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THE DISCOVERY OF QUANTUM MECHANICS AND ITS INTERPRETATION (1925 − 1927)

Introduction

In the Bohr Archives, Copenhagen, one finds a manuscript of 7 pages, entitled ‘Fundamental problems of the quantum theory’ and dated 13 September 1927. This constitutes the partly very sketchy draft of a lecture, which Niels Bohr presented nearly seventy years ago at the Volta Congress in Como and contains the first written version of what became the fundamental paper on the *complementarity principle*, the central part of the so-called ‘Copenhagen interpretation’ of quantum mechanics.

In fall of 1927 a period of about three years came to a conclusion, during which modern atomic theory emerged. In the present talk I want to offer a few glances at some major events. It is based on the scientific correspondence exchanged between the active pioneers, who informed their most interested colleagues and received in return valuable suggestions from them.[1] Thus a rather lively picture evolves of the double discovery of quantum and wave mechanics, the successes of the new theories, and finally of the unified physical interpretation achieved in Copenhagen.[2]

Letters indeeed constitute the backbone of a detailed historical reconstruction of the events, which happened some seventy years ago and were initiated by a letter of Wolfgang Pauli, then in Hamburg, to Ralph Kronig, an American visitor in Copenhagen. Pauli wrote on 21 May 1925:

> Physics at the moment again is very muddled; in any case, for me it is too complicated, and I wish I were a film comedian or something of that sort and had never heard anything about physics. Now I do hope nevertheless that Bohr will save us with a new idea. I beg him to do so urgently, and convey to him my greetings and many thanks for all his kindness and patience towards me.

Besides Berlin (where quantum theory had been invented by Max Planck in 1900 and many pioneers, such as Albert Einstein, Walther Nernst, Emil Warburg, James Franck, Gustav Hertz, Karl Schwarzschild, Max Born, Alfred Landé and Rudolf Ladenburg had worked during the first 25 years of the theory) and Munich (where, under the direction of Arnold Sommerfeld, Bohr’s atomic model was developed by the professor and his students from Peter Debye and Sophus Epstein to Werner Heisenberg, Wolfgang Pauli and Gregor Wentzel), Copenhagen constituted the very heart of atomic theory, with Niels Bohr providing ideas guiding the development. In spite of Pauli’s urgent request, however, the expected salvation did not come from Copenhagen but from Göttingen, a relatively new center of quantum theory directed since 1921 by Max Born. There Heisenberg conceived the first formulation of the new quantum mechanics, and his revolutionary ideas reached first those places, where his closest friends lived and were struggling for the same goal.

1. The discovery of quantum mechanics (Göttingen 1925)

In the beginning of May 1925, Heisenberg wrote three times to Ralph Kronig, with whom he had cooperated a little earlier in Copenhagen on the spectral theory of many-electron atoms.

In the second letter, dated 5 May, he wrote down in some detail equations expressing the transition to a new atomic theory: notably he introduced a ‘re-interpreted’ Fourier series with quantum-theoretical transition amplitudes and frequencies depending on two quantum numbers. For example, he replaced the classical periodic equation

$$b_2 \exp(2i\omega t) = \{a_1 \exp i\omega t\}^2$$

by the quantum-theoretical equation

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$$b_2(n,n-2)\ \exp\{i\omega(n,n-2)\} = a_1(n,n-1)a_1(n-1,n-2)$$
$$\exp\{i[\omega(n,n-1)+\omega(n-1,n-2)]\}$$

During his stay in Heligoland, where he recovered from a hay-fever, Heisenberg then found a cornerstone of his re-interpretation scheme, namely, that the 'classical' Bohr-Sommerfeld quantum-integral $J = \int p\, dq$ changed into the (empirically substantiated) Thomas-Kuhn sum-rule. Explicitly, he rewrote the differential expression

$$1 = \frac{d}{dJ}(J) = \frac{2\pi m}{h}\sum_{\tau=-\infty}^{\tau=+\infty}\frac{d}{dn}(\omega\tau|a_\tau|^2)$$

as the quantum-theoretical sum

$$1 = \frac{4\pi m}{h}\sum_{\tau=0}^{\infty}\left[|a(n,n+\tau)|^2\omega(n,n+\tau) - |a(n,n-\tau)|^2\omega(n,n-\tau)\right].$$

This is the earliest example of the quantum-mechanical *commutation relations*, which characterize more than anything else the new *quantum mechanics*.

About the next steps and ideas, especially the application of the philosophical principle (formulated a little earlier by Born) to use in the new quantum-mechanical formulae only observable quantities, Heisenberg informed first Wolfgang Pauli on 24 June 1925:

> I have almost no desire to write about my own work, because to me everything is still unclear and I just vaguely anticipate how things will turn out, but perhaps the basic ideas are still correct. The fundamental axiom is. In calculating any quantities, like energy, frequency, etc., only those quantities should occur, which can be controlled in principle.

A couple of weeks later he sent, again to Pauli, the completed manuscript of that paper which marked the discovery of quantum mechanics. Heisenberg gave it the title 'On quantum-theoretical re-interpreation of kinematical and mechanical relations'.[3] And Pauli characterized the spirit of the whole approach in a letter to Hendrik Kramers in Copenhagen, dated 27 July 1925:

> In particular, I have greatly rejoiced in Heisenberg's bold attempts. To be sure, one is still very far from saying something definite, and we stand at the very beginning of things.
>
> However, what has pleased me so very much in Heisenberg's consideration is the *method* of his procedure and the aspiration, with which he has embarked upon his considerations.
>
> Altogether I believe, with respect to my scientific ideas, I have now come very close to Heisenberg, and that we now agree almost about everything, as much as this is possible at all for two independently thinking persons.

Well, the 'something more definite' come very soon. On the basis of Heisenberg's ideas, Max Born in Göttingen developed, in Heisenberg's absence with Pascual Jordan, the mathematically systematic theory of *matrix mechanics*.[4] From Copenhagen, where Heisenberg went in September 1925, he then joined by correspondence Born and Jordan in fully elaborating that theory.[5] And again Pauli would be the first person to receive detailed information about the progress in quantum mechanics, before the final long manuscript of Born, Heisenberg and Jordan (representing the 'bible of matrix mechanics') was submitted for publication in November 1925. This fact enabled Pauli to make a most valuable contribution, his matrix treatment of the hydrogen of the hydrogen atom.[6] Now Heisenberg had reasons to praise his friend, and he did so in a letter of 3 November 1925: 'I need not write how much I rejoice about your new theory of the hydrogen atom, and how much I admire that you have obtained this theory so fast.'

2. The discovery of wave mechanics (Zürich 1925-1926)

Independently of these events, Erwin Schrödinger -- since Fall 1921 professor of theoretical physics at the University of Zürich -- invented a different scheme of atomic theory based on the matter-wave idea of Louis de Broglie. Upon getting to know in November 1925 the latter's comprehensive PhD thesis of 1924, Schrödinger wrote down a relativistic wave equation for the hydrogen atom; unhappily he noticed that the standard mathematical solution did not yield Sommerfeld's empirically well-established finestructure formula. So he first published a more 'successful' application of matter waves to a system of gas molecules and thus confirmed the results of Albert Einstein's recent theory of ideal gases.[7]

In January 1926 an urgent letter reached him from Willy Wien in Munich. The editor of the renown German physics journal *Annalen der Physik* wrote on 24 December 1925: ' I have not received anything for the Annalen from Zürich for a long time. I would very much enjoy obtaining several Swiss papers soon.' Schrödinger answered promptly: 'Mainly, however, I now want to get to the vibration theory of the atom at once, and I hope to be able already very soon to send you a communication on that problem.' (Letter to Wien, 8 January 1926)

Already before learning about request, he had informed Wien from Arosa (on 27 December 1925, when he spent Christmas vacations there) about his recent investigations :

> At the moment I am plagued by a new atomic theory. I believe that I can write down a vibrating system constructed in a comparatively natural manner and not by *ad hoc* assumptions -- which has as its eigenfrequencies the term frequencies of the hydrogen atom.

Wien received the completed paper on 27 January 1926 .[8] Less than a month later, Schrödinger continued the series (in English translation) 'Quantization as a problem of eigenvalues' with a second communication, in May followed a third and in June a fourth.[9]

Schrödinger had requested Wien to show the manuscript of the first communication to his Munich colleague Sommerfeld before publication, and soon a lively correspondence developed between the two theoreticians. For example, Sommerfeld asked about a possible connection of the new undulatory or 'wave-mechanical' approach with earlier ideas expressed by Walther Ritz some twenty years ago. Schrödinger replied on 20 February:

> Unfortunately, Ritz always attempted to represent the *line* frequencies; he believed that he had to derive eigenvalues of the form $1/n^2 - 1/m^2$, while one is dealing with the *term* values $1/n^2$. This fact, of course. must complicate the situation enormously.

Schrödinger also made quick progess with extending and establishing his scheme. For example, in March 1926 he submitted an investigation showing what Sommerfeld had guessed: 'It is my impression that your method is a substitute for the new quantum mechanics of Heisenberg, Born and Dirac, in particular a simplified method, so-to-speak, an analytic resolvent of the algebraic problem stated there, *because your results fully agree with theirs*!' (Letter to Schrödinger, 2 February 1926). That is, the results derived from wave or the Born-Heisenberg-Jordan matrix mechanics for all atomic problems that could be dealt with both methods coincided.[10] The next paperof the eigenvalue series, recieved by Wien on 10 May 1926, was devoted especially to calculate the Stark effect, again in agreement with the known spectroscopic results.

Informed by letters from Schrödinger himself or others, the most active quantum physicists of those days, like Pauli (Hamburg) or Paul Dirac (Cambridge), joined in applying the wave-mechanical scheme to new atomic problems. Pauli began by demonstrating (independently of Schrödinger) the equivalence of the wave- and matrix-mechanical calculations, of course in a letter (to Jordan, dated 12 April 1926). From Göttingen and Copenhagen Heisenberg stimulated Dirac to study the new scheme. After some hesitation Dirac did so and found a major result in August 1926, namely the relation between the quantum-statistical behaviour of particles and the symmetry of the wave functions describing them.[11] Heisenberg himself, in an investigation received on 24 July 1926 by *Zeitschrift für Physik*, obtained the correct energy states of the helium atom, thus solving the problem by which he and Born had demonstrated three years earlier the breakdown of the old Bohr-Sommerfeld theory of atomic structure.[12]

Finally, Born after his return from a sabbatical in USA to Göttingen employed wave-mechanical methods to derive, in June and July 1926, the first quantum-mechanical theory of scattering of atomic particles.[13] In this theory he interpreted the square of the Schrödinger wave amplitude as denoting the probability of the corresponding particle to assume a given position. Born's probability interpretation became the first cornerstone of what later was called the 'Copenhagen interpretation' of quantum mechanics.

3. The interpretation of quantum mechanics (Zurich, Copenhagen 1926-1927)

In the fourth communication of his series on the eigenvalue problem, Schrödinger intensified his efforts to obtain an interpretation of atomic phenomena on the basis of a visualizable wave picture. His partner Wien enthusiastically agreed and expressed great satisfaction about the fact that the 'quagmire of integral and half-integral quanta and discontinuities and of arbitary use of the classical theory' had been terminated and that now even 'young physicists soon would become used again to rigorous physical thinking' (Wien to Schrödinger, 23 June 1926). He cordially invited Schrödinger to present his successful theory in the Munich collquium, and Schrödinger came and did so on 23 July 1926.

Accidentally, Heisenberg participated in this Munich colloquium. His thoughts went quite differently from those of Schrödinger and Wien, and he raised in the discussion several serious objections. However, Wien told him harshly to learn more decent physics and leave atomic theory to Schrödinger. Though the crash with Wien did not change Heisenberg's convictions, the event made him very unhappy and he reported it to Bohr. The latter invited Schrödinger to Copenhagen to discuss the interpretation of the new atomic theory.

About the contents tough discussions of early October 1926 some later reminscences of Bohr and especially Heisenberg do exist.[14] However, the correspondence again throws light on the different points of view, as seen by Schrödinger. For example, in the letter to Born, dated 2 November 1926, he wrote:

> I have, however, the impression that you and others, who especially share your opinion, are too deeply under the spell of those concepts (like stationary states, quantum jumps, etc.), which have obtained civic rights in our thinking in the last dozen of years; hence you cannot do full justice to an attempt to break away from this scheme of thought.

And to Bohr, in a letter of 23 October 1926, he repeated his vision about the ideal interpretation:

> What is before my eyes, is only one thesis: One should not, even if a hundsred trials fail, give up the hope of arriving a the goal -- I do not say by means of classical pictures, but by logically consistent conceptions -- of the real structure of space-time processes. It is extremely probably that this is possible.

The second act of the interpretation story was played nearly entirely in Copenhagen and began the moment that Schrödinger had left. It involved Bohr, Heisenberg, Oskar Klein and, through correspondence, also Pauli. The first documented input came from Pauli, who discussed in a letter of 19 October to Heisenberg the motion of electrons in atoms; after some mathematical reasoning he concluded with the remarks:

> The physics is still unclear to me to a large extent. The first question is, why only the p's and certainly not the p's and the q's simulatneously can be described with arbitrary accuracy. ... One can look at the world withe the p- eye and also with the q-eye, but if one wants to open both eyes, then one is led astray.

In the same letter Pauli proposed a generalization of Born's probability interpretation, which stimulated (independently) both Dirac in Copenhagen and Jordan in Göttingen to develop their respective mathematical transformation theories of quantum mechanics (December 1926).[15]

The results of Dirac and Jordan then assisted Heisenberg in deriving his 'uncertainty relations', which he first described in detail to Pauli in a lengthy letter of 23 February 1927. After analyzing the concepts of position, orbit and momentum (velocity) of an electron -- e.g., : 'The question for position of an electron must be replaced by: How *does one determine* the position of an electron?' -- he obtained always the same result concerning the restriction of quantum-theoretical variables, hence Heisenberg arrived at the strong result:

> If there ever would be any experiment allowing to determine p and q *accurately,* then quantum mechanics must be necessarily wrong.

In the famous published paper, 'On the perceptible content of the quantum-mechanical mechanics and kinematics' [16] , received by *Zeitschrift für Physik* on 23 March 1927, the famous 'uncertainty relations' are presented and discussed in some detail.[16] In a letter to Dirac, dated 27 April 1927, Heisenberg explained in particular the application of the *pq*-uncertainty in the case of an electron observation with the help of a γ-ray microscope. He further added the remark:

> Professor Bohr says that one in all those examples sees the very important role, which the *wave*-theory plays in my theory, and, of course, he is quite right.

Since October 1926, parallel to the mentioned exchanges of Heisenberg with Pauli (and to some extent with Dirac), Bohr debated the central aspects of the physical interpretation of quantum mechanics at length with his closest associates Heisenberg and Oskar Klein. In contrast to Heisenberg, he assigned also to the wave-picture (besides the particle-picture) an essential role in the game. So he stated on 26 October 1926 to Ralph Fowler in Cambridge:

> Just in the wave mechanics we possess now the means of picturing a single statioinary state which suits for all purposes consistent with the postulates of quantum theory. In fact, this is the very reason for the advantage which the wave mechanics in certain respects exhibits when compared with the matrix method which in other respects is so wonderfully suited to bring out the true correspondence between the quantum theory and the classical ideas.

In those days, Heisenberg did not flollow Bohr's lead but proceeded without referring to the wave-picture to obtain his uncertainty relations. Bohr accepted them immediately, though not Heisenberg's physical arguments. He rather succeeded, in spring and summer 1927, to cast his own interpretation ideas into the new 'principle of complementarity'. This principle he first expounded, as we have mentioned in the introduction, at the Como-Congress on 16 September 1927 and again, a little more complete, later at the Fifth Solvay Conference in Brussels.[17]

Conclusion

At the Solvay Conference in October 1927, which assembled (apart from the Munich expert Arnold Sommerfeld), all then leading senior theoreticians in atomic theory and many of the young pioneers, Bohr's exposition of the complementarity views -- with the basic ingredients of (Born's) probability interpretation and (Heisenberg's) uncertainty relations -- met the staunt opposition of Albert Einstein. A letter of Paul Ehrenfest to his Leyden assistants describes the events and atmosphere of the Einstein-Bohr debate on the interpretation of quantum mechanics quite lively:

> Brussels-Solvay was fine! ...Bohr towering completely over everybody. At first not understood at all, then step by step defeating everybody. ... It was delightful for me to be present in during the conversations between Bohr and Einstein. Like a game of chess. Einstein all the time with new examples. I a certain sense a sort of *perpetuum mobile of the second kind* to break the UNCERTAINTY RELATION.

Bohr from out of philosophical smoke clouds constantly searching for the tools to crush one example after th other. Eintein, like a jack-in-the-box: jumping out fresh every morning. Oh, that was priceless. But I am almost without reservation pro Bohr and contra Einstein. His attitude to Bohr is now exactly like the attitude of the defenders of absolute simultaneity towards him.

Einstein, on the other hand, was not convinced at all that he had really lost the debate. In a letter to Sommefeld, dated 9 November 1927, he rather said:

> Concerning the 'quantum mechanics' I think that as regards ponderable matter it contains just as much thruth as the theory of light without quanta. It might be a correct theory of statistical laws, but it still is an insufficient conception of the individual elementary processses.

The correspondence exchanged from 1925 to 1927 between the eminent quantum physicists deals with most of the important questions discussed at this time. We are quite fortunate that the scientists involved used most intensely the way of communication by letters, and that, since the problems were so new and had to be solved by such unusual ideas, they openly expressed them hoping to get back support or helpful criticism. For the use of correspondence as a tool for exchanging scientific ideas, the three years, in which modern atomic theory was completed, have to be considered as a 'golden age'. We, therefore, must be very grateful to Bohr, Einstein, Heisenberg and Pauli -- just to name a few of the most prolific letter writers -- for the trouble and the time they took to formulate their ideas, doubts and hopes.

On the other hand, even the most intelligent evaluation of the letters would not suffice to reconstruct the historical events fully because of several reasons. First, the available correspondence exhibits serious gaps; for example, all letters addressed to Heisenberg before 1930 have been lost. Second, many guiding considerations have not made it into the letters, since they appeared obvious to the corresponding partners (though they are not to posterity). Third, the contents of the letters provide insights of momentaneous situations, not a continuous story. Still we must admit that the scientific correspondence, of which we have presented here just a few excerpts, constitutes a most valuable, authentic source of information on the enormous wealth of creative ideas that have shaped quantum mechanics. Hence all efforts should be supported to preserve and collect this source -- which belongs to the great heritage of mankind -- and to make it properly available to the historians of science and other interested people.

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