Physical Aspects of Quantum Mechanics.[1]

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THE purpose of this communication is not to give a report on the present status of quantum mechanics. Such a report has recently been published by W. Heisenberg, the founder of the new theory (*Die Naturwissenschaften*, **45**, 989, 1926). Here we shall make an attempt to understand the physical significance of the quantum theoretical formulæ.

At present we have a surprisingly serviceable and adaptable apparatus for the solution of quantum theoretical problems. We must insist here that the different formulations, the matrix theory, Dirac's non-commutative algebra, Schrödinger's partial differential equations, are mathematically equivalent to each other, and form, as a whole, a single theory. This theory enables us to compute the stationary states of atoms and the corresponding radiation, if we neglect the reaction of the radiation on the atoms; it would seem that in this respect we have nothing more to wish for, since the result of every example in which the calculations are carried out agrees with experiment.

This question, however, of the possible states of matter does not exhaust the field of physical problems. Perhaps more important still is the question of the course of the phenomena that occurs when equilibrium is disturbed. Classical physics was entirely concerned with this question, as it was almost powerless toward the problem of structure. Conversely, the question of the course of phenomena had practically disappeared from the quantum mechanics, because it did not immediately fit into the formal developments of the theory. Here we shall consider some attempts to treat this problem on the new mechanics.

In classical dynamics the knowledge of the state of a closed system (the position and velocity of all its particles) at any instant determines unambiguously the future motion of the system; that is the form that the principle of causality takes in physics. Mathematically, this is expressed by the fact that physical quantities satisfy differential equations of a certain type. But besides these causal laws, classical physics always made use of certain statistical considerations. As a matter of fact, the occurrence of probabilities was justified by the fact that the initial state was never exactly known; so long as this was the case, statistical methods might be, more or less provisionally, adopted.

The elementary theory of probability starts with the assumption that one may with reason consider certain cases equally probable, and derives from this the probability of complicated combinations of these. More generally: starting with an assumed distribution (for example, a uniform one, with equally probable cases) a dependent distribution is derived. The case in which the derived distribution is entirely or partly independent of the assumed initial distribution is naturally particularly important.

The physical procedure corresponds to this: we make an assumption about the initial distribution, if possible, one about equally probable cases, and we then try to show that our initial distribution is irrelevant for the final, observable, results. We see both parts of this procedure in statistical mechanics: we divide the phase space into equally probable cells, guided only by certain general theorems (conservation of energy, Liouville's theorem); at the same time we try to translate the resulting space-distribution into a distribution in *time*. But the ergodic hypothesis, which was to effect this translation, and states that every system if left to itself covers in time its phase space uniformly, is a pure hypothesis and is likely to remain one. It thus seems that the justification of the choice of equally probable cases by dividing the phase space into cells can only be derived *a posteriori* from its success in explaining the observed phenomena.

We have a similar situation in all cases where considerations of probability are used in physics. Let us take as an example an atomic collision—the collision of an electron with an atom. If the kinetic energy of the electron is less than the first excitation potential of the atom the collision is elastic: the electron loses no energy. We can then ask in what direction the electron is deflected by the collision. The classical theory regards each such collision as causally determined. If one knew the exact position and velocity of all the electrons in the atom and of the colliding electron, one could compute the deflexion in advance. But unfortunately we again lack this information about the details of the system; we have again to be satisfied with averages. It is usually forgotten that in order to obtain these, we have to make an assumption about equally probable configurations. This we do in the most 'natural' way by expressing the co-ordinates of the electron in its initial path (relative to the nucleus) in terms of angle variables and phases, and by treating equal phase intervals as equally probable. But this is only an assumption, and can only be justified by its results.

The peculiarity of this procedure is that the microscopic co-ordinates are only introduced to keep the individual phenomena at least theoretically determinate. For practical purposes they do not exist: the experimentalist only counts the number of particles deflected through a given angle, without bothering about the details of the path; the essential part of the path, in which the reaction of the atom on the electron occurs, is not open to observation. But from such numerical data we can draw conclusions about the mechanism of the collision. A famous example of this is the work of Rutherford on the dispersion of α-particles; here, however, the microscopic co-ordinates are not electronic phases, but the distance of the nucleus

[1] Extension of a paper read before Section A (Mathematics and Physics) of the British Association at Oxford on Aug. 10, 1926. Translated by Mr. Robert Oppenheimer. The author is very much obliged to Mr. Oppenheimer for his careful translation.

from the original path of the α-particle. From the statistics of the dispersion, Rutherford could prove the validity of Coulomb's law for the reaction between the nucleus and the α-particle. The microscopic co-ordinate had been eliminated from the theoretical formula for the distribution of the particles over different angles of deflexion.

We thus have an example of the evaluation of a field of force by counting, by statistical methods, and not by the measurement of an acceleration and Newton's second law.

This method is fundamentally like that which makes us suspect that a die is false if one face keeps turning up much more often than every sixth throw; statistical considerations indicate a torque. Another example of this is the 'barometer formula.' Of course, we can derive this dynamically, if we regard the air as a continuum and require equilibrium between hydro-dynamical pressure and gravity; but actually pressure is only defined statistically as the average transport of momentum in the collisions of the molecules, and it is therefore not merely permissible but also fundamentally more sound to regard the barometer formula as a counting of the molecules in a gravitational field, from which the laws of the field may be derived.

These considerations were to lead us to the idea that we could replace the Newtonian definition of force by a statistical one. Just as in classical mechanics we concluded that there was no external force acting if the motion of the particle was rectilinear, so here we should do so if an assembly of particles was uniformly distributed over a range. (The choice of suitable co-ordinates leads to similar problems on both theories.) The magnitude of a force, classically measured by the acceleration of a particle, would here be measured by the inhomogeneity of an assembly of particles.

In the classical theory we are of course faced with the problem of reducing the two definitions of force to one, and that is the object of all attempts at a rational foundation of statistical mechanics; we have tried to make clear, though, that these have not been altogether successful, because in the end the choice of equally probable cases cannot be avoided.

With this preparation we turn our attention to quantum mechanics. It is notable that here, even historically, the concept of *a priori* probability has played a part that could not be thrown back on equally probable cases, for example, in the transition-probabilities for emission. Of course this might be merely a weakness of the theory.

It is more important that formal quantum mechanics obviously provides no means for the determination of the position of particles in space and time. One might object that according to Schrödinger a particle cannot have any sharply defined position, since it is only a group of waves with vague limits; but I should like to leave aside this notion of 'wave-packets,' which has not, and probably cannot be, carried through. For Schrödinger's waves move not in ordinary space but in configuration space, that has as many dimensions as the degrees of freedom of the system ($3N$ for N particles). The quantum theoretical description of the system contains certain declarations about the energy, the momenta, the angular momenta of the system; but it does not answer, or at least only answers in the limiting case of classical mechanics, the question of where a certain particle is at a given time. In this respect the quantum theory is in agreement with the experimentalists, for whom microscopic co-ordinates are also out of reach, and who therefore only count instances and indulge in statistics. This suggests that quantum mechanics similarly only answers properly put statistical questions, and says nothing about the course of individual phenomena. It would then be a singular fusion of mechanics and statistics.

According to this, we should have to connect with the wave-equations such a picture as this: the waves satisfying this equation do not represent the motion of particles of matter at all; they only determine the possible motions, or rather states, of the matter. Matter can always be visualised as consisting of point masses (electrons, protons), but in many cases the particles are not to be identified as individuals, *e.g.* when these form an atomic system. Such an atomic system has a discrete set of states; but it also has a continuous range of them, and these have the remarkable property that in them a disturbance is propagated along a path away from the atom, and with finite velocity, just as if a particle were being thrown out. This fact justifies, even demands, the existence of particles, although this cannot, in some cases as we have said, be taken too literally. There are electromagnetic forces between these particles (we neglect for the moment the finite velocity of propagation); they are, so far as we know, given by classical electrodynamics in terms of the positions of the particles (for example, a Coulomb attraction). But these forces do not, as they did classically, cause accelerations of the particles; they have no direct bearing on the motion of the particles. As intermediary there is the wave field: the forces determine the vibrations of a certain function ψ that depends on the positions of all the particles (a function in configuration space), and determine them because the coefficients of the differential equation for ψ involve the forces themselves.

A knowledge of ψ enables us to follow the course of a physical process in so far as it is quantum mechanically determinate: not in a causal sense, but in a statistical one. Every process consists of elementary processes, which we are accustomed to call transitions or jumps; the jump itself seems to defy all attempts to visualise it, and only its result can be ascertained. This result is, that after the jump, the system is in a different quantum state. The function ψ determines these transitions in the following way: every state of the system corresponds to a particular characteristic solution, an *Eigenfunktion*, of the differential equation; for example, the normal state the function ψ_1, the next state ψ_2, etc. For simplicity we assume that the system was originally in the normal state; after the occurrence of an elementary process the solution has been transformed into one of the form

$$\psi = c_1 \psi_1 + c_2 \psi_2 + c_3 \psi_3 \ldots,$$

which represents a superposition of a number of *eigenfunktions* with definite amplitudes c_1, c_2, c_3, ... Then the squares of the amplitudes c_1^2, c_2^2 ..., give the probability that after the jump the system is in the 1, 2, 3, state. Thus c_1^2 is the probability that in spite of the perturbation the system remains in the normal state, c_2^2 the probability that it has jumped to the second, and so on.[2] These probabilities are thus dynamically determined. But what the system actually does is not determined, at least not by the laws that are at present known. But this is nothing new, for we saw above that the classical theory—for example, for the collision problem—only gave probabilities. The classical theory introduces the microscopic co-ordinates which determine the individual process, only to eliminate them because of ignorance by averaging over their values; whereas the new theory gets the same results without introducing them at all. Of course, it is not forbidden to believe in the existence of these co-ordinates; but they will only be of physical significance when methods have been devised for their experimental observation.

This is not the place to consider the associated philosophical problems; we shall only sketch the point of view which is forced upon us by the whole of physical evidence. We free forces of their classical duty of determining directly the motion of particles and allow them instead to determine the probability of states. Whereas before it was our purpose to make these two definitions of force equivalent, this problem has now no longer, strictly speaking, any sense. The only question is why the classical definition is so useful for a large class of phenomena. As always in such cases, the answer is: Because the classical theory is a limiting case of the new one. Actually, it is usually the 'adiabatic' case with which we have to do: *i.e.* the limiting case where the external force (or the reaction of the parts of the system on each other) acts very slowly. In this case, to a very high approximation

$$c_1^2 = 1,\ c_2^2 = 0,\ c_3^2 = 0 \ldots,$$

that is, there is no probability for a transition, and the system is in the initial state again after the cessation of the perturbation. Such a slow perturbation is therefore reversible, as it is classically. One can extend this to the case where the final system is really under different conditions from the initial one; *i.e.* where the state has changed adiabatically, without transition. That is the limiting case with which classical mechanics is concerned.

It is, of course, still an open question whether these conceptions can in all cases be preserved. The problem of collisions was with their help given a quantum mechanical formulation; and the result is qualitatively in full agreement with experiment. We have here a precise interpretation of just those observations which may be regarded as the most immediate proof of the quantised structure of energy, namely, the critical potentials, that were first observed by Franck and Hertz. This abrupt occurrence of excited states with increasing electronic velocity of the colliding electron follows directly out of the theory. The theory, moreover, yields general formulæ 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. This was first pointed out by W. Elsasser (*Die Naturwissenschaften*, **13**, 711, 1925) before the development of the general theory. He started with de Broglie's idea that the motion of particles is accompanied by waves, the frequency and wave-length of which is determined by the energy and momentum of the particle. Elsasser computed the wave-length for slow electrons, and found it to be of the order of 10^{-8} cm., which is just the range of atomic diameters. From this he concluded that the collision of an electron with an atom should give rise to a diffraction of the de Broglie waves, rather like that of light which is scattered by small particles. The fluctuation of the intensities in different directions would then represent the irregularities in the distribution of the deflected electrons. Indications of such an effect are given by the experiments of Davisson and Kunsmann (*Phys. Rev.*, **22**, 243, 1923), on the reflection of electrons from metallic surfaces. A complete verification of this radical hypothesis is furnished by Dymond's experiments on the collisions of electrons in helium (NATURE, June 13, 1925, p. 910).

Unfortunately, the present state of quantum mechanics only allows a qualitative description of these phenomena; for a complete account of them the solution of the problem of the helium atom would be necessary. It therefore seems particularly important to explain the above-mentioned experiments of Rutherford and his co-workers on the dispersion of α-particles; for in this case we have to do with a simple and completely known mechanism, the 'diffraction' of two charged particles by each other. The classical formula which Rutherford derived from a consideration of the hyperbolic orbits of the particles, is experimentally verified for a large range; but recently Blackett has found departures from this law in the encounters between α-particles and light atoms, and has suggested that these might also be ascribed to diffraction effects of the de Broglie waves. At present only the preliminary question is settled, of whether the classical formula can be derived as a limiting case of quantum mechanics. G. Wentzel (*Zeit. f. Phys.*, **40**, 590, 1926) has shown that this is in fact the case. The author of this communication has, furthermore, carried through the computation for the collision of electrons on the hydrogen atom, and arrived at formulæ which represent simultaneously the collisions of particles

[2] We may point out that this theory is *not* equivalent to that of Bohr, Kramers, Slater. In the latter the conservation of energy and momentum are purely statistical laws; on the quantum theory their *exact* validity follows from the fundamental equations. Statistical considerations only apply to quantities, like the angles of deflexion in a collision, which could not be quantised on the Bohr theory of angle variables.

of arbitrary energy (from slow electrons to fast α-particles). As yet this has only been carried out for the first approximation, and so gives no account of the more detailed diffraction effects. This calculation thus yields a single expression for the Rutherford deflexion formula and the cross section of the hydrogen atom for electrons in the range studied explicitly by Lenard. The same method leads to a calculation of the probability of excitation of the H-atom by electronic collision, but the calculations have not yet been completed.

It would be decisive for the theory if it should prove possible to carry the approximation further, and to see whether it furnishes an explanation of the departures from the Rutherford formula.

Even, however, if these conceptions stand the experimental test, it does not mean that they are in any sense final. Even now we can say that they depend too much on the usual notion of space and time. The formal quantum theory is much more flexible, and susceptible of much more general interpretations. It is possible, for example, to mix up co-ordinates and momenta by canonical transformations, and so to arrive at formally quite different systems, with quite different wave functions ψ. But the fundamental idea of waves of probability will probably persist in one form or another.[3]

[3] Compare the article of Dr. P. Jordan, "Philosophical Foundations of Quantum Theory," to appear in a later issue of NATURE.

Benedictus de Spinoza.

By Prof. G. DAWES HICKS.

FIFTY years ago a memorable gathering of distinguished men assembled at The Hague, under the presidency of Prince Alexander of the Netherlands, on the occasion of the two-hundredth anniversary of Spinoza's death. They met in a building which was only a few yards away from the house in the Paviljonensgracht where the philosopher had spent the last few years of his life, and where on Feb. 21, 1677, he died. The principal speaker at that gathering was Ernest Renan; and, having in mind the monument about to be erected, and referring to the humble dwelling hard by, Renan exclaimed: "From his granite pedestal Spinoza will teach us all to follow the way which he found to happiness, and, centuries hence, men of learning, crossing the Paviljonensgracht, will say to themselves, 'It is perhaps from this spot that God was most nearly seen.'" The statue was finished in 1880; and now, on the two-hundred-and-fiftieth anniversary of Spinoza's death, it is proposed to complete the memorial by acquiring the house, to be called the *Domus Spinozana*, and equipping it as a home for research and as a meeting-place for scientific workers of various nationalities. It will be a fitting tribute to one of the world's greatest minds.

The story of this lonely thinker's life has frequently been told. Born at Amsterdam, whither his father had migrated from Portugal about thirty years previously, on Nov. 24, 1632, he spent the whole of his days in Holland. His mother died when he was barely six years old, and his father when he was twenty-two. Two years after his father's death he was excommunicated by the Rabbis; and from that period onwards he lived in modest lodgings, supporting himself at first partly by teaching and partly by grinding lenses for spectacles and optical instruments, in which latter occupation he persevered to the end. Until 1660 he remained in Amsterdam, where he became the leading spirit of a small circle of friends, who after his departure met periodically to discuss philosophical papers which he sent to them. From 1660 until 1663 he resided in Rhynsberg, near Leyden, and there he wrote the "De Intellectus Emendatione," part of his exposition of Descartes' "Principia" with the appendix, "Cogitata Metaphysica," and perhaps a portion of the "Ethics." In 1663 he removed to Voorburg, near The Hague, and stayed there until 1670. At Voorburg he was at first occupied with the "Ethics," but laid it aside in order to devote himself to the "Tractatus Theologico-Politicus," which seemed to him to be the more urgently needed, and which was published anonymously in 1670. In 1670 he removed to The Hague, where he remained until his death in 1677. Here he finished the "Ethics" and wrote the unfinished "Tractatus Politicus," both of which were published in the "Opera Posthuma," that appeared before the end of the year 1677. Nearly two centuries later there was discovered and published the Dutch text of a work of Spinoza's which appears to have been called "Tractatus de Deo et homine ejusque felicitate," written about the year 1660.

At the beginning of the treatise "De Intellectus Emendatione," Spinoza relates the circumstances that led him to devote himself to philosophical inquiry. The ordinary objects of human pursuit —sensuous enjoyment, wealth, station—had all evinced themselves, even when attained, as incapable of yielding real and lasting happiness. The reason seemed to him to be due to the fact that, while these objects are invariably transitory and fleeting in character, in making them ultimate ends men take them to be permanent and self-sufficing. True blessedness (*beatitudo*) could come only from being in possession of a changeless and abiding object of love, and there is, he was assured, no way of obtaining that possession save by knowing things as they actually are. For it was because in everyday experience our apprehension of things is fragmentary and piecemeal, because we contemplate them in isolation and from a limited point of view, that we are misled into desiring some of them as though they could constitute for us the supreme ends of life.

Scientific knowledge would, on the other hand, reveal the interconnexion of finite events, their dependence upon each other, and upon reality as a whole. The Whole alone could be perfect and eternal; and love of it could alone satisfy the