

M A X B O R N

The statistical interpretation of quantum mechanics

Nobel Lecture, December 11, 1954

The work, for which I have had the honour to be awarded the Nobel Prize for 1954, contains no discovery of a fresh natural phenomenon, but rather the basis for a new mode of thought in regard to natural phenomena. This way of thinking has permeated both experimental and theoretical physics to such a degree that it hardly seems possible to say anything more about it that has not been already so often said. However, there are some particular aspects which I should like to discuss on what is, for me, such a festive occasion. The first point is this: the work at the Göttingen school, which I directed at that time (1926-1927), contributed to the solution of an intellectual crisis into which our science had fallen as a result of Planck's discovery of the quantum of action in 1900. Today, physics finds itself in a similar crisis - I do not mean here its entanglement in politics and economics as a result of the mastery of a new and frightful force of Nature, but I am considering more the logical and epistemological problems posed by nuclear physics. Perhaps it is well at such a time to recall what took place earlier in a similar situation, especially as these events are not without a definite dramatic flavour.

The second point I wish to make is that when I say that the physicists had accepted the concepts and mode of thought developed by us at the time, I am not quite correct. There are some very noteworthy exceptions, particularly among the very workers who have contributed most to building up the quantum theory. Planck, himself, belonged to the sceptics until he died. Einstein, De Broglie, and Schrödinger have unceasingly stressed the unsatisfactory features of quantum mechanics and called for a return to the concepts of classical, Newtonian physics while proposing ways in which this could be done without contradicting experimental facts. Such weighty views cannot be ignored. Niels Bohr has gone to a great deal of trouble to refute the objections. I, too, have ruminated upon them and believe I can make some contribution to the clarification of the position. The matter concerns the borderland between physics and philosophy, and so my physics lecture

will partake of both history and philosophy, for which I must crave your indulgence.

First of all, I will explain how quantum mechanics and its statistical interpretation arose. At the beginning of the twenties, every physicist, I think, was convinced that Planck's quantum hypothesis was correct. According to this theory *energy* appears in finite quanta of magnitude $h\nu$ in oscillatory processes having a specific frequency ν (e.g. in light waves). Countless experiments could be explained in this way and always gave the same value of Planck's constant h . Again, Einstein's assertion that light quanta have *momentum* $h\nu/c$ (where c is the speed of light) was well supported by experiment (e.g. through the Compton effect). This implied a revival of the corpuscular theory of light for a certain complex of phenomena. The wave theory still held good for other processes. Physicists grew accustomed to this *duality* and learned how to cope with it to a certain extent.

In 1913 Niels Bohr had solved the riddle of *line spectra* by means of the quantum theory and had thereby explained broadly the amazing stability of the atoms, the structure of their electronic shells, and the Periodic System of the elements. For what was to come later, the most important assumption of his teaching was this: an atomic system cannot exist in all mechanically possible states, forming a continuum, but in a series of discrete « stationary » states. In a transition from one to another, the difference in energy $E_m - E_n$ is emitted or absorbed as a light quantum $h\nu_{mn}$ (according to whether E_m is greater or less than E_n). This is an interpretation in terms of energy of the fundamental law of spectroscopy discovered some years before by W. Ritz. The situation can be taken in at a glance by writing the energy levels of the stationary states twice over, horizontally and vertically. This produces a square array

	E_1	E_2	E_3	...
E_1	11	12	13	-
E_2	21	22	23	-
E_3	31	32	33	-
	-	-	-	-

in which positions on a diagonal correspond to states, and non-diagonal positions correspond to transitions.

It was completely clear to Bohr that the law thus formulated is in conflict with mechanics, and that therefore the use of the energy concept in this

connection is problematical. He based this daring fusion of old and new on his *principle of correspondence*. This consists in the obvious requirement that ordinary classical mechanics must hold to a high degree of approximation in the limiting case where the numbers of the stationary states, the so-called quantum numbers, are very large (that is to say, far to the right and to the lower part in the above array) and the energy changes relatively little from place to place, in fact practically continuously.

Theoretical physics maintained itself on this concept for the next ten years. The problem was this: an harmonic oscillation not only has a frequency, but also an intensity. For each transition in the array there must be a corresponding intensity. The question is how to find this through the considerations of correspondence? It meant guessing the unknown from the available information on a known limiting case. Considerable success was attained by Bohr himself, by Kramers, Sommerfeld, Epstein, and many others. But the decisive step was again taken by Einstein who, by a fresh derivation of Planck's radiation formula, made it transparently clear that the classical concept of intensity of radiation must be replaced by the statistical concept of *transition probability*. To each place in our pattern or array there belongs (together with the frequency $\nu_{mn} = (E_n - E_m)/h$) a definite probability for the transition coupled with emission or absorption.

In Göttingen we also took part in efforts to distil the unknown mechanics of the atom from the experimental results. The logical difficulty became ever sharper. Investigations into the scattering and dispersion of light showed that Einstein's conception of transition probability as a measure of the strength of an oscillation did not meet the case, and the idea of an *amplitude* of oscillation associated with each transition was indispensable. In this connection, work by Ladenburg¹, Kramer², Heisenberg³, Jordan and me⁴ should be mentioned. The art of guessing correct formulae, which deviate from the classical formulae, yet contain them as a limiting case according to the correspondence principle, was brought to a high degree of perfection. A paper of mine, which introduced, for the first time I think, the expression *quantum mechanics* in its title, contains a rather involved formula (still valid today) for the reciprocal disturbance of atomic systems.

Heisenberg, who at that time was my assistant, brought this period to a sudden end⁵. He cut the Gordian knot by means of a philosophical principle and replaced guess-work by a mathematical rule. The principle states that concepts and representations that do not correspond to physically observable facts are not to be used in theoretical description. Einstein used the

same principle when, in setting up his theory of relativity, he eliminated the concepts of absolute velocity of a body and of absolute simultaneity of two events at different places. Heisenberg banished the picture of electron orbits with definite radii and periods of rotation because these quantities are not observable, and insisted that the theory be built up by means of the square arrays mentioned above. Instead of describing the motion by giving a coordinate as a function of time, $x(t)$, an array of transition amplitudes x_{mn} should be determined. To me the decisive part of his work is the demand to determine a rule by which from a given

$$\text{array } \begin{bmatrix} x_{11} & x_{12} & \dots & \dots \\ x_{21} & x_{22} & \dots & \dots \\ \text{---} & \text{---} & \text{---} & \text{---} \end{bmatrix} \text{ the array for the square } \begin{bmatrix} (x^2)_{11} & (x^2)_{12} & \dots & \dots \\ (x^2)_{21} & (x^2)_{22} & \dots & \dots \\ \text{---} & \text{---} & \text{---} & \text{---} \end{bmatrix}$$

can be found (or, more general, the *multiplication rule* for such arrays).

By observation of known examples solved by guess-work he found this rule and applied it successfully to simple examples such as the harmonic and anharmonic oscillator.

This was in the summer of 1925. Heisenberg, plagued by hay fever took leave for a course of treatment by the sea and gave me his paper for publication if I thought I could do something with it.

The significance of the idea was at once clear to me and I sent the manuscript to the *Zeitschrift für Physik*. I could not take my mind off Heisenberg's multiplication rule, and after a week of intensive thought and trial I suddenly remembered an algebraic theory which I had learned from my teacher, Professor Rosanes, in Breslau. Such square arrays are well known to mathematicians and, in conjunction with a specific rule for multiplication, are called matrices. I applied this rule to Heisenberg's quantum condition and found that this agreed in the diagonal terms. It was easy to guess what the remaining quantities must be, namely, zero; and at once there stood before me the peculiar formula

$$pq - qp = h/2\pi i$$

This meant that coordinates q and momenta p cannot be represented by figure values but by symbols, the product of which depends upon the order of multiplication - they are said to be « non-commuting ».

I was as excited by this result as a sailor would be who, after a long voyage, sees from afar, the longed-for land, and I felt regret that Heisenberg was not

there. I was convinced from the start that we had stumbled on the right path. Even so, a great part was only guess-work, in particular, the disappearance of the non-diagonal elements in the above-mentioned expression. For help in this problem I obtained the assistance and collaboration of my pupil Pascual Jordan, and in a few days we were able to demonstrate that I had guessed correctly. The joint paper by Jordan and myself⁶ contains the most important principles of quantum mechanics including its extension to electrodynamics. There followed a hectic period of collaboration among the three of us, complicated by Heisenberg's absence. There was a lively exchange of letters; my contribution to these, unfortunately, have been lost in the political disorders. The result was a three-author paper⁷ which brought the formal side of the investigation to a definite conclusion. Before this paper appeared, came the *first dramatic surprise*: Paul Dirac's paper on the same subject⁸. The inspiration afforded by a lecture of Heisenberg's in Cambridge had led him to similar results as we had obtained in Göttingen except that he did not resort to the known matrix theory of the mathematicians, but discovered the tool for himself and worked out the theory of such non-commutating symbols.

The first non-trivial and physically important application of quantum mechanics was made shortly afterwards by W. Pauli⁹ who calculated the stationary energy values of the *hydrogen atom* by means of the matrix method and found complete agreement with Bohr's formulae. From this moment onwards there could no longer be any doubt about the correctness of the theory.

What this formalism really signified was, however, by no means clear. Mathematics, as often happens, was cleverer than interpretative thought. While we were still discussing this point there came the *second dramatic surprise*, the appearance of Schrödinger's famous papers¹⁰. He took up quite a different line of thought which had originated from Louis de Broglie¹¹.

A few years previously, the latter had made the bold assertion, supported by brilliant theoretical considerations, that wave-corpuscule duality, familiar to physicists in the case of light, must also be valid for electrons. To each electron moving free of force belongs a plane wave of a definite wavelength which is determined by Planck's constant and the mass. This exciting dissertation by De Broglie was well known to us in Göttingen. One day in 1925 I received a letter from C. J. Davisson giving some peculiar results on the reflection of electrons from metallic surfaces. I, and my colleague on the experimental side, James Franck, at once suspected that these curves of

Davisson's were crystal-lattice spectra of De Broglie's electron waves, and we made one of our pupils, Elsasser¹², to investigate the matter. His result provided the first preliminary confirmation of the idea of De Broglie's, and this was later proved independently by Davisson and Germer¹³ and G. P. Thomson¹⁴ by systematic experiments.

But this acquaintance with De Broglie's way of thinking did not lead us to an attempt to apply it to the electronic structure in atoms. This was left to Schrödinger. He extended De Broglie's wave equation which referred to force-free motion, to the case where the effect of force is taken into account, and gave an exact formulation of the *subsidiary conditions*, already suggested by De Broglie, to which the wave function ψ must be subjected, namely that it should be single-valued and finite in space and time. And he was successful in deriving the stationary states of the hydrogen atom in the form of those monochromatic solutions of his wave equation which do not extend to infinity.

For a brief period at the beginning of 1926, it looked as though there were, suddenly, two self-contained but quite distinct systems of explanation extant: matrix mechanics and wave mechanics. But Schrödinger himself soon demonstrated their complete equivalence.

Wave mechanics enjoyed a very great deal more popularity than the Göttingen or Cambridge version of quantum mechanics. It operates with a wave function ψ , which in the case of *one* particle at least, can be pictured in space, and it uses the mathematical methods of partial differential equations which are in current use by physicists. Schrödinger thought that his wave theory made it possible to return to deterministic classical physics. He proposed (and he has recently emphasized his proposal anew's), to dispense with the particle representation entirely, and instead of speaking of electrons as particles, to consider them as a continuous density distribution $|\psi|^2$ (or electric density $e|\psi|^2$).

To us in Göttingen this interpretation seemed unacceptable in face of well established experimental facts. At that time it was already possible to count particles by means of scintillations or with a Geiger counter, and to photograph their tracks with the aid of a Wilson cloud chamber.

It appeared to me that it was not possible to obtain a clear interpretation of the ψ -function, by considering bound electrons. I had therefore, as early as the end of 1925, made an attempt to extend the matrix method, which obviously only covered oscillatory processes, in such a way as to be applicable to aperiodic processes. I was at that time a guest of the Mas-

sachusetts Institute of Technology in the USA, and I found there in Norbert Wiener an excellent collaborator. In our joint paper¹⁶ we replaced the matrix by the general concept of an operator, and thus made it possible to describe aperiodic processes. Nevertheless we missed the correct approach. This was left to Schrödinger, and I immediately took up his method since it held promise of leading to an interpretation of the ψ -function. Again an idea of Einstein's gave me the lead. He had tried to make the duality of particles - light quanta or photons - and waves comprehensible by interpreting the square of the optical wave amplitudes as probability density for the occurrence of photons. This concept could at once be carried over to the ψ -function: $|\psi|^2$ ought to represent the probability density for electrons (or other particles). It was easy to assert this, but how could it be proved?

The atomic collision processes suggested themselves at this point. A swarm of electrons coming from infinity, represented by an incident wave of known intensity (i.e., $|\psi|^2$), impinges upon an obstacle, say a heavy atom. In the same way that a water wave produced by a steamer causes secondary circular waves in striking a pile, the incident electron wave is partially transformed into a secondary spherical wave whose amplitude of oscillation ψ differs for different directions. The square of the amplitude of this wave at a great distance from the scattering centre determines the relative probability of scattering as a function of direction. Moreover, if the scattering atom itself is capable of existing in different stationary states, then Schrödinger's wave equation gives automatically the probability of excitation of these states, the electron being scattered with loss of energy, that is to say, inelastically, as it is called. In this way it was possible to get a theoretical basis¹⁷ for the assumptions of Bohr's theory which had been experimentally confirmed by Franck and Hertz. Soon Wentzel¹⁸ succeeded in deriving Rutherford's famous formula for the scattering of α -particles from my theory.

However, a paper by Heisenberg¹⁹, containing his celebrated uncertainty relationship, contributed more than the above-mentioned successes to the swift acceptance of the statistical interpretation of the ψ -function. It was through this paper that the revolutionary character of the new conception became clear. It showed that not only the determinism of classical physics must be abandoned, but also the naive concept of reality which looked upon the particles of atomic physics as if they were very small grains of sand. At every instant a grain of sand has a definite position and velocity. This is not the case with an electron. If its position is determined with increasing accuracy, the possibility of ascertaining the velocity becomes less and *vice*

versa. I shall return shortly to these problems in a more general connection, but would first like to say a few words about the theory of collisions.

The mathematical approximation methods which I used were quite primitive and soon improved upon. From the literature, which has grown to a point where I cannot cope with, I would like to mention only a few of the first authors to whom the theory owes great progress: Faxén in Sweden, Holtsmark in Norway²⁰, Bethe in Germany²¹, Mott and Massey in England²².

Today, collision theory is a special science with its own big, solid textbooks which have grown completely over my head. Of course in the last resort all the modern branches of physics, quantum electrodynamics, the theory of mesons, nuclei, cosmic rays, elementary particles and their transformations, all come within range of these ideas and no bounds could be set to a discussion on them.

I should also like to mention that in 1926 and 1927 I tried another way of supporting the statistical concept of quantum mechanics, partly in collaboration with the Russian physicist Fock²³. In the above-mentioned three-author paper there is a chapter which anticipates the Schrödinger function, except that it is not thought of as a function $\psi(x)$ in space, but as a function ψ_n of the discrete index $n = 1, 2, \dots$ which enumerates the stationary states. If the system under consideration is subject to a force which is variable with time, ψ_n becomes also time-dependent, and $|\psi_n(t)|^2$ signifies the probability for the existence of the state n at time t . Starting from an initial distribution where there is only one state, transition probabilities are obtained, and their properties can be examined. What interested me in particular at the time, was what occurs in the adiabatic limiting case, that is, for very slowly changing action. It was possible to show that, as could have been expected, the probability of transitions becomes ever smaller. The theory of transition probabilities was developed independently by Dirac with great success. It can be said that the whole of atomic and nuclear physics works with this system of concepts, particularly in the very elegant form given to them by Dirac²⁴. Almost all experiments lead to statements about relative frequencies of events, even when they occur concealed under such names as effective cross section or the like.

How does it come about then, that great scientists such as Einstein, Schrödinger, and De Broglie are nevertheless dissatisfied with the situation? Of course, all these objections are levelled not against the correctness of the formulae, but against their interpretation. Two closely knitted points of view

are to be distinguished: the question of determinism and the question of reality.

Newtonian mechanics is deterministic in the following sense:

If the initial state (positions and velocities of all particles) of a system is accurately given, then the state at any other time (earlier or later) can be calculated from the laws of mechanics. All the other branches of classical physics have been built up according to this model. Mechanical determinism gradually became a kind of article of faith: the world as a machine, an automaton. As far as I can see, this idea has no forerunners in ancient and medieval philosophy. The idea is a product of the immense success of Newtonian mechanics, particularly in astronomy. In the 19th century it became a basic philosophical principle for the whole of exact science. I asked myself whether this was really justified. Can absolute predictions really be made for all time on the basis of the classical equations of motion? It can easily be seen, by simple examples, that this is only the case when the possibility of absolutely exact measurement (of position, velocity, or other quantities) is assumed. Let us think of a particle moving without friction on a straight line between two end-points (walls), at which it experiences completely elastic recoil. It moves with constant speed equal to its initial speed v_0 backwards and forwards, and it can be stated exactly where it will be at a given time provided that v_0 is accurately known. But if a small inaccuracy Δv_0 is allowed, then the inaccuracy of prediction of the position at time t is $t \Delta v_0$ which increases with t . If one waits long enough until time $t_c = l / \Delta v_0$ where l is the distance between the elastic walls, the inaccuracy Δx will have become equal to the whole space l . Thus it is impossible to forecast anything about the position at a time which is later than t_c . Thus determinism lapses completely into indeterminism as soon as the slightest inaccuracy in the data on velocity is permitted. Is there any sense - and I mean any physical sense, not metaphysical sense - in which one can speak of absolute data? Is one justified in saying that the coordinate $x = \pi$ cm where $\pi = 3.1415\dots$ is the familiar transcendental number that determines the ratio of the circumference of a circle to its diameter? As a mathematical tool the concept of a real number represented by a nonterminating decimal fraction is exceptionally important and fruitful. As the measure of a physical quantity it is nonsense. If π is taken to the 20th or the 25th place of decimals, two numbers are obtained which are indistinguishable from each other and the true value of π by any measurement. According to the heuristic principle used by Einstein in the theory of relativity, and by Heisenberg in the quantum theory, concepts which correspond to no conceivable observation should be eliminated

from physics. This is possible without difficulty in the present case also. It is only necessary to replace statements like $x = \pi$ cm by: the probability of distribution of values of x has a sharp maximum at $x = \pi$ cm; and (if it is desired to be more accurate) to add: of such and such a breadth. In short, ordinary mechanics must also be statistically formulated. I have occupied myself with this problem a little recently, and have realized that it is possible without difficulty. This is not the place to go into the matter more deeply. I should like only to say this: the determinism of classical physics turns out to be an illusion, created by overrating mathematico-logical concepts. It is an idol, not an ideal in scientific research and cannot, therefore, be used as an objection to the essentially indeterministic statistical interpretation of quantum mechanics.

Much more difficult is the objection based on reality. The concept of a particle, e.g. a grain of sand, implicitly contains the idea that it is in a definite position and has definite motion. But according to quantum mechanics it is impossible to determine simultaneously with any desired accuracy both position and velocity (more precisely : momentum, i.e. mass times velocity) . Thus two questions arise: what prevents us, in spite of the theoretical assertion, to measure both quantities to any desired degree of accuracy by refined experiments? Secondly, if it really transpires that this is not feasible, are we still justified in applying to the electron the concept of particle and therefore the ideas associated with it?

Referring to the first question, it is clear that if the theory is correct - and we have ample grounds for believing this - the obstacle to simultaneous measurement of position and motion (and of other such pairs of so-called conjugate quantities) must lie in the laws of quantum mechanics themselves. In fact, this is so. But it is not a simple matter to clarify the situation. Niels Bohr himself has gone to great trouble and ingenuity²⁵ to develop a theory of measurements to clear the matter up and to meet the most refined and ingenious attacks of Einstein, who repeatedly tried to think out methods of measurement by means of which position and motion could be measured simultaneously and accurately. The following emerges: to measure space coordinates and instants of time, rigid measuring rods and clocks are required. On the other hand, to measure momenta and energies, devices are necessary with movable parts to absorb the impact of the test object and to indicate the size of its momentum. Paying regard to the fact that quantum mechanics is competent for dealing with the interaction of object and apparatus, it is seen that no arrangement is possible that will fulfil both require-

ments simultaneously. There exist, therefore, mutually exclusive though complementary experiments which only as a whole embrace everything which can be experienced with regard to an object.

This idea of *complementarity* is now regarded by most physicists as the key to the clear understanding of quantum processes. Bohr has generalized the idea to quite different fields of knowledge, e.g. the connection between consciousness and the brain, to the problem of free will, and other basic problems of philosophy. To come now to the last point: can we call something with which the concepts of position and motion cannot be associated in the usual way, a thing, or a particle? And if not, what is the reality which our theory has been invented to describe?

The answer to this is no longer physics, but philosophy, and to deal with it thoroughly would mean going far beyond the bounds of this lecture. I have given my views on it elsewhere^{2,6}. Here I will only say that I am emphatically in favour of the retention of the particle idea. Naturally, it is necessary to redefine what is meant. For this, well-developed concepts are available which appear in mathematics under the name of invariants in transformations. Every object that we perceive appears in innumerable aspects. The concept of the object is the invariant of all these aspects. From this point of view, the present universally used system of concepts in which particles and waves appear simultaneously, can be completely justified.

The latest research on nuclei and elementary particles has led us, however, to limits beyond which this system of concepts itself does not appear to suffice. The lesson to be learned from what I have told of the origin of quantum mechanics is that probable refinements of mathematical methods will not suffice to produce a satisfactory theory, but that somewhere in our doctrine is hidden a concept, unjustified by experience, which we must eliminate to open up the road.

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