The Lesson of Quantum Theory, edited by J. de Boer, E. Dal and O. Ulfbeck

Epistemological Considerations

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1. Introduction

It is a great privilege to have the opportunity to speak on this occasion at Videnskabernes Selskab, but the task before me is by no means an easy one. Shall I be able to do more than add a few platitudes to what Bohr has said himself and to what others have said about Bohr? Let me first of all explain my choice of subject.

I often had the impression that Bohr himself attached more importance to his ideas about the fundamental principles of the description of nature, to his contribution to the "philosophia naturalis" and to philosophy in general, than to his numerous more concrete triumphs. The confirmation of his notion of stationary states by the experiments of Franck and Hertz, the exact agreement with observations of his formula for the line-spectrum of ionized helium, the discovery of hafnium, an element with properties in agreement with the predictions of his theory of the Periodic System, must have given him immense satisfaction, but from the very beginning he was concerned with the question how his bold approach could be made to supplement, rather than to contradict the notions of classical theory.

I came for the first time to Copenhagen in the spring of 1929 and was introduced to Bohr by my teacher Ehrenfest with the words "... er kann schon etwas, aber braucht noch Prügel" (he has already some ability but still needs thrashing). How true this was—the second part that is. As a matter of fact, I had to a certain extent mastered the formalism of quantum mechanics, which by then had reached a fairly definitive form, but that was about all I knew. I soon discovered that Bohr's interest in those days centered on a further clarification and elaboration of his thoughts on complementarity. He was not particularly interested in the further development and refinement of mathematical techniques, although he encouraged me to do some work in that direction. Neither did solid state physics and other applications of quantum mechanics play an important role in his own work or in that of his institute. A few years after I had left—I stayed until the spring of 1931 with interruptions to pass my examinations at Leiden—nuclear physics began to dominate both the work at the institute and the yearly conferences, and Bohr himself made important contributions to the theory of nuclear reactions and the theory of fission, almost a second youth one might say, but even then Bohr returned time and time again to his epistemological considerations. Therefore, I want to make these considerations the main topic of my talk. But I shall not try to give a comprehensive account of Bohr's thinking: it would be both presumptuous and useless if I tried to reformulate and summarize the ideas Bohr himself was at such great pains to express as well as possible. My purpose is more modest: I shall say something about the discussions between Bohr and Einstein, but first of all I want to make some remarks about the methods Bohr used when dealing with these fundamental questions.

It is a striking fact that in pondering the most profound aspects of quantum mechanics Bohr did always consider simple cases and used only the simplest mathematics. Deceptively simple I should like to say, for there are many pitfalls to be avoided, and it took Bohr's grasp of classical physics to reduce the essence of complicated mathematics to easily grasped concepts. Such simplification is not only a matter of convenience: it meets a more profound requirement. Bohr always emphasized that the final result of a measurement must be something we can tell other people about and that we can describe in simple everyday language: the position of a pointer, a black spot on a film and so on.

2. *Magnetic moment of a free electron*

Although the most sophisticated application of Bohr's methods is to be found in his work with Rosenfeld on measurements of electromagnetic fields [1], in my opinion —and in the opinion of many others—the clearest summary of his thoughts can be found in his "Discussion with Einstein on epistemological problems in atomic physics", his contribution to the volume "Albert Einstein: Philosopher-Scientist" [2], and I shall presently look more closely at the examples discussed therein. However, my first contact with Bohr's way of thinking related to a slightly different theme. In his opening talk at the 1929 meeting Bohr had shown that it is impossible to determine the magnetic moment of a free electron by a "classical" Stern–Gerlach experiment, and one of my first assignments after I came to Copenhagen was to assist Bohr in writing a note on the subject. Bohr liked to have someone to talk to, who could say yes or no at the right moment and who would write down the sentences he pronounced while pacing the room. Stenography was not required, but one had to get used to his rather soft and somewhat slurred voice. Several drafts were produced—they are still in the Bohr archives, and here and there my handwriting appears—but the work was never considered fit for publication.

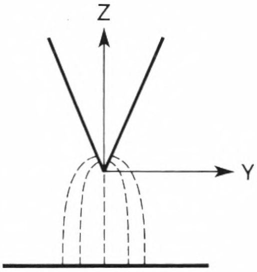

Fig. 1. The wedge-shaped polepiece for the Stern–Gerlach experiment has its edge along the x-axis.

However, Pauli used part of it in his report to the 1930 Solvay Conference (published in 1932) [3]. Figure 1 shows a usual arrangement for a Stern–Gerlach experiment. A wedge-shaped polepiece with its edge along the x-axis is facing a flat one. In the symmetry plane the field is strictly along the z-axis, but as soon as we get out of that plane there is a component H_y. Now we have

$$\operatorname{div} H = 0,$$

which means in this case

$$\frac{\partial H_z}{\partial z} + \frac{\partial H_y}{\partial y} = 0.$$

The force on the magnetic moment of the electron is given by

$$F_s = \frac{eh}{4\pi mc} \cdot \frac{\partial H_z}{\partial z}$$

and this has to be large compared with the uncertainty of the Lorentz force, which is given by

$$\delta F_L = \frac{ev}{c} \delta H_y.$$

Since

$$|\delta H_y| = \left| \delta y \cdot \frac{\partial H_z}{\partial z} \right|$$

it follows from

$$F_s \gg \delta F_L$$

that

$$\delta y \ll \frac{h}{4\pi mv} = \frac{\lambda}{4\pi}.$$

But, if a wavepacket has to be much smaller than the wavelength it is entirely impossible to speak about a trajectory in the classical sense.

The whole trick is to have a clear picture of what a Stern–Gerlach experiment really is and to introduce an equation for the magnetic field at the right moment. Any student can follow the reasoning, but no one thought about it before Bohr.

3. Thought experiments

Let us now look at four thought experiments discussed in the paper I mentioned before.

3.1. First case

A screen with one hole or slit is illuminated by a plane electron-wave. If a is the radius of the hole (or half the width of the slit) and λ the wavelength of the electron, the wave emerging from the hole will show a spread given by $\delta\varphi \sim \lambda/a$ (fig. 2). Since $\lambda = h/mv$ we have $\delta p \simeq p\delta\varphi \sim h/a$. In principle one can measure the recoil of the slit. Then we know how much transverse momentum is imparted to the electron, but in order to carry out such a measurement the screen has to be mobile and the position of the hole becomes uncertain. Here I want to call attention to the following. Bohr uses here and elsewhere a simple formula for $\delta\varphi$. He does not exactly define this $\delta\varphi$. It might be a halfwidth or the width of the first diffraction peak. In textbooks one usually defines Δq as the root-mean-square deviation

$$(\Delta q)^2 = \int (q - \bar{q})^2 \, |\Psi|^2 \, \mathrm{d}q$$

and similarly for Δp. Bohr does not use this definition and that is just as well for in the case of a slit with sharp edges this Δp becomes infinite. Recently, attention has been drawn to the fact that in many cases the root-mean-square deviations are useless and that a more refined analysis is called for. Bohr always steered clear of such difficulties.

3.2. Second case

A screen with two holes illuminated by a point source is considered. Qualitatively, we can easily see what a wave will do. Waves will emerge from the two holes and on

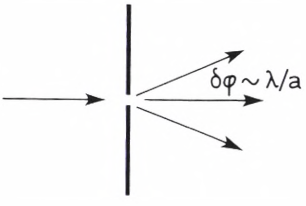

Fig. 2. Diffraction of electron wave by hole or slit.

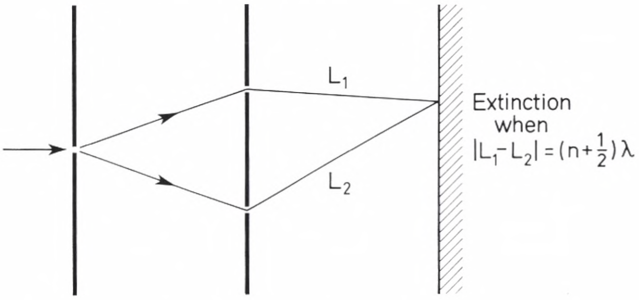

Fig. 3. Screen with two holes illuminated by a point source.

a distant observation screen they will strengthen or annihilate each other, depending on their phase difference. We may use a photographic plate or some kind of particle detector and we shall never find an electron at a node of the wave pattern. But as soon as we find an electron, somewhere between the nodes, we are tempted to ask: "Through which hole has it come? But if it came through one hole, how could the position of the other hole have exerted any influence?" Now it is not too difficult to show that any arrangement that makes it possible to tell through which hole the electron has come does away with an interference pattern. There exists no experimental arrangement that makes it possible to observe interference and thus to demonstrate the wave character of the electron and also to observe through which hole the electron has come (fig. 3). Can experiments of the type described really be carried out? With light, interference patterns obtained with two holes or two slits can easily be observed. With electrons this would be difficult because of the much shorter wavelength. But one can perform and has performed experiments that amount to almost the same: one can study diffraction round a very thin wire in the electron microscope. The unanswerable question is then: On which side has the electron passed the wire?

With the two-hole experiment we are so to say in the very middle of complementarity. I remember an evening at the Carlsberg mansion with Harald Høffding, Bohr's predecessor there. Bohr explained among other things the two-hole experiment. Of course the remark was made: "but the electron must be somewhere on its road from source to observation screen," to which Bohr replied: "what is in this case the meaning of the word *to be*?" And I remember the reaction of the philosopher Jørgen Jørgensen, protesting: "man kan sgu ikke reducere hele filosofien til en skærm med to huller" (one can, damn it, not reduce the whole of philosophy to a screen with two holes).

3.3. Third case

Einstein points out that energy may in principle be determined by weighing. Suppose we have a box full of lightquanta or of electrons, provided with a timer that opens a shutter and placed on a balance or suspended by a spring (fig. 4). The timer can set the time with any desired accuracy, and weighing, both before and after the

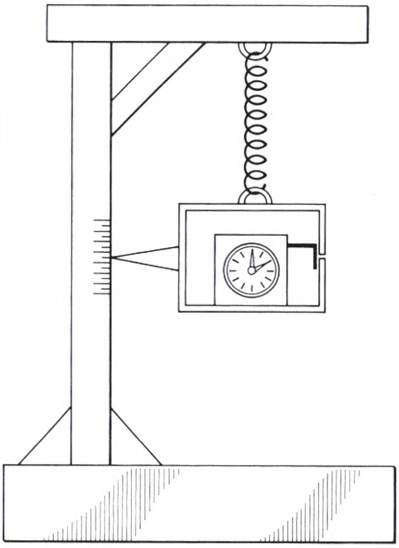

Fig. 4. Weighing apparatus.

electron or lightquantum have escaped, determines the energy, so we circumvent the relation $\delta E \cdot \delta t > h$. Bohr's answer is simple, but again it took Bohr to find it. How can we determine mass with a precision δm? Then $g\delta m$ must impart a measurable momentum to a balance in time t, available for the measurement. Therefore, the initial momentum of the balance must be determined better than $gt\delta m$. Hence, the place of the balance in the vertical direction is uncertain to the amount $\delta z \sim h/gt\delta m$. But general relativity tells us that this leads to an uncertainty of time, because of the red shift of the timer that activates the shutter. We have

$$\frac{\delta t}{t} = \frac{g\delta z}{c^2},$$

and it follows

$$\delta t \sim \frac{h}{\delta m \cdot c^2} = \frac{h}{\delta E}.$$

Again, extremely simple mathematics, but to see at once that weighing must involve uncertainty in place and that uncertainty in place involves uncertainty in time, and to extract from the imposing edifice of general relativity the one simple formula that settles the question, requires a penetrating understanding of the basic principles involved.

3.4. *Fourth case*

On page 229 of his "Discussion with Einstein" Bohr [4] refers to what Ehrenfest reported concerning some further objections of Einstein. I happened to be present at the Leiden colloquium when Einstein spoke about these objections. He again considered the box with shutter of the former example, but now he pointed out that after the electron or lightquantum had left, one still had the choice either to read the time immediately or to carry out a lengthy weighing procedure. The relation $\delta E \cdot \delta t > h$ is not violated, but the curious fact is that the particle whose energy or time of passage we want to determine is left untouched by the choice. Ehrenfest had given me the task of opening the discussion and I tried, to the best of my abilities, to explain the Copenhagen view on these matters. I still remember Einstein's reaction: "Ich weiss, widerspruchsfrei ist die Sache schon, aber sie enthält meines Erachtens doch eine gewisse Härte" (I know, the story is free from contradictions but in my opinion it contains all the same a certain unpalatability).

Einstein's famous paper with Rosen and Podolski [5] goes along similar lines. Two particles may interact temporarily and by measuring either the momentum or the place of the one, we can determine place or momentum of the other, without touching it. Therefore, Einstein concludes, this other particle must *have* a definite place and momentum. For this, there is no room in quantum mechanics, hence the quantum mechanical description is incomplete. In his discussion, Bohr slightly extends his usual mathematics. He points out that $P_1 + P_2$ commutes with $Q_1 - Q_2$ so we can know these two quantities simultaneously. Now we can measure Q_1 and find Q_2 or measure P_1 and find P_2. There is no way to assign a meaning to P_2 and Q_2, unless we specify the measuring equipment that has always to be included in any system considered.

Of course, in all these cases it is possible to go into more mathematical detail, and if one does, one is again struck by the power of Bohr's simple arguments.

4. *Complementarity and completeness of description*

Is there really a difference of opinion between Einstein and Bohr? Is it not a question of words? When Bohr says that quantum mechanics offers a complementary description is that not tantamount to saying that from a classical point of view the description is incomplete? And is that not exactly what Einstein is complaining about? Personally, I think that it is a legitimate use of language to say that the limited applicability of classical concepts to atomic and subatomic phenomena shows that the quantum mechanical description is incomplete, but that does not mean that there is no serious difference between Bohr and Einstein. Bohr argues that the quantum mechanical description is as complete as it can possibly be and he is ready to accept the limitations of our pictures of reality and of our language, ready also to renounce strict causality such as we find in classical mechanics and electrodynamics. He is willing to accept these limitations, because he regards them as essential features of nature and of our human existence. New forces may be revealed, surprising new phenomena may be brought to light, but Bohr considered it impossible that they would ever transgress the limitations imposed by quantum mechanics. Einstein on the other hand, though admitting that quantum mechanics is a powerful discipline that provides a valid description of many phenomena, was convinced that one should search for something beyond, for a theory that would to a certain extent re-establish the notions of classical physics.

A simple analogy may be in order. Thermodynamics is a powerful discipline, providing a satisfactory description of many phenomena, but we know now that behind thermodynamics there are innumerable atoms and molecules at work, and physicists have successfully looked for phenomena where the atomic structure reveals itself. Shouldn't we in a similar way look for something behind the statistical laws of quantum mechanics? Bohr's answer would certainly be an emphatic NO!

What will the future be? So far, the followers of the Copenhagen School can point to greater success. Accepting the limitations of the quantum mechanical description as inviolable laws of nature they have enormously enriched our understanding of nature and our ability to create new phenomena and new devices, whereas Einstein and his followers have made little headway. I am convinced that also in centuries to come atoms and molecules will be studied by means of the same Schrödinger equations we use today, just as we can use Newtonian mechanics to calculate the orbits of planets and satellites, with only minor and in most cases negligible relativistic corrections. But, since Einstein, we talk in a different way about gravitation, although Newton's formula for the attraction remains an excellent approximation. Will one in centuries to come talk about quantum mechanics in the same way as we talk today? Who am I to make a prediction?

The battle of wits between Bohr and Einstein did never lead to personal antagonism, to insinuations or intrigues, things alas not unknown in the history of science. Bohr has on many occasions expressed his admiration for Einstein and his indebtedness to his critical objections. And Einstein, though instinctively averse the Copenhagen School, frankly admitted its consistency and its importance and did not grudge it its successes. We, physicists, should be grateful for this, but that is only part of my gratitude for having known the great physicist and even more for having known the great human being that was Niels Bohr.

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[5] A. Einstein, B. Podolsky and N. Rosen, Phys. Rev. 47 (1935) 777.