundamental role in the construction of quantum mechanics. A role, moreover, that was explicitly recognized by the protagonists of those events, Kramers. Heisenberg. Born and of course Bohr himself. To this subject I dedicated my essay on atoms, metaphors and paradoxes, with an interpretation of that principle that attempts to restore a comprehensible image of an otherwise unintelligible mosaic, Petruccioli (2006).

68 Bothe and Geiger (1925);Compton and Simon (1925).

69 Even Bohr's closest collaborators had been mystified by his theoretical turning point and by his use of completely unconventional concepts. Even before he had read the article Pauli wrote to Bohr: "I laughed a little (you will certainly forgive me for that) about your warm recommendation of the words 'communicate'

Actually, with those words Bohr was disguising no embarrassment; indeed, he revealed with great intellectual honesty which lesson was to be learned from the results of that experiment. Nature had not expressed itself in favor of a corpuscular image of radiation. Rather, it showed the limits of applicability of classical concepts for the first time. In the microscopic world, characterized by discontinuous processes, the space–time description of events forbids any attempt to apply the conservation principles. In other words, Bohr understood through the theory of virtual oscillators, that the concepts of space and time were in conflict with the concepts of energy and momentum, or rather that said concepts could not be used simultaneously to describe the interaction processes between radiation and matter as in classical physics. Apparently, the dual nature of radiation was not destined to crystallize in an insuperable contradiction paralyzing quantum theory. Instead, dualism produced paradoxes that would show up quite clearly, if only it were grasped that they arose from an incorrect application of classical concepts in the context of quantum processes.[70] It was an important clue that Bohr was to exploit in the following months in light of the developments of matrix mechanics and of Schrödinger's wave mechanics, and especially after the experimental confirmation of Louis de Broglie's wave conception of matter.

3 [10-7-1927]?

3.1 The conceptual framework

After the above necessary survey of the historical and theoretical context in which Niels Bohr's 1926–1927 work was set, we can go back to the mss26, attempting first of all to find some kind of logical thread between those scattered and apparently disorganized notes. Let us leave the third sheet aside for the moment.

In the first two items, Bohr asserts as an axiom of quantum theory the central role played by classical concepts, as well as their insuperable role as elements of a language that allows us to 'read' microscopic world phenomena and to collect the information from which to formulate any possible interpretation. But those concepts, he says, besides being the lexicon of physics, have the peculiarity of being defined only in respect of space–time images; in other words, they can take on a clear meaning provided that they are consistent with a space-time description method of events in

Footnote 69 continued
and 'virtual'. [...] On the basis of [my] knowledge of these two words [...] I have tried to guess what your paper may deal with. But I have not succeeded", Pauli to Bohr, 21 February 1924, WB 146.

[70] During a long interview with Thomas Kuhn, Heisenberg focused on a distinction he deemed essential in the evolution of Bohr's thought, between 'inconsistency' and 'paradox'. Regarding the conflict that existed between quantum mechanics and classical physics, he noted: "Of course, this paradoxical situation looks like inconsistency, and it was very difficult to understand or to realize that one could have a theory which was completely consistent and at the same time contained all the paradoxes. [...] On the other hand, in the course of the development, Bohr realized that an inconsistency is something still much worse that a paradox because inconsistency means that you talk nonsense–that you do not know what you are talking about. A paradox may be very disagreeable but still you can make it work. An inconsistency can never be made to work", Interview to Werner Heisenberg by Thomas S. Kuhn, cit.

the physical world. ["All information about atoms expressed in classical concepts / All classical concepts defined through space-time pictures".[71]]

From the beginning—and we are at the third item—quantum theory has had to deal with a bitter and apparently insurmountable conflict inherent to the above axiom, keeping the use of space-time images limited to local situations (in Danish Bohr speaks of "stykkevis Brug") with wide approximation margins. This is because Planck's constant appears in all fundamental relationships of quantum physics, essential to ensure the validity of conservation laws even in microscopic processes.

In the next item, a total of 13 handwritten lines, Bohr summarizes his research program as follows: (a) the recognition that between the space–time images and quantum laws only non-continuous and statistical connections may be established—as it was effectively contained both in the postulates of quantum theory for hydrogen and in Einstein's derivation of the so-called Planckian law of black body radiation; (b) the formulation of the correspondence principle as a consequence of the recognition of an important numerical coincidence between classical and quantum theory in the high quantum numbers area in which "in statistical applications one may disregard the discontinuous element"; finally, (c) the failure of the project, supported by the correspondence principle, aimed at achieving a quantitative treatment of atomic phenomena based on space–time images. In the latter case, the reference to the fields and virtual oscillators and to the far from exciting results of the Bohr, Kramers and Slater theory is more than a mere coincidence. At least 12 months had passed since he had written to Geiger that that theory was primarily an "endeavor to attain the greatest possible applicability of the classical concepts" and that several evidence seemed to "exclude the retention of the ordinary space–time description of phenomena". Bohr now saw the failure of the BKS theory taking a clearer meaning, allowing him to dispel any doubts. "The theory exhibited a duality when one considered on the one hand the superposition principle and on the other hand the conservation of energy and momentum".[72] The incompatibility between the classical image of the wave field and the laws of conservation in individual processes, as demonstrated by irrefutable empirical evidence, did not share the fate of a particular model, nor was it the result of an obstinate defense of the classical image of radiation. According to Bohr, it was rather an unavoidable feature of the nature of any quantum object.

The first sheet of mss26 ends with a statement that completes the initial postulate. If classical concepts are and remain the language with which we can describe our experiences and give meaning to information gathered from our experimental devices, the quantum world and Planck's constant forces us to acknowledge the existence of "complementary aspects of experiences that cannot be united into a space-time picture based on the classical theories".[73] For the first time, the notion of 'complementarity' is mentioned in Bohr's writing.

Apart from mathematical formulae, which could also have been inserted in the text at a later time, the few lines collected in the second sheet contain three statements,

[71] CW6 61.

[72] Ibid.

[73] Ibid.

the last one of which is of the utmost importance. First, Bohr returns to the conflict that exists among the space-time description models typical of classical physics and quantum phenomena due to the contrast between the concepts of wave theory of radiation and the hypothesis of light quanta: "Light waves require time for their definition"; "Particles must change velocity rapidly" and "Light quantum cannot be made sufficiently small". Secondly, de Broglie's assumption of wave matter produced a radical change in the interpretative framework since it extended dualism, hitherto confined to discussions about the nature of radiation and the atom's radiative behavior, to the whole quantum world. Even a material particle, an electron, can be described "by superposition of waves", and the same relations introduced for light quanta apply to it. In addition to de Broglie, only two names appear in the manuscript: Einstein, without any comment, but probably intended as a reference to his 1924 article[74] in which, in order to explain the terms of a formula which governs the fluctuations of a Bose gas, he suggested that the cavity be filled with standing waves. The second name is obviously Clinton Davisson. The manuscript reads literally: "Change with wave theory of matter. De Broglie. Einstein. Confirmation by Davidson [*sic*]".[75]

The mss26 concludes with a statement which we shall comment in the last paragraph, after having dispelled any and all nodes connected to the document's dating.

4 The third sheet

As mentioned above the mss26, archived in the 'Como lecture II' folder, consists of three sheets, the last one of which is undated. A brief examination of the text is enough to realize that this third sheet is an independent document; furthermore, there are reasons to believe that it was not written at the same time as the preceding two pages, such and so many are the differences—even formal ones—between them (a graphology analysis could support this claim). My opinion is based mainly on the content and sequence of topics treated by Bohr, always in form of notes. The sequence of items is almost identical with that of the first page of mss26. Reference is made to the essential use of classical concepts, to their definition in space and time, to the difficulty of a classical space–time description in the quantum context, to the formulation of the correspondence principle and to the outcome of the theory of virtual oscillators with the asserted statistical validity of the principle of conservation of energy and so on. I don't know the reasons that led the editors of Bohr's works to place such clearly different manuscripts together, nor do I understand Jørgen Kalckar's reasoning in his decision to publish them jointly in volume 6 of the *Collected Works*, thus implicitly giving credence to the thesis that they could be a single document. But there can be no doubt as to the papers' lack of logical connection, and in light of the quotation of Heisenberg's investigation on the conditions for measurements, the third sheet may quite likely be traced back to the early months of 1927.

---

[74] Einstein (1924). After having read in December the Thèse of de Broglie, Einstein returned to the subject with a second article with the same title, Einstein (1925). In the context of his discussion on the interference term in the energy-fluctuation formula, Einstein examined the de Broglie hypothesis in detail in which he saw much more than "merely an analogy".

[75] CW6 62.

4.1 Comparison with the other documents

Before tackling the enigma associated with the naming of American physicist Clinton Davisson in mss26, we should delve further into the texts to see whether or not there is a congruence with the other three documents written by Bohr from June to October 13, 1927. According to the new date attributed by Kalckar, the 10 July 1927 manuscript should be placed sometime immediately after the note written by Bohr for *Nature*, stimulated by the discussion developed in the columns of that same journal between Norman Campbell and Pascual Jordan on the philosophical foundations of quantum theory; furthermore, it should precede by a few weeks the outline prepared for the Como conference (13 September), and the notes for his Solvay Conference speech (12 and 13 October). If so, it would seem very odd that only in mss26—please remember that according to Kalckar this is the summer of 1927—Schrödinger's name is completely ignored, the wave theory of matter is associated only with the name of de Broglie and above all no explicit reference is made to Heisenberg's uncertainty paper, published just over a month before and which had involved Bohr for so long. Even harder, if not downright impossible to explain, is the quite different interpretative perspective present in these documents; it would rather seem to point towards an evolution between Bohr's theoretical mss26 and other quoted manuscripts, for which the events of those months could not have been irrelevant: the meeting with Schrödinger in Copenhagen, the long and exhausting confrontation with Heisenberg and finally, the complementarity hypothesis with which he thought to avoid the paradoxes of dualism. There are two theoretical premises informing the whole treatment of the three manuscripts dating back to 1927 and that are certainly completely missing from mss26: the quantum postulate and the consequent recognition that it is necessary to impose a restriction to classical concepts when applying them within a quantum context. The quotes of the aforementioned manuscripts speak for themselves.

In the note written for *Nature*, after having brought back the well-known fundamental difficulties in the description of atomic phenomena based on Planck's discovery "to the contrast between the principles underlying the ordinary description of natural phenomena and the element of discontinuity characteristic for the quantum theory", Bohr claims that "we must be prepared that every concept used in accounting for the experimental evidence will have only restricted validity when dealing with atomic phenomena".[76]

In the manuscript of 13 September these concepts are explained further. Bohr speaks openly of a postulate that expresses "the essence of the theory": "any atomic process open to direct observation involves an essential element of discontinuity or rather individuality completely foreign to the classical idea and symbolized by Planck's quantum of action". And picking up a concept he had formulated a few weeks before, he said that a "characteristic of the quantum theory is the acknowledgment of a fundamental limitation in our classical physical ideas when applied to atomic phenomena".[77]

[76] Ibid., p. 69.

[77] Ibid., p. 75.

With small and irrelevant changes in terminology, we find those same assumptions in the text of 13 October. The essence of the theory may be expressed by the so-called quantum postulate "which to any atomic process open to direct observation attributes an essential discontinuity or rather individuality completely foreign to the classical theories and symbolized by Planck's quantum of action". Furthermore, "the quantum theory is characterized by the acknowledgment of a fundamental limitation in the classical physical ideas, when applied to atomic phenomena".[78]

These premises will gain a more comprehensive treatment in the large article that Bohr was to publish in the Supplement of *Nature* in April 1928[79], after having brilliantly passed the test of the Solvay Council and above all after successfully demolishing each of Einstein's subtle objections.

This paper too starts with the observation that the characteristic feature of quantum theory is that classical ideas, concepts and images of mechanics and electrodynamics are subject to strong limitations when applied to the study of atomic phenomena. These difficulties have weighed on thirty years or so of atomic physics research, keeping it caged in what looked increasingly like an insurmountable contradiction whenever dealing with phenomena in which Planck's quantum of action intervened in an essential way. However, Bohr points out, the evolution of knowledge and the developments of quantum theory especially in recent years have emphasized the existence of a paradox: those concepts, the classical ideas subject to major restrictions in the context of quantum theory, are still essential and irreplaceable for our interpretation of experimental material. Concepts like 'position' and 'velocity' are therefore the terms in which we can, and probably must, continue to talk about what we observe; they are part of that language with which we can decipher, even in the context of micro-physics, the answers provided by nature to our experimental research. The origin of this paradox is far from obvious and arises above all from the belief, developed by Bohr after the 'catastrophic' outcome of the BKS theory, that it was not necessary to use a new conceptual apparatus, but rather to be prepared for "a fundamental revolution in the concepts upon which the description of nature has been based until now".[80]

As he had done in 1913, when he had attempted to reconcile the radiative properties of the atom with the stability of its structure, here too Bohr 'escaped' the paradox by placing a postulate at the theory's summit. In 1913, he postulated the mechanical stability of atoms against all theoretical evidence, associating a discontinuous variation between states to their radiative behavior. The new 1927 postulate finally grasped the essence of Planck's great revolution early on in the century, concerning a matter that was entirely foreign to classical theory. That postulate was, therefore, the element

[78] Ibid., p. 91.

[79] Nevertheless, Bohr's ideas elicited not a few reserves within the scientific community. The editor of *Nature* became the interpreter of this widespread unease, with a note accompanying the publication of the article that ended with this eloquent account: "It must be confessed that the new quantum mechanics is far from satisfying the requirements of the layman who seeks to clothe his conceptions in figurative language. Indeed, its originators probably hold that such symbolic representation is inherently impossible. It is earnestly to be hoped that this is not their last word on the subject, and that they may yet be successful in expressing the quantum postulate in picturesque form", Bohr (1928b, p. 579).

[80] Bohr (1925), „Nachscrift" (Juli 1925), 155.

of separation between macro- and micro-physics, stating that any observable atomic process is characterized by a discontinuity or essential individuality which finds its most effective symbolic expression in Planck's quantum of action.

The dissimilar nature of the documents obviously makes it impossible to go any deeper in the comparative analysis of texts. However, I believe that the evidence gathered is sufficient to question the legitimacy of the choice made by the book's editor to change the date reported by Bohr in his manuscript. Therefore, we need only fit-in the last element of the puzzle, trying to explain why Davisson's name appeared in a note written by Bohr in July 1926.

5 A matter of years: Mr. Davisson's puzzle

"It will be a second honeymoon". With such words of enthusiasm and longing, Clinton Davisson announced to his wife Charlotte their imminent trip to England in the summer of 1926, for the 94th meeting of the British Association for the Advancement of Science to be held in Oxford from August 4 to 11 1926. After many years of intense activity in the laboratories of the Engineering Department at Western Electric (the future and famous Bell Telephone Laboratories), Davisson thought of a relaxing break, free, among other things, from heavy family burdens. He never imagined this trip to be crucial for his research work and for a discovery that would earn him much prestige among the international scientific community.

Clinton Davisson was a brilliant and skillful experimental physicist with an impressive background. His mentors were three Nobel prize winners: Robert A. Millikan, who won the prestigious award in 1923 for his studies on the electron and the photoelectric effect, Owen W. Richardson, awarded the Nobel in 1928 for his work on the thermionic phenomenon and finally Joseph J. Thomson, Nobel laureate in 1906 for his investigations on the conduction of electricity by gases. After receiving his Ph.D. at Princeton with Professor Richardson, Davisson had spent the summer of 1913 at the prestigious Cavendish Laboratory, directed by Thomson.[81]

Davisson was to obtain the Nobel in 1937, in recognition of his "experimental discovery of the diffraction of electrons by crystals",[82] a brilliant work he had carried out in collaboration with Lester H. Germer, published on the *Physical Review* in December 1927.[83] It was his due for an experimental result that had showed the wave nature of matter beyond any doubt, as assumed by Louis de Broglie in 1924 and whose

[81] Cf. Kelly (1962) and Gehrenbeck (1978).

[82] Davisson shared the Nobel Prize with George Paget Thomson who obtained equally significant evidence by a different experimental procedure to support the hypothesis of de Broglie waves of matter. At the Nobel Lecture, Thomson describes his discovery with these words: "A narrow beam of cathode rays was transmitted through a thin film of [...] metal. The scattered beam was received on a photographic plate normal to the beam, and when developed showed a pattern of rings, recalling optical halos and the Debye–Scherrer rings well known in the corresponding experiment with X-rays. An interference phenomenon is at once suggested", Thomson (1965, p. 389). Contrary to Davisson, Thomson had used a beam of fast electrons produced in a cathode-ray tube and the scheme of his experiment reproduced that Elsasser had tried in vain to propose to Franck in Göttingen laboratory. See, also for bibliographic references, Russo (1981) especially § 3.

[83] Davisson and Germer (1927b). First results of the experiment, however, were contained in the letter to *Nature* dated 3 March; Davisson and Germer (1927a).

solid conceptual foundation lay in the context of Schrödinger's wave mechanics. In his Nobel Lecture, Davisson attributed to Walter Elsasser, a young student of physics in Göttingen, the merit of having been the first, in 1925, to draw attention to the fact that "beams of electrons like beams of light would exhibit the properties of waves, that scattered by an appropriate grating they would exhibit diffraction"[84] In short, the experimental demonstration of diffraction had decreed the physical existence of the electron waves. But then, with great intellectual honesty and with considerations that had surely been surprising for all those present, he was to reconstruct the genesis of his great discovery as follows.

"It would be pleasant to tell you that no sooner had Elsasser's suggestion appeared than the experiments were begun in New York which resulted in a demonstration of electron diffraction – pleasanter still to say that the work was begun the day after copies of de Broglie's thesis reached America. The true story contains less of perspicacity and more of chance. The work actually began in 1919 with the accidental discovery that the energy spectrum of secondary electron emission has, as its upper limit, the energy of the primary electrons, even for primaries accelerated through hundreds of volts; that there is, in fact, an elastic scattering of electrons by metals. Out of this grew an investigation of the distribution-in-angle of these elastically scattered electrons. And then chance again intervened; it was discovered, purely by accident, that the intensity of elastic scattering varies with the orientations of the scattering crystals. Out of this grew, quite naturally, an investigation of elastic scattering by a single crystal of predetermined orientation. The initiation of this phase of the work occurred in 1925, the year following the publication of de Broglie's thesis, the year preceding the first great developments in the wave mechanics. *Thus the New York experiment was not, at its inception, a test of the wave theory*. Only in the summer of 1926, after I had discussed the investigation in England with Richardson, Born, Franck and others, did it take on this character".[85]

It was no affectation of modesty. The story was really witness to an incredible sequence of fortuitous events, and some accidents did take Davisson to success as if by magic. At the Western Electric laboratories, Davisson carried out research that had a declared applicative aim, intended to study the conditions (maximum of reliability, long life and the highest electron-emitting efficiency from the cathode) for the full utilization of the thermionic high-vacuum tube in the field of telecommunications. In particular, in the years following the end of the First World War, his studies had been aimed at understanding an effect, the secondary electron emission from the grid structure of the vacuum tubes, that was considered responsible for their malfunctioning and unreliability.[86] As chance had it, a tube exploded during an experiment and the sample of nickel used as a target underwent a series of manipulations to remove the impurities. This led to a change in its crystalline structure passing from a random configuration to a perfectly regular lattice. Once that the experimental device was restored, an entirely new angular distribution of scattered electrons resulted from the

[84] Davisson (1965, p. 390).

[85] Ibid., my emphasis.

[86] Cf. Russo (1981, § 2).

surface of single crystal nickel: it showed maximum and minimum intensity typical of a diffraction pattern (the so-called quantum bumps), absolutely inexplicable according to Davisson's own atomic model.[87]

As Davisson recalled, there followed years of intense work in the laboratory with complex and progressive refinements of the experimental device and with equally accurate variations in the observation conditions. Because of this unusual laboratory procedure some 'anomalies' previously observed in experiments that Davisson had conducted in collaboration with his research assistant Charles H. Kunsman were confirmed; in April 1923, the *Physical Review* had published their full-bodied article on "The scattering of low speed electrons by platinum and magnesium".[88] When using a platinum target, measurements revealed a pattern for which the authors simply stated that "the means of analyzing distribution curves in terms of the present theory are not sufficiently developed to permit of a quantitative consideration of pattern as complex as these exhibited by platinum". Despite the favors of chance, it was really like groping in the dark.

Across the ocean, in the heart of Mitteleurope, at the Faculty of physics in Göttingen, ideas were nevertheless a little clearer, but unknown to Davisson very heated discussions were developing around his research. Or so it seems according to the reconstructions made by the main players of the time decades later: Max Born, James Franck, Friedrich Hund and Walter Elsasser. We have, *inter alia*, three reconstructions of the same 1925 events, contained in the personal interviews collected in 1962 by Thomas Kuhn and John Heilbron: 29 May with Walter Elsasser at the Scripps Institute of Oceanography in La Jolla, California, 12 July with James Franck in Falmouth, Massachusetts, and finally 17 October with Max Born, and the participation of Friedrich Hund,[89] at Bad Pyrmont in West Germany. With the exception of some obvious and gross loss of memory affecting mostly Elsasser's account,[90] despite the diversity of

[87] Davisson (1923).

[88] Davisson and Kunsman (1923).

[89] Friedrich Hund studied in Göttingen where he received his PhD in 1922. He remained there until 1926, when he moved to Copenhagen. Born, who along with Franck was one of his teachers, so wrote to Bohr, "Now I still have [a] request for you. My assistant Dr. Hund has a great desire to spend some time at your Institute, and I think nothing could be more beneficial for his training as a period spent under your influence. [...] So I thought I'd send Hund to Copenhagen for six months next summer, with a Rockefeller grant, if you are willing to receive him at the Institute. As to Hund's character, Heisenberg can inform you better than I can, in fact the two are close friends. Let me just tell you that Hund is the perfect assistant, always available, able, energetic, of ready intelligence and remarkable talent. On the scientific level he is not up to Heisenberg's–that would be unthinkable–but he is a person of great acumen and critical thinking, and a vast knowledge" Born to Bohr, 10 October 1925, in CW5 311–313: 313.

[90] From the oral history transcript of Elsasser it is sufficient to mention two striking errors. First, referring to the article by Davisson and Kunsman (1923) he speaks of "a little paper". Actually, the memoir of the two American physicists is a broad and thorough study, with a detailed description of the device and the experimental procedures used and accompanied by drawings and charts, occupying 17 pages of the *Physical Review*. Secondly Elsasser said to have come to Göttingen in 1921, i.e., at the age of 17. But as Harry Rubin documented in detail–Rubin (1995)–Elsasser enrolled in 1922 at the University of Heidelberg where Nobel Prize Phillip Lenard taught at the time, famous supporter of the Nazi ideology and openly anti-Semitic; in 1923, Jewish Elsasser moved to the University of Munich to the school by Wilhelm Wien and Arnold Sommerfeld; only at the beginning of 1925, again due to political and racial turbulence, did he move to the safer University of Gottingen.

mentioned details—quite reasonable at a distance of about 37 years from the events—it is possible to achieve, thanks to the objective documentary evidence, a credible and accepted reconstruction. Apart from some clarification that will be introduced later, it is however possible to take Max Born's testimony as a fully reliable basis of how things must have gone within the rooms of the Faculty of physics in Göttingen.

The discussion was introduced by Friedrich Hund who, urged by Kuhn, pointed out that in talking over the anomalies highlighted by the so-called Ramsauer effect[91] it had been Franck himself, at the time holding the chair of experimental physics in Göttingen, who had grasped within it the first indications for electron waves. And this is where Born's account begun.

"The first indication of the waves came from a letter I got from Einstein. And it was quite a short letter. I haven't got it anymore, but I remember he wrote, "I am quite excited about a paper by a young fellow in Paris, de Broglie, and about waves which correspond to particles in the same way as photons correspond to the waves in optics. And you must read it". And now it wasn't so easy to get it from Paris, so at last I wrote to de Broglie. I got the thesis with his own dedication–but I have given it with my library to America. And then I tried to read it, and it was very impressive. But I didn't think about how to verify it experimentally at all. I thought that it was only abstract theory—an abstract idea. But this consideration that to the hn of Planck belonged also an 'h' times momentum was so convincing. And then came a letter from Davisson. He sent me a letter with some photographs of deflection experiments of electrons by Nickel, I think, as far as I remember. And there were diagrams which showed anomalous maxima in different directions. And I looked at it, and I thought, well, that's not very remarkable such a crystalline lattice, and in different directions there are different forces so why shouldn't there be such anomalies. I showed it then a week later to Franck, and I told Franck about this letter from Einstein. And Franck became very worried and said, "Now I don't believe these are just responses from different forces. Don't you remember what you have told me about de Broglie's paper which you learned of from Einstein?" And then he sat down and combined these things, and he said, "Well I should like to try". And we had hours of discussion on how one could find a simple criterion, and then at last quite simply we hit on the idea of the connection between momentum and reciprocal wave length. And I made quite a rough estimate in my mind and said to Franck, "It seems to be in the right order of magnitude". "Then give it one of your boys" he said. I said, "I have none now; we are all busy". And then Franck came to me the next day and said, "Oh, I have one I want to get rid of, and he's the right man for it". That was Elsasser. He was an experimentalist, but Franck was always exasperated about his inability to tackle simple experimental things. And so he wanted to shift him to me. I didn't know him, but when he came to me, I found him a very nice and attractive fellow and very clever. So I suggested to him this problem. And I said I had quite a crude estimate and thought it might be

[91] In studying the process of diffusion of slow electrons passing through a gas Carl Ramsauer focused on a non-justifiable behavior in the light of the classic model of description in which electrons are treated as point-like particles: Ramsauer (1921). Hund, as recalled in his interview with Kuhn, had discussed the results of the Ramsauer experiments in his doctoral thesis attained at Göttingen in 1922; in Interview to Max Born by Thomas S. Kuhn, on 17 October 1962, AIP.

right and that he would have great success if this was right. And in a very short time, he got this result. It was, I think, the first experimental verification of de Broglie".[92]

It seems that it was Franck, therefore, who read in the strange graphics that Davisson had sent to Born not the consequence of the distribution of forces within the crystal lattice of the metal used as target, but a clue to the wave properties of matter. And it follows that it had been Franck who connected de Broglie's revolutionary hypothesis to the incomprehensible pattern of the distribution of the scattered electrons discovered in a distant General Electric lab. All of this took place in 1925, i.e., some months before the publication of Schrödinger's first article on wave mechanics. Born and Franck agree in recognizing the important role played by Elsasser in the whole story, since he quickly found confirmation of the suggestions of both his 'teachers' and handed in this result in a very short note that was submitted for publication to *Die Naturwissenschaften*[93] Elsasser's own account, although different in claiming for completely understandable reasons a different and much less other-directed route[94], does not alter the course of events in any substantial way. It is true that Elsasser was not able to translate his intuition into a workable experiment in Franck's laboratory, because of his young age, and because, as Franck noted, "he was not gifted to do experiments"[95], he was more theoretically minded. One could enrich the Born account with the contribution made by Sommerfeld during his visit to Göttingen[96]; or with the role played by Einstein in the decision of the magazine to publish the brief note of

92 Ibid.

93 Elsasser (1925). "Even for the interference seems there are data obtained in an experiment of Davisson and Kunsman [Phys. Rev., **22**, 243, 1923], in which it is investigated the angular distribution of electrons which were reflected on a plate of platinum. Were found several maxima of intensity that, increasing the speed of electrons, went in the direction that it is expected according to the equation. [$\lambda = h/mv$], when we consider the maximum values as a diffraction pattern in very similar way to an optical lattice. If you put as a constant of the crystal lattice the value of platinum and if you consider, because of the relatively low penetration depth of the electrons, a grid in first approximation plane, on the basis of this rough calculation had been obtained for the wave-length values which coincide with those calculated on the basis of the eq. [$\lambda = h/mv$] to second order (almost 100%)".

94 See the Interview of Walter Elsasser by John L. Heilbron, on 29 May, 1962, AIP

95 Interview to James Franck by Thomas S. Kuhn, on 12 July, 1962, AIP.

96 Franck notes in his reconstruction that Elsasser told him one day that he wanted to carry out experimental work that had been inspired by reading an article by Einstein in which he spoke of the wave theory of de Broglie. "And he said, "Yes, it would be nice if one could show that an electron really has wave character." And I had just read the day before that paper of Davisson and what do you say his name is? Kunsman. And when he said that to me, I said, "Yes, it would be. But how about if I tell you that it has been done. Only the authors don't know it. Namely, in that paper of Davisson and Kunsman they have treated their metal surface in such a way that it became crystalline. And therefore I am sure that what they have done for us is to have studied just the wave character of electrons without knowing what it is." Then Sommerfeld paid us a visit. [...] Also Sommerfeld said, "Look at that [Davisson and Kunsman] paper and see whether you can make some sense of it." [...] Anyway, that paper interested also Sommerfeld very much. [...] And then when Elsasser came, it clicked in my mind that it would be that. And I said, "Now Elsasser, the best thing to do is make some experiments, and you make these experiments. [...] Well, we started those experiments, but it turned out after four weeks that it was hopeless for him to make such experiments. He was not gifted to do experiments. So I told him, "Now let's see. You came with this idea of electrons and waves, and I showed you this paper of these men. Now you write a little note to the *Naturwissenschaften* and explain the situation and what they did"". Ibid.

Elsasser's[97]; or even with the letter sent from Born to Einstein on 15 July in which, even among legitimate reservations about the considerations "of our Mr Elsasser", he stated authoritatively that "the essential part of his idea can be saved, in particular concerning the reflection of electrons".[98] But nothing could change nor improve the certainty that during those months in 1925 confirmation of the de Broglie wave hypotheses by Davisson experiments had been found at Göttingen.

Any remaining doubts could be entirely dispelled by an exceptional witness who had been involved at the time in drafting the famous paper on the quantum-theoretical reinterpretation of kinematics and mechanics from which matrix mechanics would derive. After a brief stay in Copenhagen, Heisenberg had returned to Göttingen in the spring of 1925 and in a letter to Pauli, 24 June, informed him of what was happening. "Before you take care of my silly work I want to tell you about a funny thing: you know about Einstein's new work on the atoms that move according to [the] wave theory? If you apply this theory to slow electrons we obtain Ramsauer's curves for noble gases ('light scattering in colloidal particles'), or better if you throw slow electrons against a crystal lattice a spectrum of 1. order, of 2. order and so on is obtained, the experiments were carried out some time ago [but] I cannot find out by whom [...]. If what I write is rubbish, I do not know, it is claimed here [in Göttingen] it was Mr. Elsasser, and I almost believe it".[99] A few days later, on 29 June, Heisenberg had sent Pauli a postcard in which not only was he able to remember the name of authors of experiments cited, but he stated: "I consider it possible in all seriousness for both the Ramsauer and the Davisson and Kunsman experiments to be explained by Einstein's theory (this was Elsasser's only statement), and since I started studying Einstein's work I'm really excited about it".[100]

The annual meeting of the British Association for the Advancement of Science began on 4 August 1926, with official ceremonies and the Presidential address given by the Prince of Wales. The work of the Mathematical and Physical Sciences section began the next day with a most exceptional participation of foreign guests. In fact, along with Ernest Rutherford, William Bragg and Alfred Fowler the halls of Oxford hosted Niels Bohr, Max Born, James Franck, Manne Siegbahn, Wilhelm Wien and Peter Zeeman, all scholars who had been or would be awarded the Nobel Prize. Max Born opened the session of 10 August with a lecture on "The Quantum Mechanics of Electron Collisions".[101] Although the final text of this conference was to appear on the 5 March 1927 issue of *Nature* with a different title, "Physical Aspects of Quantum Mechanics",[102] the heart of the discussion remained the quantum formulation of

[97] In Elsasser's recollection, essentially contained in Elsasser (1978), the role of Born in the whole story is completely cut downsized: "Born's statement to an interviewer [...] that he had 'suggested' my note, is no doubt a lapse of memory in old age. As a young student in Born's classes, I hardly knew him personally, and I had talked with Franck, whom I knew well", ibid., p. 63.

[98] Born to Einstein, 15 July 1925, in Einstein and Born (1969, p. 118).

[99] Heisenberg to Pauli, 24 Jun 1925, WB 225–228: 226–227.

[100] Heisenberg to Pauli, 29 Jun 1925, WB 229–230: 229.

[101] British Association for the Advancement of Science, *Report of the Ninety-Fourth Meeting*, London, 1926, 340.

[102] Born (1927).

electron collisions. It had been Elsasser's merit, recognized Born, to show that "the theory yields general formulae for the distribution of electrons over the different angles of deflexion, that differ in a characteristic way from the results that we should have expected classically".[103] Assuming then de Broglie's idea that the motion of material particles is accompanied by waves allowing, for example, to establish a relationship between wave-length and momentum of the particle itself, it could be concluded "that the collision of an electron with an atom should give rise to a diffraction" in a completely analogous way to that involving a beam of light scattered by small particles. It was therefore correct to state that the maximum and minimum intensity that occur in different directions due to the collision processes represent the irregularities in the distribution of the deflected electrons; a phenomenon that could be interpreted only if they are ascribed a wave nature. At this point, Born introduced a comment that according to the above must have seemed quite natural to him:"Indications of such an effect are given by experiments of Davisson and Kunsman (*Phys. Rev.*, **22**, 243, 1923), on the deflection of electrons from metallic surfaces".[104] So, one of the most successful theoretical physicists of the time, speaking in one of the most prestigious forums to an impressively qualified audience recognized publicly that the merit of the first confirmation of the validity of the wave view of matter was to be credited to Davisson and Kunsman's experimental research. It was August 1926 and the only one to be surprised in hearing Born's statement was probably just Clinton Davisson.

6 The Bohr Uncertainty

Let's try to summarize the elements highlighted so far. Since the summer of 1925, it had been suggested that the data reported in the article that Davisson and Kunsman had published in 1923 provided a particularly meaningful empirical evidence in support of the wave view of de Broglie's particles of matter. This hypothesis later took on the efficacy of a demonstration as the same conclusion had to be evinced in order to interpret the 'strange' behavior of electrons as revealed in the so-called Ramsauer effect. Everything had been published in a letter sent on 18 July 1925 to the *Naturwissenschaften*. The author was indeed a young 21-year-old student, but the letter came from Göttingen, certainly one of the most prestigious centers of physical research of the time. Moreover, the text had been endorsed by James Franck and Max Born, while Einstein himself had been involved in the decision of publishing Walter Elsasser's text. With the publication of that letter the interpretation of Davisson's experiment ceased to be a privately circulating conclusion among a small group of experts. That hypothesis had now been brought to the attention of the scientific community at large and suggested the creation of a further experiment that Einstein himself considered to be most promising.

We can still grant Jørgen Kalckar and his brave editorial choice the qualm that Bohr may not have been aware of the note published in *Naturwissenschaften*, or that he had not valued it with the necessary attention being distracted by other issues,

[103] Ibid., p. 356.

[104] Ibid.

just weeks after the blatant experimental falsification of the theory of virtual oscillators. It would be a very singular circumstance given the very close relationship that existed between Göttingen and Copenhagen, as well as the intense scientific collaboration that Bohr had kept in those years with both Franck and Born, who had undoubtedly played a key role in connecting Davisson's research to the wave properties of matter. But any remaining reservations are dispelled by Bohr's own words, contained in an addendum he wrote in those very days that Elsasser's note was being drafted.

On 30 March 1925, Bohr had sent an article to the *Zeitschrift für Physik*[105] concerning the behavior of atoms during collisions which was obviously affected by the conclusions he had reached in the BKS article on the non-validity of the principles of conservation in individual physical processes. Although, after the publication of Geiger and Bothe on X-ray scattering,"the question of the applicability of the conservation laws appears in a new light"[106] compared to what is claimed in the article, Bohr decided to publish it anyway, limiting himself to add a large text to the proofs, Nachschrift (Juli 1925),[107] whose closing statements leave no room for ambiguity or doubt about the direction of Bohr's research. First, Bohr quoted the Ramsauer discovery which emphasizes the existence of "a different kind of collision effect involving a conspicuous deviation from the properties of mechanical models".[108] It is therefore precisely the study of collision processes to bring up behaviors that seem to decree the end of the traditional model-like approach. The discussion became even more explicit and assumed a quite general value because the anomalies were not ascribable to any particular mechanical model; in fact, shortly after he pointed out that here we are dealing"with a collision phenomenon for which space-time pictures based on the concepts of classical electrodynamics fail in a manner that reminds us of the paradoxes revealed in the analysis of the radiation phenomena".[109] The point-like particles too exhibited properties that fell outside a space–time mode of description based on the concepts of classical physics and, surprisingly, even matter is subject to the same paradoxes concerning the supposed dual nature of radiation. Bohr quoted the de Broglie Theses and Einstein's articles which, by introducing assumptions about the wave nature of particles, seemed to him to open new and interesting perspectives towards the overcoming of obstacles resulting from the renunciation of space–time pictures. These are precisely the same works cited, together with the Ramsauer effect, in Elsasser's note.

Thus Kalcar's main reason for re-dating Bohr's manuscript is no longer valid and in the light of the other deductions I have drawn from the above analysis it can be

[105] Bohr (1925).

[106] Ibid., p. 142.

[107] Ibid., pp. 154–157.

[108] Ibid., p. 156. In the same quoted letter, Bohr confessed to Geiger that "recently I have also felt that an explanation of collision phenomena, especially Ramsauer's results on the penetration of slow electrons through atoms, presents difficulties to our ordinary space-time description of nature similar in kind to those presented by simultaneous understanding of interference phenomena and a coupling of change of state of separated atoms by radiation", CW5 79.

[109] Ibid., p. 157.

concluded beyond any reasonable doubt that sheets I and II of mss26 were written by Bohr on 10 July 1926. No reasonable argument can compel us to invoke a gross error by the author, which would actually also be inexplicable in such a proverbially careful and scrupulous scholar.

"Possibility for space–time description closely connected with conservation theorems. Measurement of energy or momentum with given accuracy implies loss of phase relations, which implies impossibility of interference by superposition". This item ends the framework outlined by Bohr in his manuscript. This is a surprising statement admitting only one interpretation. Bohr spoke of complementary aspects of the experience and, as he emphasized in the July 1925 *addendum*, reality shows a complexity that undermines any attempt of bringing it into the typical space-time images of classical theories. Having acknowledged that the theoretical framework of quantum physics is entirely changed by the formulation of the wave theory of matter, and having thus recognized the general failure of classical models of description, Bohr said that in the quantum world the "possibility for space–time description" stands in a relationship of mutual exclusion "with conservation theorems"; by this he meant that in the microscopic world any traditional mode of causal space–time description is impossible. The "closely connected" should not mislead: the next sentence clarified that the connection in fact entails a mutual exclusion. The more accurate the measurement of the energy or momentum—a necessary requirement to ensure the validity of conservation laws—the greater the impossibility of using the superposition principle to obtain the space-time localization of our quantum object, quantum of light or electron through a wave train; this is why the conditions to apply to them a space-time description do not exist. Any further deduction drawn from these notes could only result in a forced interpretation. But keeping only to the bare wording, it seems that here are collected *in nuce* all the issues that Bohr was to develop during 1927, his *annus mirabilis*. In particular, referring to the precision with which certain dynamical variables can be measured and the consequences the latter would have on the ability to determine the corresponding kinematic quantities, it is not far-fetched to glimpse a more than obvious anticipation of that idea of uncertainty in the definition of classical concepts that conveys the very essence of the idea of complementarity. Bohr based his reasoning on the discontinuity typical of quantum processes, elevating this concept to the rank of postulate, but at the same time he grasped the enormous potentialities stemming from the existence of a behavior of matter to be interpreted only on the basis of wave concepts.

Were these not the issues at the heart of the endless discussions with Heisenberg in the winter of 1927, before an exhausted Bohr decided to leave Copenhagen for some rest? Were these not the arguments that he was to use to criticize quite heavily Heisenberg's uncertainty paper, as soon as he was able to examine it carefully? Were these not the arguments with which he had tried to prevent its publication? Were these not the arguments used in his letter sent to Einstein in April 1927 in which he proposed an alternative derivation of the uncertainty relations to the one proposed by Heisenberg? Are there are not, finally, sufficiently well-founded reasons to talk about a Bohr uncertainty, and of course of complementarity before uncertainty?

References

Beller, M. 1999. *Quantum dialogue. The making of a revolution.* Chicago: University of Chicago Press.

Bohr, N. 1923. The correspondence principle. *Report of the British Association for the Advancement of Science*, Liverpool, 428–429.

Bohr, N. 1925. Über die Wirkung von Atomen bei Stoßen. *Zeitschrift für Physik* 34: 142–157.

Bohr, N. 1928a. *The quantum postulate and the recent development of atomic theory.* Atti del Congresso internazionale dei Fisici, 11–20 Settembre, Vol. II, 565–588. Bologna: Zanichelli.

Bohr, N. 1928b. The quantum postulate and the recent development of atomic theory (with an introductory note by the Editors: New problems in quantum theory). *Nature* 121(Suppl.): 579–590.

Bohr, N. 1984. *Collected works. Vol. 5. The emergence of quantum mechanics (mainly 1924–1926)*, ed. K. Stolzenburg. Amsterdam: NorthHolland.

Bohr, N. 1985. *Collected works. Vol. 6. Foundations of quantum physics I (1926–1932)*, ed. J. Kalckar. Amsterdam: NorthHolland.

Bohr, N., Kramers, H. A., and Slater, J.C. 1924. The quantum theory of radiation. *Philosophical Magazine* 47: 785–802.

Born, M. 1927. Physical aspects of quantum mechanics. *Nature* 119: 354–357.

Bothe, W., and Geiger, H. 1925. Experimentelles zur Theorie von Bohr, Kramers und Slater. *Die Naturwissenschaften* 13: 440–441.

Campbell, N.R. 1927. Philosophical foundations of quantum theory. *Nature* 119: 779.

Cassidy, D. 1992. *Uncertainty. The life and science of Werner Heisenberg.* New York: Freeman.

Compton, A.H. 1923. A quantum theory of the scattering of X-rays by light elements. *Physical Review* 21: 483–502.

Compton, A.H., and Simon, A.W. 1925. Directed quanta of scattered X-rays. *Physical Review* 26: 289–299.

Davisson, C.J. 1923. The scattering of electrons by a positive nucleus of limited field. *Physical Review* 21: 637–649.

Davisson, C.J. 1965. *The discovery of electron waves.* Nobel Lectures, Physics 1922–1941, 387–394. Amsterdam: Elsevier Publishing Company.

Davisson, C.J., and Kunsman C.H. 1923. The scattering of low speed electrons by platinum and magnesium. *Physical Review* 22: 242–258.

Davisson, C.J., and Germer, L.H. 1927a. The scattering of electrons by single crystal of nickel. *Nature* 119: 558–560.

Davisson, C.J., and Germer, L.H. 1927b. Diffraction of electrons by a crystal of nickel. *Physical Review* 30: 705–740.

Dirac, P.A.M. 1927. Physical interpretation of quantum dynamics. *Proceedings of the Royal Society of London* 113: 621–641.

Einstein, A. 1917. Zur Quantentheorie der Strahlung. *Physikalische Zeitschrift* 18: 121–128.

Einstein, A. 1924. *Quantentheorie des ein atomigen idealen Gases.* Berliner Berichte 20 September: 261–267.

Einstein, A. 1925. *Quantentheorie des ein atomigen idealen Gases.* Berliner Berichte 9 February: 3–14.

Einstein, A., and Born, M. 1969. *Brief Wechsel 1916–1955.* Munich: Nymphenburger.

Elsasser, W. 1925. Bemerkungen zur Quantenmechanik freier Elektronen. *Die Naturwissenshaften* 13: 711.

Elsasser, W. 1978. *Memoirs of a physicist in the atomic age.* New York: Science History Publications.

Gehrenbeck, R.K. 1978. Electron diffraction: fifty year ago. *Physics Today* 31: 34–41.

Heisenberg, W. 1925. Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen. *Zeitschrift für Physik* 33: 879–893.

Heisenberg, W. 1927. Über den anschaulichen Inhalt der quantentheoretische Kinematik und Mechanik. *Zeitschrift für Physik* 43: 172–198.

Jordan, P. 1927. Über eine neue Begründung der Quantenmechanik. *Zeitschrift für Physik* 40: 809–838.

Kelly, M.J. 1962. *Clinton Joseph Davisson 1881–1958.* Biographical Memoirs of the National Academy of Sciences. New York: Columbia University Press, 51–84.

Lakatos, I. 1970. Falsification and the methodology of scientific research programmes. In *Criticism and the growth of knowledge*, ed. I. Lakatos and A. Musgrave, 172–198. Cambridge: Cambridge University Press.

Pauli, W. 1970. *Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg, u. a.*, vol. I: 1919–1929, ed. A. Hermann, K. von Meyenn and W. F. Weisskopf. New York: Springer.

Petruccioli, S. 2006. *Atoms metaphors and paradoxes. Niels Bohr and the construction of new physics*. Cambridge: Cambridge University Press.
Ramsauer, C. 1921. Über den Wirkungsquerschnitt der Gasmoleküle gegenüber langsamen Elektronen. *Annalen der Physik* 4: 513–540.
Rubin, H. 1995. *Walter M. Elsasser 1904–1991*. Biographical Memoirs of the National Academy of Sciences, 103–161. New York: Science History Publications.
Russo, A. 1981. Fundamental research at Bell Laboratories: The discovery of electron diffraction. *Historical studies in the physical sciences* 12: 117–160.
Thomson, G. P. 1965. *Electronic waves*. Nobel Lectures, Physics 1922–1941, 397–403. Amsterdam: Elsevier Publishing Company.