Do we need the Concept of Particle?

Early wave-mechanical account of radioactivity and tracks in a Wilson cloud chamber[1]

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Abstract : At the end of the 1920's, purely wave-mechanical accounts of atomic and subatomic processes were quite numerous, due to the relative familiarity of physicists with Schrödinger's wave equation. The interpretative weakness of these accounts, as denounced by Heisenberg, was that they were seemingly difficult to reconcile with the discontinuity of microscopic phenomena, and especially with the concept of particle. This difficulty is an early statement of the famous measurement problem of quantum mechanics. But Nevill Mott demonstrated that one can perfectly bypass the measurement problem, and yet offer a coherent account of quantum processes. The most elaborated statement of his strategy of avoidance was presented in his theory of α-ray tracks in a cloud chamber, published in 1929. The performative solution (or dissolution) of the measurement problem presented in this paper of Nevill Mott is found to be remarkably convincing, and able to inspire a renewal of the debate about the interpretation of quantum mechanics almost one century after its publication.

Nevill Mott's paper of 1929, entitled "The wave mechanics of α-ray tracks", is deservedly well-known as a seminal work about certain fundamental aspects of the quantum theory of measurement[2]. I think that its importance lies in its having shown how it is possible to avoid stating an *explicit* solution to the measurement problem of quantum mechanics, and yet maintain an efficient and coherent attitude in the use of the quantum formalism, thus giving this problem what I will call a *performative* solution (or dissolution). Let me first put his theory of α-ray tracks back in its historical context.

As it is well known, quantum mechanics was born twice, in 1925 and 1926. Heisenberg's matrix mechanics of 1925 was the end-product of a process of abstraction from the corpuscularian picture of the Rutherford-Bohr atom. What was retained of this picture was *only an algebraic skeleton.* An ordered set of matrices and a finite difference scheme were related respectively to the classical variables and the classical differential equations by means of a generalized version of Bohr's correspondence principle, thus keeping the general law-like structure of classical mechanics but *not* the associated corpuscularian concept of trajectory. Yet the corpuscularian categories were still at work in the thought of many physicists, and in the fall of 1926 Heisenberg tried to show how they could be rescued somehow in the new theory. According to Heisenberg's retrospective account in *Physics and Beyond*[3], his "uncertainty relations" were aimed at achieving this task. They provided a quantitative solution to the apparent contradiction between the lack of any formal equivalent of the concept of trajectory in quantum mechanics (not only in wave mechanics, but also in matrix mechanics), and the tracks that were nevertheless observed in Wilson cloud chambers.

As for Schrödinger's wave mechanics in the beginning of 1926, it made an exclusive use of continuous mathematics; it tended to *replace* the description of the motion of one electron in the atom by the description of a three-dimensional standing wave ψ. This allowed Schrödinger to recover quantum conditions without postulating them, since they arose "in the same natural way as [the notion of whole numbers] does in the case of the node-numbers of a vibrating string"[4]. However, such a representation provided no clue for line intensities and polarizations, and Schrödinger thus focused his attention on the electric charge density $-e\psi\psi^*$ rather than directly on the wave-like process ψ. This move did not solve all of the problems either, and Schrödinger soon faced a series of difficulties that jeopardized his own view of the quantum theory. First, he had to accept reluctantly that problems involving many degrees of freedom can only be solved by means of multidimensional ψ-functions in phase space, and *not* by means of several three-dimensional waves in ordinary space; this was likely to put an end to his hope of recovering *Anschaulichkeit* in the new theory. Second, the concept of wave packet he had invented to maintain something of the notion of particle trajectory in wave mechanics, had an important defect: wave packets generally disperse and they are thus unable to mimic an enduring particle-like location such as that apparently manifesting itself in the tracks in cloud chambers. Third, the physicists of the Göttingen-Copenhagen group, especially Heisenberg and Bohr, pointed out that pure wave mechanics was likely to be unable to account for the very experimental effects that prompted the introduction of the quantum of action, such as the photo-electric effect, black body radiation, and *also* the discontinuous ionization of hydrogen atoms that is the basic mechanism by which water droplets develop in cloud chambers, thus giving rise to tracks. Fourth, even though Schrödinger eventually accepted Born's suggestion that the ψ-functions can be used to calculate probabilities of *final* experimental events, he could never make sense of the idea that the ψ-functions are only probabilistic tools that supervene on *intermediate* real processes made of stochastic particle motions and quantum jumps.

What was needed at this point was a unification of the two accounts of quantum phenomena, namely matrix mechanics and wave mechanics, and a common solution to their difficulties. Even though Schrödinger had published what he took to be a proof of equivalence of matrix mechanics and wave mechanics by the summer of 1926, full unification was only obtained by von Neumann a few years later[5]. Von Neumann's strategy tended to leave aside the initial incompatible representations that guided the creators of quantum mechanics, and to obtain unification at the higher level of abstraction of the Hilbert-space formalism. Another (more philosophical) approach was Bohr's complementarity. This approach consisted in retaining *both* the wave representation and the corpuscular representation by ascribing them the status of purely symbolic pictures whose relevance is restricted to mutually incompatible experimental situations, or to mutually incompatible ways of analysing a measurement process.

We will now see how this basic debate was echoed in the subtle interplay of similarities and differences in the way Mott and Heisenberg accounted for the phenomenon of tracks in Wilson's cloud chambers. Mott was clearly attempting to promote a purely wave-mechanical standpoint, although in order to do so he had to go beyond the position that was reached by Schrödinger himself at the end of the 1920s. As for Heisenberg, he rather tried to enact Bohr's concept of complementarity as fully as possible. Despite this discrepancy, the relevant chapter of Heisenberg's *Physical Principles of the Quantum Theory*[6] of 1930 mostly relies on Mott's calculation. There, the strong wave-mechanical bias of Mott's paper is played down and replaced by an alternation of wave and particle representations.

So, let us begin with Mott. It is especially striking that Mott's paper contains an implicit answer to the four major difficulties that forced Schrödinger to relinquish his own interpretation of quantum mechanics at the end of the 1920s. To reach this aim, Mott followed quite closely the interpretation of wave mechanics which was developed by Charles Galton Darwin[7] a few weeks earlier in 1929, but he made these ideas more concise, more operational, and also less metaphysical.

The initial motivation of both Darwin's and Mott's papers was Gamow's theory of radioactive disintegration of 1928, which was also formulated independently by Gurney and Condon[8]. In this theory, the emission of α-rays was explained wave-mechanically by means of potential-barrier penetration. The amplitude of the wave function decreases exponentially with the thickness of the potential barrier, but it has a non-zero value outside, and this accounts for the non-zero probability of the leaking of α-rays out of the nucleus. Now, the problem is that as soon as they have emerged, the α-rays appear to have essentially corpuscle-like properties, for they give rise to tracks in cloud chambers. Charles Galton Darwin's project was then to restore a certain conceptual homogeneity between the explanation of the radioactive emission (which is based on pure wave mechanics) and the account of detection (which must apparently involve corpuscularian categories). He wished to make sense of the α-ray tracks without resorting to the process that consists in imagining that at each observation "the wave [turns] into a particle and then back again [into a wave]"[9]. He wanted "to show how a discussion *only* involving the wave function ψ would give spontaneously the results which simple intuition would suggest could only be due to particles"[10]. As for Mott, he also insisted that "the wave mechanics unaided ought to be able to predict the possible results of any observation that we could make on a system"[11].

Thus, at first sight, Darwin and Mott adopted exactly the position that Schrödinger was advocating in 1926. However, they introduced many important qualifications to this position, and it was precisely by means of these qualifications that they were able to overcome the difficulties which plagued the original version of wave mechanics. To begin with, they completely renounced the concept of a wave packet that (according to Heisenberg's celebrated paper of 1927), would have to undergo successive "reductions" each time an observation is performed[12]. No problem of dispersion of wave packets is thus to be feared, since no wave packet is considered. Besides that, Darwin and Mott felt absolutely no reluctance towards multi-dimensional wave functions in phase space. According to Darwin, the relevant wave function must contain factors corresponding not only to the α-particle, but also to every ionizable atom in the cloud chamber. "Before the very first collision, (the wave function) can be represented as the product of a spherical wave for the α particle, by a set of more or less stationary waves for the atoms. ... [The] first collision changes this product into a function in which the two types of coordinates are inextricably mixed". This is a very clear early statement of what we now call the *entanglement* of wave functions after Schrödinger's papers of 1935[13]. Darwin even insisted that "the trouble ... in the quantum theory has only arisen through attempts to work with an incomplete ψ", namely a ψ-function that does not incorporate relevant elements of the measuring device. As for Mott, he noticed that "... we are really dealing with wave functions in the multispace formed by the coordinates both of the α-particle and of every atom in the Wilson chamber". It is then quite obvious that the loss of *Anschaulichkeit* related to the use of multidimensional ψ-functions did not worry Mott and Darwin at all. Mott did not even mention this point in his paper; as for Darwin, the reason why he noticed that a wave function for two or more bodies is "outside our ordinary space intuition", was only that he wished to emphasize the importance of a careful mathematical study of the wave mechanics of many-body systems. The reason for this lack of interest in the problem of *Anschaulichkeit* is probably that, after their training in the atmosphere of the Cavendish laboratory at Cambridge, both Darwin and Mott were more concerned with affording a proper connection between theory and *experiments* than by the autonomous life of spatio-temporal representations.

But at this point a new problem arises. Let us suppose that we accept the complete loss of intuitive representations in ordinary space. We can then hardly say, as Mott did, that our initial difficulty in picturing how the spherical wave function associated with the α particle can produce a straight track, is solved by considering compound wave functions in multidimensional phase space. Apparently, this difficulty in picturing the process has been enhanced rather than solved by a systematic study of the entanglement of wave functions. Actually, however, even though Mott's and Darwin's work did not fulfill our need for pictures, it contributed to a proper understanding of what can be asked and what should not be asked from the quantum mechanical account of a phenomenon. According to Mott and Darwin, the quantum mechanical account, including when this account uses entangled wave functions, does not provide the slightest element of *description* of the putative processes underlying the phenomenon; it only enables us "to *predict* the possible results of any observation". In other terms, "interpreting the wave function should give us simply the *probability* that such and such an atom is ionized". This sentence perfectly fits Schrödinger's late conception of his wave mechanics. In a lecture given in the early 1950's, Schrödinger insisted, almost in the same terms as Mott's, that what he called the "interpretation" of a wave function was nothing more and nothing less than the (probabilistic) connection between the continuously changing overall wave functions and the observed outcomes of a measurement[14].

As soon as this is accepted, the third and fourth difficulties on which Schrödinger stumbled in 1926-1927 are defused. Indeed, it can be shown easily (and it has been shown by Schrödinger himself) that the quantum-theoretical phenomena that were invoked by

Heisenberg and Bohr against the possibility of making exclusive use of wave mechanics, can in fact be dealt with by this theory, provided it is applied to a system large enough to include a relevant part of the measurement device. One must only note that the wave-mechanical model has no ambition other than providing a method to calculate the probabilities which are needed at the end of the process. It does not offer a *description* of the intermediate events between the preparation of an experiment and the final outcome. It says nothing about the continuity or discontinuity of the putative real processes that take place between the preparation and the final pointer readings. It actually does not *need* to say anything of this kind to be efficient; and those who claim that discontinuities must be imposed onto it by means of wave-packet reductions to account for certain phenomena are flatly wrong.

Accordingly, Mott and Darwin insisted, again in good agreement with Schrödinger's late views, that the multidimensional wave-mechanical account must be pushed as far as possible, and that any reference to corpuscular or discontinuous processes must be delayed as much as possible. This procedure is fully homogeneous and coherent, insofar as it consists in developing continuously the predictive formalism until the stage where a probabilistic prediction is required, rather than mixing up continuous predictive elements with unwarranted discontinuous descriptive stories. In Schrödinger's terms, "One must, to repeat this, hold on to the wave aspect throughout"[15]. Mott also recommended this attitude when he wrote that, "Until this final [probabilistic] interpretation is made, no mention should be made of the α-ray being a particle at all".

As for Darwin, he took this delay as the pivotal concept of his interpretation of quantum mechanics, and as a sort of *leitmotiv:* I take it that the infinitive "to postpone" is the key-wording in his paper. Darwin's major aim was to show "how it is possible to postpone speaking of particles", for according to him, "there is no need to invoke particle-like properties in the unobserved parts of any occurrence, since the wave function ψ will give all the necessary effects". Each entangled wave function can be read as a disjunction of conditional statements, relating one ionization to a series of other ionizations approximately located on the straight line joining the radioactive nucleus and the first ionization. While the probability of the first ionization is evenly distributed, the conditional probability of obtaining an approximately straight track following this first ionization is very high. However, here again, says Darwin, "The decision as to the actual track can be postponed until the wave reaches the uncovered part where the observations are made". Later on, Darwin went even further, in suggesting that it is only at the level of the brain that we are really compelled to stop the chain of entanglements, and that it is our *consciousness* that so to speak cuts sections of the overall wave-function when it becomes aware of the outcome of observations. He thus anticipated later controversial views of the measurement problem such as Von Neumann's, London's and Bauer's, or Wigner's. But this urge to *explain* how it is that we finally see a single track, in spite of the multiple-track structure of the relevant overall wave function, is a clear sign that Darwin was still tempted with ascribing a partly descriptive status to wave mechanics, rather than a purely predictive one. Mott avoided such speculations straightaway; he contented himself with having proved that the probability of observing two ionized atoms in the cloud chamber vanishes unless the line that joins them passes near the radioactive atom.

As I mentioned earlier, the interpretative strategy used by Heisenberg in his *Physical Principles of the Quantum Theory* was quite different. Unlike Mott and Darwin (and owing to the influence that Bohr had exerted on him), Heisenberg had no reluctance to jump from corpuscle representation to wave representation and back again whenever it appeared convenient to do so. According to him, nothing prevents one from using the corpuscular picture when the tracks in Wilson cloud chambers have to be accounted for quantum mechanically, notwithstanding the fact that a wave picture is more convenient to explain the radioactive emission. In the same spirit, he considered that including the α particle and the ionizable hydrogen atoms of the cloud chamber within the same compound system, or taking the α particle as the only system and the ionizable atoms as part of the observation device, is a matter of free choice. A cut has to be introduced somewhere between the system and the observation device, but, says Heisenberg after Bohr, the location of this cut is almost arbitrary; it only depends on pragmatic considerations. Accordingly, Heisenberg did his best to show that, in the problem of α-ray tracks, the method of successive reductions of a wave packet (in which the α particle is the system) gives exactly the same predictions as the method of entangled wave functions (in which the system includes the ionizable atoms of the cloud chamber).

Now, what are we to think about this difference between Heisenberg on the one side and Mott and Darwin on the other? Heisenberg was certainly right to point out the strict predictive equivalence between wave packet reductions and entangled wave-functions. However, the two methods are not equivalent from an intellectual standpoint. And the second one is definitely more coherent.

The method of successive wave-packet reductions is usually much simpler, for it consists in using the information afforded by each point-like observation to extract a new wave function for the α particle alone out of the compound wave function of the larger system consisting of the α particle and an ionized atom. The problem is that one then usually forgets this process; one usually forgets that successive reductions are by no means *changes* of the initial wave function, but rather *redefinitions* of it for practical purposes. As a consequence of this forgetfulness, the discontinuous evolution of the wave function is taken as a sort of *descriptive* account of the process that gives rise to the track, and this arouses spurious questions about the physical mechanism of the wave packet reduction. By contrast, the method of the entangled wave-functions has the merit of permanently maintaining a clear distinction between the predictive continuous model and the series of predicted discontinuous events. The only question which arises in it concerns not the discontinuous collapse of the wave function, but the progressive shift from a wave-like to a classical probability theory when the system encompassed within the global wave-function grows bigger and bigger. In other words, the question here is that of a progressive loss of interference terms in the probabilistic formalism. As we now know, a plausible answer to *that* question, but *not directly* to the question about wave-function collapse, has been provided by the decoherence theories.

There is another significant intellectual difference between the two attitudes. Heisenberg's insistence that corpuscularian categories are good enough to explain tracks in cloud chambers may be taken as an incentive to forget in the long term Bohr's cogent statement according to which the corpuscular picture is *relative* to a certain class of experimental situations, or to a certain mode of analysis of experiments, and that one should therefore avoid taking it at ontological face value. By contrast, holding on to the wave-mechanical model until the very moment when the probability of a series of ionizations is to be calculated enables one to *bypass* completely the corpuscularian categories, and thus to avoid taking them too seriously. A very strong expression of this alternative way of seeing things was provided by Schrödinger: "it is better to regard a particle not as a permanent entity but as an instantaneous event. Sometimes these events form chains that give the illusion of permanent beings"[16]. Thus, according to Schrödinger, the cloud-chamber phenomenon of track-like series of water droplets is not to be construed as the sign of a real trajectory, but rather as the departure point of the *illusion* of seeing corpuscular spatio-temporal continuants; a type of illusion which bears some analogy to the one well-known in psychology under the name "Phi-effect". Let me remind you that in the "Phi-effect" experiment, two distant spots of light are successively switched on, and, if the succession is rapid enough, the *two static* spots are seen as a *single moving* object with permanent identity over time.

To conclude, I will briefly evoke a few recent reflections on Mott's wave-mechanical account of α-ray tracks. Some of them manifest a partial misunderstanding of the aim of Mott's paper, but there also are some cases of genuine revival of Mott's original intention. In the first category, there are two papers published respectively in 1993 and 1994 in the *Physical Review* by A.A. Broyles and D. Marolf[17]. These authors both tend to explain the track phenomenon in terms of wave mechanics, or pure unitary quantum mechanics. And they both acknowledge Mott's pioneering work in this field. But they think that something is missing in this work. Broyles, for instance, claims that in Mott's approach, "The questions still remain how a wave function with a broad extent collapses to a track, and what causes the probability distribution of observed tracks to be proportional to the magnitude squared of the incident wave". But, as we noted above, Mott did not fail to answer these questions; he did not treat them as questions at all. The first question, about the collapse, was made irrelevant by his choice to consider an entangled wave function and by his using this overall wave function as a *predictive* device (for calculating the *probabilities* of the events), rather than as a *descriptive* device (for giving a counterpart of the discontinuous events themselves). The second question, about the reason why there is a relationship between ψ-waves and probabilities, did not even arise, for in Mott's approach there was not a *real wave* on the one side, and a probability assessment that had to be connected with it on the other side; there was a wave *function* whose only role is to afford probabilities. These recent papers are thus in some way closer to Darwin, who was still trying to *explain* the selection of one term among many within the overall wave function, than to Mott who tended to dissolve the problem by a thoroughly probabilistic interpretation of this wave function.

But, as I mentioned, there also are some contemporary authors who caught the spirit of Mott's paper, and who tried to push it to its ultimate consequences.

John Bell for instance noticed, in his celebrated paper "Quantum mechanics for cosmologists", that Mott's paper is both essential for a proper understanding of the problem of measurement, and usually misunderstood. As he writes, "(...) many students are left to rediscover for themselves [Mott's ideas]. When they do so it is often with a sense of revelation"[18]. Then, Bell proceeds with a careful analysis of Mott's treatment of the α-ray track problem, and he concludes with some characteristic considerations about the location of Heisenberg's cut between the quantum and the classical domain being only valid *"for all practical purposes"* .

Another author who organized the whole of his interpretation of quantum mechanics by due reference to Mott's account of α-ray tracks is Hermann Wimmel, in his book *Quantum Physics and Observed Reality*[19]. Wimmel claims that wave-functions should by no means be construed as possible elements of description of the observed discontinuous facts, but only as a generalized instrument of prediction. He then emphasizes the dissociation between the domain of the wave functions and the domain of experimental facts, in a way that is akin to van Fraassen's modal interpretation of quantum mechanics. The two domains of ψ-function evolution and value-ascriptions are carefully distinguished throughout Wimmel's treatment; their relations are construed in terms of parallelism rather than in terms of periodic intervention of one of them into the other by means of successive wave-packet reductions. Thus, in order to predict the probability of *sequences of events* arising from the interaction of an object and several parts of an apparatus, Wimmel uses exactly the same strategy as Mott; and he refers explicitly to Mott as a forerunner of the view he advocates. Wimmel first considers the global wave function of the object *and* the various parts of the apparatus, then he develops this wave function according to a composite observable basis, and finally he calculates the joint probability of the whole sequence by applying the generalized Born rule to a given coefficient of the resulting entangled linear superposition. Just as in Mott's paper, the evolution of the wave function is increasingly holistic but continuous, and the only stochastic element comes in when the *final connection* between the entangled wave function and the experimental facts is at stake. At no point *between* the preparation of the experiment and the final outcome (here, a track) does one have to assume that quantum jumps or localized particle collisions occur at all.

Hence the title of this paper: all of the phenomena of radioactivity, from the emission of radiation to the detection of tracks, can be predicted with maximal consistency if one does *not* make any reference to particles.

NOTES

1 Some of the ideas presented in this paper were developed in M. Bitbol, *Schrödinger's Philosophy of Quantum Mechanics,* Dordrecht : Kluwer, 1996 ; M. Bitbol, *Mécanique quantique : une introduction philosophique,* Paris : Flammarion, 1996

2 N.F. Mott, "The wave mechanics of α-ray tracks", Proceedings of the Royal Society, London, A126, 79-84, 1929; reprinted in: J.A. Wheeler & W.H. Zurek, *Quantum theory and measurement,* Princeton University Press, 1983. Biographical information about Sir Nevill Mott can be found in: N.F. Mott, *A life in science,* Taylor & Francis, 1986. Let me just mention a few relevant events: Mott was born on September 30, 1905. He was trained as an undergraduate at St John's College (Cambridge) and as a graduate in the Cavendish Laboratory. He then spent a few months in Copenhagen (discussing with Bohr and Gamow) during the autumn of 1928, and also in Göttingen during the spring of 1929.

3 W. Heisenberg, *Physics and Beyond,* Harper & Row, 1969, chapter VI

4 E. Schrödinger, *Collected Papers on Wave Mechanics,* Blackie & sons, 1928, p. 1

5 F. A. Muller, "The equivalence myth of quantum mechanics", *Studies in the history and philosophy of modern physics,* 28B, 35-62, 1997

6 W. Heisenberg, *The Physical Principles of the Quantum Theory,* University of Chicago Press, 1930, chapter V

7 C.G. Darwin, "A collision problem in the wave mechanics", Proceedings of the Royal society, London, A124, 375-394, 1929. C.G. Darwin was the granson of the great Charles Robert Darwin; he was a member of Rutherford's Manchester team, where Bohr was working when he presented his celebrated model in 1913. See A. Pais, *Niels Bohr's times,* Oxford University Press, 1991

8 G. Gamow, "Zur Quantentheorie des Atomkernes", *Zeitschrift für Physik,* 51, 204-212, 1928; R.W. Gurney & E.U. Condon, "Wave mechanics and radioactive disintegration", *Nature,* 122, 439, 1928

9 C.G. Darwin, "A collision problem in the wave mechanics", loc. cit.

10 Ibid.

11 N.F. Mott, "The wave mechanics of α-ray tracks", loc. cit.

12 W. Heisenberg, "Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik", *Zeitschrift für Physik,* 43, 172-198, 1927

13 E. Schrödinger, "Discussion of probability relations between separated systems", *Proc. Cambridge Philos. Soc.,* 31, 555-563, 1935

14 E. Schrödinger, *The Interpretation of Quantum Mechanics,* edited and with introduction by M. Bitbol, Ox Bow Press, 1995, p. 50-53. See also: M. Bitbol, *Schrödinger's Philosophy of Quantum Mechanics,* Kluwer, 1996

15 E. Schrödinger, “The meaning of wave mechanics”, in: A. George (ed.), *Louis de Broglie physicien et penseur,* Albin Michel, 1953.
16 E. Schrödinger, *Science and Humanism,* Cambridge University Press, 1951, p. 17
17 A.A. Broyles, “Wave mechanics of particle detectors”, *Physical Review* A48, 1055-1065, 1993; D. Marolf, “Models of particle detection in regions of space-time”, *Physical Review* A50, 939-946, 1994
18 J.S. Bell, *Speakable and unspeakable in quantum mechanics,* Cambridge University Press, 1987, p. 119
19 H. Wimmel, *Quantum physics and observed reality,* World Scientific, 1992