THE INTERPRETATION OF QUANTUM MECHANICS *

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THE following pages are a reply to Erwin Schrödinger's article, 'Are There Quantum Jumps? Parts I and II', published in August and November 1952, in this *Journal*. A discussion on this subject was to be held in the meeting of the Philosophy of Science Group on 8th December 1952, and I was asked to open it. I accepted this honour rather reluctantly, for I find it awkward to display in public a disagreement on a fundamental question with one of my best and oldest friends. Yet I had several motives for accepting the challenge: The first is my conviction that no discrepancy of opinion on scientific questions can shake our friendship. The second, that other good and old friends of the same standing as Schrödinger, such as Niels Bohr, Heisenberg and Pauli, share my opinion. My third, and the most important reason for entering into this discussion of Schrödinger's publication is that by its undeniable literary merits, the width of its historical and philosophical horizon, and the ingenious presentation of the arguments, it may have a confusing effect on the mind of those who, without being physicists, are interested in the general ideas of physics.

The discussion on 8th December was rather frustrated by Schrödinger's absence, due to serious illness. I read my prepared introduction and answered questions. But this was, of course, not fair play to Schrödinger himself. Therefore I have to state my case in print. The following is a slightly enlarged version of my introduction to the discussion. As such, it covers not in the least all points made by Schrödinger, but only those which seemed to me suited for a debate amongst philosophers.

1 *Schrödinger's Case Restated*

The whole discrepancy is not so much an internal matter of physics, as one of its relation to philosophy and human knowledge in general. Any one of us theoretical physicists, including Schrödinger,

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confronted with an actual problem would use the same, or at least equivalent mathematical methods, and if we should obtain concrete results our prediction and our prescription for the experimental verification would be practically the same. The difference of opinion appears only if a philosopher comes along and asks us : Now what do you really mean by your words, how can you speak about electrons to be sometimes particles, sometimes waves, and so on? Such questions about the real meanings of our words are just as important as the mathematical formalism. Schrödinger challenges the use of words in the current interpretation of the formalism ; he suggests a simple, puristic language and maintains that it can cope with the situation. We answer, that this purism is not only perfectly impracticable by its clumsiness, but also quite unjustifiable from the historical, psychological, epistemological, philosophical standpoint.

I suppose you have all read Schrödinger's paper. What he maintains can be condensed in a few sentences : The only reality in the physical world is waves. There are no particles and there are no energy quanta $h\nu$; they are an illusion due to a wrong interpretation of resonance phenomena of interfering waves. These waves are connected with integers in a way well known from the vibrations of strings and other musical instruments, and these integers have deluded the physicist into believing that they represent numbers of particles. But there is a special resonance law, characteristic of quantum mechanics, according to which the sum of the eigenfrequencies of two interacting systems remains constant. This has been interpreted by the physicists as the conservation law of energy applied to quanta or particles. But there are no such things. Any attempt to describe the physical phenomena in terms of particles without contradicting the well-established wave character of their propagation in space, leads to impossible, unacceptable conceptions, like the assumption of timeless quantum jumps of particles from one stationary state to another. Moreover, if you try to describe a gas composed of particles you are compelled to deprive them of their individuality ; if you write the symbol (A, B) to express that A is here at one place, B there at another, the two situations (A, B) and (B, A) are not only physically indistinguishable, but represent statistically only one case, not two, as common sense would demand. All these and many other difficulties disappear if you abandon the particle concept and use only the idea of waves.

2 *Are There Atoms?*

It is only a few years ago since Schrödinger published a paper under the title '2,500 Years of Quantum Mechanics', in which he stressed the point that Planck's discovery of the quantum was the culmination of a continuous development starting with the Greek philosophers Leucippus and Democritus, the founders of the atomistic school. At that time he obviously thought the idea that matter is composed of atoms, ultimate indivisible particles, a great achievement. Now he rejects the same idea, because the execution of the programme leads to some grinding noise in our logical machinery.

It is this anti-atomistic attitude which appears to me the weakest, in fact quite indefensible, point in Schrödinger's arguments against the current interpretation of quantum mechanics. All other points are of a more technical nature, but this one is fundamental. Schrödinger opens both parts of his paper by a section entitled 'The Cultural Background', in which he accuses the theoretical physicists of our time of having lost the feeling of historical continuity and over-estimating their own achievements as compared with those of their forerunners. He gives examples of such defaults which I do not wish to defend, but I think that he himself offers an example which is even worse.

The atomistic idea, since its revival through Daniel Bernoulli (1738) in the kinetic theory of gases and through Dalton (1808) in chemistry, has been so fertile and powerful that Schrödinger's attempt to overthrow it appears to me almost presumptuous, and in any case an obvious violation of historical continuity.

3 *Waves Instead of Atoms*

Such a violation would be justified if he could supply a better and more powerful substitute. That is exactly what he claims. He says that everything in physics and in chemistry as well can be described in terms of waves. The ordinary reader will certainly understand this as meaning: ordinary waves of some not specified substance in ordinary 3-dimensional space. Only in the last section of Part II (p. 241) he indicates that one has in general to do with waves in a multi-dimensional space, but 'To enlarge on this in general terms would have little value'. I think this is a very essential point which must be discussed. But before doing so I wish to say that I regard

Schrödinger's wave mechanics as one of the most admirable feats in the whole history of theoretical physics. I also know that his motive was his dislike of Bohr's theory of stationary states and quantum jumps, which he wished to replace by something more reasonable. I quite understand his triumph when he succeeded in interpreting those horrible stationary states as innocuous proper vibrations and the mysterious quantum numbers as the analogy to the numbers of musical overtones. He is in love with this idea.

I, of course, have no personal attachment to the waves. I have been involved, together with Heisenberg and Jordan, in the development of another method, matrix mechanics, in which stationary states and quantum jumps have a natural place. But I have no special preference for the matrix theory. As soon as Schrödinger's wave equation was published, I applied it to the theory of collisions; this suggested to me the interpretation of the wave function as probability amplitude. I welcomed Schrödinger's elegant proof of the formal equivalence of wave mechanics and matrix mechanics. I do not plead in favour of matrix mechanics, or its generalisation due to Dirac, nor do I attack wave mechanics. I wish to refute the exaggerated claims of Schrödinger's paper from which the non-expert reader must get the impression that all phenomena can be described in terms of ordinary waves in ordinary space.

The physicist knows that this is not true. In the case of a 2-body problem (like the hydrogen atom) one can split the wave equation into two, one for the motion of the centre of mass, the other for the relative motion, both in 3-dimensional space. But already, in the case of the 3-body problem (for instance, the helium atom, one nucleus with two electrons) this is impossible; one needs a 6-dimensional space for the relative motion. In the case of N particles one needs a 3(N-1)-dimensional space which only in singular cases is reducible to a smaller number of dimensions.

But this means that the claim of simplicity and of 'Anschaulichkeit', the possibility of seeing the process in space, is illusory.[1] In fact a multi-dimensional wave function is nothing but a name for the abstract quantity ψ of the formalism, which by some of the modern

[1] In another article which has recently appeared ('Louis de Broglie, Physicien et Penseur', ed. Albin Michel, Paris, 1952) Schrödinger remarks that the 3-dimensionality of the waves can be saved with the help of second quantisation. But the 'Anschaulichkeit' is then also lost and the statistical character of the ψ-function is introduced on an even deeper and more abstract level.

theorists also goes under the more learned title of 'state vector in the Hilbert space'. Any attempt to describe phenomena, except the simplest ones, in terms of these multi-dimensional wave functions, means the formulation of the concise contents of mathematical formulæ in words of ordinary language. This would be not only extremely clumsy but practically impossible.

In fact, Schrödinger makes no attempt in this direction. All his examples are chosen in such a way that a 3-dimensional representation is possible. He restricts himself to cases which in the particle language correspond to independent (non-interacting) particles. Then he shows that these particles are not behaving as good, well bred particles, like a grain of sand, should behave.

4 *Why Atoms are Indispensable*

I think that in spite of these abnormities the concept of particle cannot be discarded.

As I said already, for the calculations of the theoretical physicist the whole question is almost irrelevant. But if he wants to connect his results with experimental facts, he has to describe them in terms of physical apparatus. These consist of bodies, not of waves. Thus at some point the wave description, even if it were possible, would have to be connected with ordinary bodies. The laws governing the motion of these tangible bodies are undoubtedly those of Newtonian mechanics. Thus the wave theory has necessarily to provide means to translate its results into the language of mechanics of ordinary bodies. If this is done systematically, the connecting link is matrix mechanics, or one of its generalisations. I cannot see how this transition from wave mechanics to ordinary mechanics of solid bodies can be possibly avoided.

Let us look at the matter the other way round, starting from ordinary bodies. These can be divided into parts, and sub-divided into still smaller parts. The Greek idea was that this procedure has an end somewhere, when parts become particles, atoms, which are indivisible.

Modern theory has modified this view to some degree, but I need not go into details which you all know. The parts of a substance obtained by division and subdivision are of the same physical nature until you approach the chemical atom. This is not indivisible, but its parts are of a different nature, particles of a more subtle quality, nucleons and electrons. Then we discover that the smallest units, the chemical atoms and still more the nucleons and electrons have not only different qualities, but decidedly strange qualities, strange if you expect always to find the same as you are accustomed to. They behave differently from the powder particles into which you have first ground your material. They have no individuality, their position and velocity can be determined only with a restricted accuracy (according to Heisenberg's uncertainty relation) and so on. Shall we then say, well, there are no particles any more, we must regretfully abandon the use of this simple and attractive picture?

We can do it if we take a strictly positivistic standpoint: The only reality are the sense impressions. All the rest are 'constructs' of the mind. We are able to predict, with the help of the mathematical apparatus of quantum mechanics what the experimentalist will observe under definite experimental conditions, the current shown by a galvanometer, the track in a photographic plate. But it is meaningless to ask what there is behind the phenomena, waves or particles or what else. Many physicists have adopted this standpoint. I dislike it thoroughly, and so does Schrödinger. For he insists that there is something behind the phenomena, the sense impressions, namely waves moving in a still scantily explored medium. Recently an American physicist, Bohm, has taken the opposite standpoint; he claims that he can interpret the whole of quantum mechanics in terms of ordinary particles with the help of parameters describing unobservable 'concealed' processes.

5 *How to Modify the Atomistic Concept*

I think that neither of these extremist views can be maintained. The current interpretation of quantum theory which tries to reconcile both aspects of the phenomena, waves, and particles, seems to me on the right way. It is impossible to give here an account of the intricate logical balance. I wish only to illustrate the manner in which the particle concept is adapted to new conditions, by some examples from other fields, where a similar situation is found. It is of course no new situation that a concept in its original meaning turns out too narrow. But instead of abandoning it, science has applied another method, which is by far more fertile and satisfactory. Consider the example of the number concept. Number means originally what we now call integer, 1, 2, 3 . . . Kroneker has said that God has made the integers, while the rest are human work. Indeed, if you define numbers as the means of counting things, even rational numbers like 2/3 or 4/5 are not numbers any more. The Greeks extended the concept of number to them by restricting the consideration to a finite set where a smallest unit (the greatest common denominator) can be found. But then they made the fundamental discovery that the diagonal of the square (of the side 1), which we write $\sqrt{2}$, is not a number in this sense; but great as their logical genius was, they did not make the next constructive step. They had not the pluck to generalise the number concept in such a way that $\sqrt{2}$ was included, but invented an ingenious yet rather clumsy geometrical method to deal with such cases. This was the stumbling block which retarded mathematics for about 2,000 years. Only in modern times the necessary generalisation of the idea of number was made so as to include these things such as $\sqrt{2}$, still called irrational. But then further generalisations followed, the introduction of algebraic, transcendental, complex numbers. You cannot count with the help of these. But they have other, more formal properties in common with the integers, and the latter are a special case. Similar generalisations of concepts are common in mathematics. But they appear also in physics. Sound was certainly defined as that which you can hear, light as that which you can see. But we speak now of inaudible sound (ultrasonics) and invisible light (infrared, ultraviolet). Even in ordinary life this process of extension of meaning is going on. Take the concept of democracy which originally meant the organisation of government in the Greek city states where the citizens assembled in the market place to discuss and decide their problems; today, it is used for the government of gigantic states by parliamentary representation. In Russia it even means something which we should regard as the opposite of democracy. Therefore we had better return to the safe ground of science.

I maintain that the use of the concept of particles has to be justified in the same way. It must satisfy two conditions: First it must share some (not in the least all) properties of the primitive idea of particle (to be part of matter in bulk, of which it can be regarded as composed), and secondly this primitive idea must be a special, or better, limiting case.

Now it is exactly in this sense that the particle concept is used in quantum mechanics. I cannot see any objection to it. Schrödinger's examples seem to me of the kind which prohibited the Greeks from admitting the representation of the diagonal of the unit square as a number; it differs from all possible ratios of integers, as can easily be seen. The effect of accepting Schrödinger's thesis would perhaps not be equally portentous, because he does not attack the formal theory, only its philosophical background. He would even allow the physicists and chemists to use the particle language with a proper 'as if'. Imagine a textbook of chemistry written according to this prescription. Water behaves as if it were composed of molecules H_2O, each of which again reacts as if composed of two H-atoms and one O-atom. But when we continue, each H-atom has properties as if it were composed of a nucleus and an electron, we transgress the permitted domain of 'as if', for here Schrödinger insists that there is no particle called electron but a charged wave around the nucleus which itself actually is also a wave of some kind. But when we then wish to deal with a photo-ionisation of this H-atom we have to fall back to his 'as if' to describe the discontinuous recording of a Geiger counter.

All our language, in life and in science, is growing through generalisations of concepts, which sometimes are first considered to be 'as ifs', but then are amalgamated and become legitimate words in their own right. For this end it is necessary to fix the rules of their employment in a reasonable manner. This process, in which Niels Bohr has played a leading part, is still going on, and, I think, with fair success. One can, of course, pick out points where some logical hardness or roughness appears, and that is what Schrödinger has done.

On the other hand, Schrödinger cannot avoid the use of the words particles or atoms. They appear in many of his examples; otherwise his words would convey no meaning. For instance, when he speaks about quantum statistics of gases he has to discuss a wave equation in a multi-dimensional space. This equation has, of course, a simple meaning if considered from the particle standpoint; it is the wave-mechanical translation of the law of conservation of kinetic energy for n particles. Now Schrödinger is compelled to disown this translation, the lovely child of his brain, for otherwise he would admit there are, in some sense, particles. He has to take the $3n$-dimensional wave equation as something given to him by inspiration and confirmed by experiments. This is a distortion of historical facts.

6 *Collisions*

Though I wish to avoid technical details I have to say a few words about the problem of collisions which Schrödinger discusses at several places (Sections 6 and 8). He finds the usual quantum-mechanical treatment faulty, he accuses the physicists of loose speech, he preaches to them that 'Science is not a soliloquy' and prophecies that their work will be forgotten in 2,000 years' time, while that of Archimedes of Galileo, has survived similar periods. In a letter to me he maintains that 'almost all great successes of quantum mechanics consist of the satisfactory calculation of extended systems of eigenvalues (of the energy), each from a definite, more or less plausible assumption about the nature of the system in question (Hamilton operator), and have nothing at all to do with the statistical interpretation. On the other side there are the scattering experiments (calculation of differential cross-sections of interaction) and things like that. Only the Klein-Nishina formula is apparently quantitatively confirmed. (The latter represents the scattering of light, or photons, by an electron.)' He further doubts that the statistical interpretation, which I have first suggested and which has been formulated in the most general way by von Neumann, is applicable to these cases at all.

To this I reply that in principle we know about the eigenvalues of the energy (Hamiltonian) of material systems only from experiments about emission, absorption, scattering of light or electrons. These processes are all due to the coupling of the system considered with a 'messenger' field (the electromagnetic or photon field, or de Broglie's electron field) and it seems to me quite arbitrary to pick out the scattering as less reputable than the other two effects. Further, a look into the literature, for instance, the well-known book by Mott and Massey, or the important articles by Niels Bohr, on the penetration of particles through matter and innumerable other papers and books, shows that the number of more or less quantitative confirmations of the quantum-statistical scattering laws is very large, and that there are qualitative confirmations of a particularly convincing kind. Even in nuclear physics, where the knowledge of the interaction law (Hamiltonian) is doubtful and scanty, the principles of the statistical theory have been used with great success, of which the atomic bomb is one very impressive example.

Concerning Schrödinger's scepticism about the applicability of the general scheme for transitions (quantum jumps) to the case of collisions I am unable to follow his reasoning. He describes the procedure as if a collision were a transition between two states of different energy. In fact the typical 'elastic' collision is a transition between states of equal energy but different momentum vectors. My original method dealing with this case avoids any reference to time; it considers the steady state of an incoming wave (representing a beam of 'messenger' particles), transformed by its interaction with an atom into a spherical wave (representing the out-going, scattered particles). In this way of considering the process there is no initial and no final state, concepts which seem to Schrödinger ill-defined. They appear in Dirac's version of the collision theory which he developed in order to consider collisions as a special case of the general theory of transitions in time (formulated first in my papers on 'adiabatic invariants' and in Dirac's simultaneous publications, and perfected by J. v. Neumann). But Dirac has shown that his method (involving time) is mathematically equivalent to the 'stationary' method; the conceptual difficulties which worry Schrödinger are therefore only a matter of careful formulation.

Another objection which he raises refers to the approximation method which I introduced in my early papers to solve the very complicated mathematical equations of scattering. This method gives reasonable, and often well-confirmed, results in the first approximation; but higher approximations are difficult to obtain, and if they can be constructed there are cases where they lead to divergent integrals. However, there are other methods which use quite different expansions (for instance, in terms of spherical harmonics and Bessel functions) and lead to results which are mathematically sound and well confirmed by experiments.

I cannot see at all that these purely mathematical objections have anything to do with the question of 'particles-waves', or 'quantum jumps'. For if we accept Schrödinger's standpoint that there are no particles, only waves, the scattering calculations would be exactly the same as before; the only difference would be that we would speak about the intensity of the incoming and the outgoing wave (electromagnetic, electronic, protonic, etc., wave, as the case may be), and omit to interpret this intensity as the probability of the appearance of particles. The real problem raised by Schrödinger is, whether this probability interpretation is significant. His mathematical scruples have nothing to do with it. To decide this significant question, consider, for instance, Rutherford's experiments about the scattering of α-rays by nuclei. Here, by a kind of lucky mathematical incidence, the classical calculation (using particles obeying the laws of Newtonian mechanics) and the wave-mechanical calculation (which can be performed rigorously in this case) give the same result. This result is confirmed by counting the α-particles in the incoming and in the outgoing beam (for different directions of scattering). The result is completely independent of the method of counting, whether by scintillations of a zinc-sulphide screen, or by different types of counters. How does Schrödinger account for this fact? As far as I see he has no ready explanation. He seems to think that it is not a discontinuity in the beam, which produces the countable events, but some feature of the counting instrument. But how then is it to be explained that the result is independent of the type of instrument, even to that degree, that sparks in the little crystals of the zinc-sulphide screen and gas tubes, connected with elaborate amplifier apparatus, count the same (average) number of events? Here Schrödinger's bias against the particle idea leads him to an almost mystical attitude; he hopes that the future will solve this riddle in a satisfactory way.

7 *Conclusion*

I have refrained from discussing the statistical interpretation of quantum mechanics in detail. This is not a simple matter, and demands not only the knowledge of a complicated mathematical formalism, but a certain philosophical attitude: the willingness to sacrifice traditional concepts and to accept new ones, like Bohr's principle of complementarity. I am far from saying that the present interpretation is perfect and final. I welcome Schrödinger's attack against the complacency of many physicists who are accepting the current interpretation because it works, without worrying about the soundness of the foundations. Yet I do not think that Schrödinger has made a positive contribution to the philosophical problems. It is very awkward for me to criticise the philosophy of a friend whom I deeply admire as a great scholar and deep thinker. Therefore I shall make use of a method of defence which Schrödinger himself is not too proud to use, namely the quotation of authorities who share my own opinion. I choose as my witness W. Pauli who is generally acknowledged to be the most critical, logically and mathematically exacting amongst the scholars who have contributed to quantum mechanics. I translate a few lines from a letter (in German) which I have recently received:

> Against all retrograde efforts (Schrödinger, Bohr, etc., and, in a certain sense, also Einstein) I am certain that the statistical character of the ψ-function, and thus of the laws of nature—which you have, right from the beginning, strongly stressed in opposition to Schrödinger—will determine the style of the laws for at least some centuries. It is possible that later, for example in connection with the processes of life, something entirely new may be found, but to dream of a way back, back to the classical style of Newton-Maxwell (and it is nothing but dreams which those gentlemen indulge in), that seems to me hopeless, off the way, bad taste. And we could add 'it is not even a lovely dream'.

What Pauli means by the 'style' of a conceptual structure you might prefer to call the philosophical attitude of a period, which determines the cultural background. It is here that we differ, and the auspices of an agreement are therefore frail.

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