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Schrödinger's interpretation of quantum mechanics and the relevance of Bohr's experimental critique

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Abstract

E. Schrödinger's ideas on interpreting quantum mechanics have been recently re-examined by historians and revived by philosophers of quantum mechanics. Such recent re-evaluations have focused on Schrödinger's retention of space–time continuity and his relinquishment of the corpuscularian understanding of microphysical systems. Several of these historical re-examinations claim that Schrödinger refrained from pursuing his 1926 wave-mechanical interpretation of quantum mechanics under pressure from the Copenhagen and Göttingen physicists, who misinterpreted his ideas in their dogmatic pursuit of the complementarity doctrine and the principle of uncertainty. My analysis points to very different reasons for Schrödinger's decision and, accordingly, to a rather different understanding of the dialogue between Schrödinger and N. Bohr, who refuted Schrödinger's arguments.

Bohr's critique of Schrödinger's arguments predominantly focused on the results of experiments on the scattering of electrons performed by Bothe and Geiger, and by Compton and Simon. Although he shared Schrödinger's rejection of full-blown classical entities, Bohr argued that these results demonstrated the corpuscular nature of atomic interactions. I argue that it was Schrödinger's agreement with Bohr's critique, not the dogmatic pressure, which led him to give up pursuing his interpretation for 7 yr. Bohr's critique reflected his deep understanding of Schrödinger's ideas and motivated, at least in part, his own pursuit of his complementarity principle.

However, in 1935 Schrödinger revived and reformulated the wave-mechanical interpretation. The revival reflected N. F. Mott's novel wave-mechanical treatment of particle-like properties. R. Shankland's experiment, which demonstrated an apparent conflict with the results of Bothe–Geiger and Compton–Simon, may have been additional motivation for the revival. Subsequent measurements have proven the original experimental results accurate, and I argue that

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Schrödinger may have perceived even the reformulated wave-mechanical approach as too tenuous in light of Bohr's critique.

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1. Introduction

Schrödinger's early 1926 interpretation of quantum mechanics—which interpreted wave function directly in terms of an ontology of waves—and the ideas concerning quantum entanglements that he developed in 1930s, have recently undergone a historical and philosophical re-evaluation, and largely as a result of this re-evaluation, a small renaissance. At the center of this re-evaluation is a harsh judgment of the critiques offered by the Copenhagen and Göttingen Schools of Schrödinger's interpretation of quanta. (e.g., Beller, 1993, 1994, 1999). For example, Beller (1997) states that "[t]he Copenhagen orthodoxy trivialized Schrödinger's objections and understated his prominent insights. Göttingen–Copenhagen physicists presented Schrödinger as a reactionary, hopelessly trapped in the deterministic, naively realistic modes of thought of classical physics" (p. 422). She further argues, "Schrödinger's defense of the comprehensive wave-ontology," led to the "ongoing Copenhagen caricature of Schrödinger as a conservative, simple-minded classical realist, unwilling and unable to 'sacrifice traditional concepts and accept new ones'" (p. 422).

It cannot be denied that Schrödinger's attempt to reformulate his initial interpretation, which took the wave function at its "face value," was viewed unfavorably by Bohr, Heisenberg and others. He developed a wave-mechanical alternative that emphasized electric charge density as an accurate description of atomic processes, but this development was stalled in 1927, partly, as Bitbol (1995) notes, because of "the pressure of strong criticism from the Göttingen–Copenhagen physicists, and their elaboration of a full-blown synthetic apprehension of quantum mechanics whose two cornerstones were Heisenberg's 'uncertainty principle' and Bohr's complementarity principle" (p. 2). And Beller (1999) writes that although "Schrödinger's methods did indeed win an overwhelming victory… the interpretation that most physicists seemed to accept was the one given by Schrödinger's opponents" (p. 38).

In his mid-1930s revival of the wave-mechanical interpretation, in a series of three papers, Schrödinger (1935a, b, 1936) argued for the wave-mechanical resolution of the Einstein–Podolsky–Rosen paradox by focusing on the phenomenon of quantum entanglements, but by that time, the Copenhagen–Göttingen "orthodoxy" had been firmly established: "The aging Schrödinger witnessed a remarkable state of affairs: the universal use of his theory coupled with an almost total rejection of his interpretation. Schrödinger's methods proved indispensable. His philosophy did not" (Beller, 1999, p. 39).

For a long time, philosophers and historians of quantum theory understood the question of whether or not quantum systems are deterministic as central to the debate between Schrödinger and his opponents. Although this may be an important aspect of this complex debate, its central issue was rather the question of whether the principle of continuity could be retained in interpreting quantum theory, as Y. Ben-Menahem convincingly argued in

(1989). Approaching the debate from this perspective, Bitbol (1995, 1996)[1] has presented Schrödinger's arguments for adhering to the wave-mechanical framework and the principle of continuity in interpreting quantum phenomena as largely misunderstood in his time.

Among the many overlapping features of Schrödinger's 1926 interpretation and his subsequent ideas, the most challenging may perhaps be his insistence on *discarding the corpuscular view* as an adequate framework for understanding quantum phenomena. In both the early and late period of his interpretation, Schrödinger's answer to whether it is "coherent to keep speaking of 'particles' if they have nothing like a trajectory?" was a "definite no" (Bitbol, 1995, p. 7). As a result, he indicated that relinquishing trajectories should free physics from "corpuscular talk", given that "[c]lassical particles are individualized by the position they occupy in ordinary space at each point in time, namely by their trajectory, whereas the Ψ-waves are individualized by their form in configuration space" (Bitbol, 1995, p. 112). Thus, "[i]n agreement with Schrödinger's decision to dismiss any concession to the ontology of localized bodies, the experimental discontinuities could not reflect any corpuscular aspect of the microscopic processes; they had to arise from a peculiar feature of the interaction between the (wave-like) system and the (wave-like) apparatus" (Bitbol, 1995, p. 13).

It is important to emphasize that this *"hyper-revolutionary attitude"* of Schrödinger's, as Bitbol (1995, p. 5) labels it, exceeds the domain of pure historical interest, as a very similar attitude has been embraced by some influential interpretations of quantum mechanics,[2] and as some recent philosophical ideas on the nature of the physical world (Bitbol, 1996; French, 1989; Humphreys, 1997; Teller, 1986) accord with Schrödinger's "profound and everlasting … insights into the nature of *quantum holism and entanglement*" (Beller, 1997, p. 422).

Whether the alleged pressure of Göttingen and Copenhagen physicists (predominantly Bohr and Heisenberg) forced Schrödinger to refrain from arguing his wave-mechanical interpretation in public is of socio-historical interest. But my first goal in this paper is to point out the reasons, quite different than those that a socio-historical analysis might reveal, that led to Schrödinger's abandonment of his early arguments for the wave-mechanical interpretation and for the relinquishment of the corpuscular aspect of quantum phenomena. These reasons derive from the scattering experiments performed by Bothe and Geiger (1926) and by Compton and Simon (1925), and cannot be reduced to the hegemonic pressure of the Copenhagen and Göttingen physicists. Niels Bohr advanced a critique of Schrödinger's views based on these experiments and presented it to him in 1926, and in light of this critique, Schrödinger explicitly conceded the inadequacy of his interpretation. However, Bohr's critique does not caricature Schrödinger's ideas, as has been claimed. Rather, it reflects Bohr's profound interest in, and sophisticated understanding of them. If one overlooks that the debate revolved around the exact meaning of the scattering experiments, it is tempting to conclude that Bohr misunderstood and distorted Schrödinger's account in order to promote his dogma, and that Schrödinger gave up pursuing it irrespective of, or at best secondarily due to, the content of the critique. I will also argue that the differences between these two interpretations are subtler than usually

[1] See also (Ben-Menahem, 1989, 1992) and (Darrigol, 1992).

[2] See (Bitbol, 1996) for fundamental similarities between Schrödinger's, the so-called Many Worlds, and other interpretations.

thought, but that their authors nevertheless perceived the differences as essential. Both Bohr and Schrödinger dismissed the full-blown light quanta, but unlike Schrödinger, and led by the experimental discontinuities, Bohr insisted on retaining the corpuscular aspect of localized micro-interactions.

My second goal is to examine whether and to what extent Bohr's experimental critique may have been a constraining factor in Schrödinger's intention to pursue a revived and reformulated version of his wave mechanical interpretation of the 1930s. This version emphasized the importance of the understanding of quantum states in terms of quantum entanglements, and retained the attitude that no concession should be made to the corpuscular view of quantum phenomena.

Finally, throughout the text I will point out some of the key strengths and weaknesses of the approaches of Bohr and Schrödinger, with respect to the particles/waves dilemma. The views propounded by Bohr, and those propounded by Schrödinger, in both early and later periods, have been driven by different approaches to the problem. Bohr's view was largely drawn from a careful assessment of relevant experimental results, whereas Schrödinger's was determined by theoretical considerations, most importantly by his commitment to the principle of space–time continuity. In the 1920s and likely in the 1930s, however, Schrödinger recognized in the scattering experiments' results the limitations of his hope to realize his theoretical goals. But as my main concern in this paper will be with Schrödinger's view and Bohr's critique of it, I will only briefly touch on the principle of complementarity that arose from the aforementioned experimental background. Nor I will discuss the details of Bohr's critique of Göttingen School (i.e., his defense of wave mechanics as indispensable in a complete account of quanta).

2. Why Schrödinger gave up pursuing his 1926 interpretation: Bohr's experimental critique and the relevance of the Bothe–Geiger and Compton–Simon experiments with electron scattering

Bohr, in his groundbreaking 1913 paper, successfully applied Planck's quantum of action to Rutherford's picture of the atom as a classical system consisting of a nucleus at the center of elliptically orbiting electrons. The classical theory of radiation (Maxwell) allowed for all possible values of electron orbits; i.e., it allowed the possibility of the continuous dissipation of the energy of the rotating electron, the continuous increase of the frequency of the electron's revolution and the continuous decrease of the length of orbit. Contrary to these classical predictions, experiments with spectra demonstrated that atoms are fairly stable systems of a *finite size*, which radiate light in *discrete packages*. Accommodating this fact, Bohr postulated the existence of stationary orbital states. Electrons may occupy only certain, viz., stationary, orbital states, thus providing for the stability of the system.

It was this abrupt transformation, this space–time discontinuity, which the electron, as an essentially classical particle in a stationary state, undergoes when the emission or absorption take place, that Schrödinger found unsatisfying in Bohr's account. Schrödinger preferred that Bohr's transitions be formulated in such a fashion as to preserve the space–time *continuity* of the process. He therefore believed that a fundamental reformulation of Bohr's account was necessary, "for we cannot really alter our manner of thinking in space and time, and what we cannot comprehend within it we cannot understand at all. There *are* such things—but I do not believe that atomic structure is one of them'' (1926b, p. 27).

In a series of four papers published in 1926, Schrödinger introduced characteristic frequencies (E/h) as the basic properties of interacting atomic systems, where the dynamics of atomic interactions is explained as a *resonance* phenomenon that does not defy space–time continuity:

> It is hardly necessary to emphasize how much more congenial it would be to imagine that at a quantum transition the energy changes over from one vibration to another, than to think of a jumping electron. (Schrödinger, 1926a, p. 10)

This enabled Schrödinger to describe two interacting physical systems in terms of the interaction of two different resultant frequencies: namely that it is the normal modes of the systems' vibrations that constitute the energy exchange and accordingly account for the ''transitions.'' The difference between the energies of two atomic states, explained by Bohr in terms of the electron's quantum ''energetic'' jumps, results instead from the exchange of energies between two vibrating systems, characterized by appropriate modes of vibration. Such wave-mechanical processes do not violate space–time continuity.

Rather than following a classical mechanical method, ascribing n particles to every point in q-space,[3] Schrödinger argued, each ''particle'' must be attributed a wave function. He clarified this with the analogy with the failure of geometrical optics, where any attempt to trace the incoming ray of light in the neighborhood of the diffraction patch is meaningless (Schrödinger, 1926b, p. 26). Schrödinger argued that, in dealing with very small wavelengths, the classical mechanical equations describing the underlying mechanics of the behavior of particles in the electromagnetic field become as otiose in accounting for the true nature of the micro-physical world, as the optics of rays does in explaining the phenomena of diffraction. Hence, ''we *must* treat the matter strictly on the wave theory, i.e. we must proceed from the *wave* equation and not from the fundamental equations of mechanics, in order to form a picture of the manifold of the possible processes'' (p. 25). Similarly, there is a whole range of ''paths'' stretching in all directions, within the classical ''path'' of the $3n$ continuum. In effect, there is no exact point of phase agreement to which one could point, and it is this phase agreement between the waves of the group that determines a location of a particle in q-space.[4] Schrödinger's conclusion ''that we can never assert that the electron at a definite instant is to be found on *any definite one* of the quantum paths, specialized by the quantum conditions'' (p. 26) led him to suggest an explanation of quantum phenomena in terms of *continuous wave interactions* which required invoking *the manifold of particle paths*, which can be examined by analyzing the properties of the wave function. He proposed that ''all these assertions systematically contribute to relinquishing the ideas of 'place of the electron' and 'path of the electron''' in q-space (p. 26).

Shortly after Schrödinger published his four 1926 papers, he met with Bohr to discuss his approach. The resulting vigorous debate ended with Schrödinger's defeat, mental

[3]The q-space or configurational space is a multidimensional space, each dimension represented by a property characterizing the system.

[4]Although Schrödinger admitted (1926a, p. 9) that de Broglie's idea (de Broglie, 1924, 1925) was a major inspiration for his own account, *the whole numbers* occurring in de Broglie's expression for the phase-wave were an unacceptable compromise with Bohr's idea.

exhaustion and finally illness (Bohr, 1984, Vol. 5, pp. 10–11). This was not a defeat of his dream to introduce wave mechanics as a general account of micro-physical systems, thereby preserving the principle of continuity, however. Rather, it was a defeat of his arguments that aimed to justify the dream. Shortly after his return to Berlin, on 23 October 1926, he wrote Bohr the following:

> It is possible that the stubbornness, with which in our dialogues I continued to adhere to my 'wishes' for a physics of the future,[5] in the end may have left you with the impression that the general and specific objections that you raised against my views had not made any real impression on me. That is certainly not the case. In a certain sense I can say the psychological effect of these objections—*in particular the numerous specific cases in which for the present my views apparently can hardly be reconciled with experience* [italics by the author]—is probably even greater for me than for you. (Bohr, 1984, Vol. 6, p. 12)

And Bohr's general impression of the meeting was that he and Heisenberg "succeeded at least in convincing him [Schrödinger] that for the fulfilment of his hope he must be prepared to pay a cost, as regards reformulation of fundamental concepts, formidable in comparison with that hitherto contemplated by the supporters of the idea of a continuity theory of atomic phenomena" (Bohr to Fowler, 26 October 1926 in Mehra & Rechenberg, 1982, pp. 140–141).

Bohr was not unsympathetic to Schrödinger's intention to provide a view that ascribes reality to the states of the system in terms of wave mechanics. But Schrödinger mistook Bohr's skepticism regarding the wave-mechanical formalism as the exclusive grounding for the understanding of such states as an indication that Bohr sided with Heisenberg's anti-realism.[6] Yet Bohr's doubts concerning wave mechanics as the sole ground for interpreting quantum phenomena had been motivated by some of the "numerous specific cases" that he pointed out to Schrödinger during their debate, as I will argue shortly. Moreover, his doubts regarding the wave-like nature of atomic interactions were new, as he had strongly opposed Einstein's quantum corpuscular account of radiation until 1925 (Jammer, 1989, p. 191).

In order to understand why Schrödinger accepted Bohr's argument, it is necessary to determine which "specific cases" they discussed. Heisenberg's letter to Pauli reflects his critical attitude towards Schrödinger's interpretation, and hints at these "specific cases":

> Just as nice as Schrödinger is as a person, just as strange I find his physics. When you hear him, you believe yourself 26 years younger. In fact, Schrödinger throws overboard everything [that tastes] "quantum theoretical": Photoelectric effect, Franck collisions, Stern–Gerlach effect etc. Then it is not difficult to make a theory. But it just does not agree with experience. (Bohr, 1984, Vol. 6, p. 10)

Born's remarks were similar to those of Heisenberg:

> On this point I could not follow him [Schrödinger]. This was connected with the fact that my Institute and that of James Franck were housed in the same building of the

[5]The physics of the future to which Schrödinger referred, which he also mentioned in another letter dated 5 May 1928 (Bohr, 1984, Vol. 6, p.47), is a physics that seeks to overcome the notion of quantum jumps and the discontinuous nature of atomic systems.

[6]Bohr was aware of this misunderstanding. See his letter to Fowler in (Bohr, 1984, Vol. 6, p. 15). It is doubtful whether even Heisenberg himself subscribed to a radical instrumentalist reading of quantum mechanics as it is often claimed. See (Frappier, 2004) and (Bokulich, 2004).

> Göttingen University. Every experiment by Franck and his assistants on electron collisions … appeared to me as a new proof of the corpuscular nature of the electron. (Born, 1961, p. 103)

Judging by these passages, the "numerous specific cases" seem to include the Franck–Hertz and Stern–Gerlach's experiments, and the photoelectric effect.

The Franck–Hertz experiments (Franck & Hertz, 1914) with electron collisions in gases were concerned with Bohr's hypothesis about atomic stationary states and their relation to atomic spectra. The results confirmed Bohr's hypothesis that the difference in energy between the ground state and the excited state of the atom corresponds to the loss of the electron's energy. Although this was taken to be a relevant confirmation of the discontinuous nature of atomic systems, it only addressed the changes in the energy of the excited atoms, not the nature of individual micro-processes that Schrödinger treated with his wave-mechanical approach in the above-cited passages.

Otto Stern proposed the Stern–Gerlach experiment in 1921 (Stern & Gerlach, 1922a, b) as a test for Sommerfeld–Landé's theory of the magnetic-core structure of spectral lines. Stern and Gerlach initially carried out the experiments with atoms of silver, and subsequently with other elements, demonstrating that the beam of an atom splits into two beamlets in a magnetic field. The absence of any atoms at the center of a deflection pattern was taken as a confirmation of space quantization. Rather than being distributed according to the classical law of Gaussian distribution, with the maximum at the center of the pattern, the magnetic moment was spatially quantized. The results, however, defied attempts to offer a precise interpretation of the atomic structure. The experimental set-up involved a complete vacuum, thus eliminating interpretations in terms of quantized radiation, the key to addressing Schrödinger's idea. Moreover, the hypothesis involving the mechanism attributed to the atom itself produced some unsatisfactory conceptual consequences.

These two cases were certainly relevant to the debate but they did not sufficiently expose the weaknesses of Schrödinger's arguments. And although Schrödinger and Bohr must have discussed them, it was the third case, viz. photoelectric effect, which addressed most directly the waves-or-particles dilemma concerning the *individual processes*. Simply stated, photoelectric effect, which attracted much attention at the beginning of the 20th century, demonstrates that the velocity of the electrons released when light hits a metal target does not depend on the intensity but rather on the frequency of light. Einstein (1906) had explained this phenomenon by hypothesizing that the energy transfers from the incoming light-quanta (rather than the incoming wave) of radiation to the electrons in the impinged material. Following the earlier attempts by Hughes (1912) and by Richardson and Compton (1912), Millikan (1916) demonstrated the accuracy of Einstein's theory, despite his own reservations with respect to the light quantum hypothesis. (Stuewer, 1975, pp. 75–77) In fact, the pervasive attitude among physicists at the time was that Einstein's hypothesis that radiation is quantized and thus cannot take up any possible energy state was essentially unacceptable, and that it should (and would) be replaced by a more plausible theory.

A. H. Compton's experiments with interactions of radiation (X-rays) and matter (electrons) in the 1920s were part of a stream of experiments designed to improve the understanding of the photoelectric effect. His discovery of the Compton effect in 1922 had a strong impact on the debates on the nature of atomic interactions, as it offered strong experimental evidence for Einstein's quantum-corpuscular theory, and thus turned the attention of the physics community to such an approach once again, after it had been sidelined by the advances of the electromagnetic field.

Compton's hypothesis concerning the interactions among particulars was quantum-corpuscular in its treatment of each quantum of X-ray energy as a single particle hitting a free electron (1923a). He offered convincing experimental evidence to support the now well-known formula $k = (h/mc)(1 - \cos\theta)$, which relates the change of the wavelength (k) to the scattering angle (θ). J. J. Thomson's theory of X-ray scattering, widely accepted at the time (and which Compton's theory would replace), treated radiated energy as waves. According to this theory, regardless of its size, a "package" of radiated energy, even a very small package, should scatter much like a regular wave (such as a wave in the water) when it encounters a rigid obstacle (such as a cliff). As the wave spreads out across an area of the cliff's surface, water will run tumultuously all around the cliff, rather than in any one definite direction.

But there was a problem with the straightforward application of this model to X-ray scattering. The experimentalists, Compton among them, noticed that as the "size" of the packages of radiation (i.e., their wavelength) decreases, the scattering becomes much more directed. Compton's explanation of this phenomenon (1923b), which he abandoned after his 1922 experiments, was "that this reduced scattering of the very short wave-length X-rays might be the result of interference between the rays scattered by different parts of the electron, if the electron's diameter is comparable to the wave-length of the radiation" (p. 484). In order for any significant interference among the rays constituting the wave to take place, the wave cannot be much smaller than the obstacle, as the scattering angles of neighbouring rays in the wave will be negligibly different and will "miss each other" upon the impact. But if the size condition is satisfied, then, to use the analogy with the water waves again, different parts of the wave that hit different sides of the cliff might interfere with each other and a much more directed outgoing stream of water might be observed. But as Compton noted, "recent experiments have shown that the size of the electron which thus must be assumed increases with the wave-length of the X-rays employed, and the conception of an electron whose size varies with the wave-length of the incident rays is difficult to defend" (p. 484).

Compton turned to a quantum-corpuscular hypothesis as an acceptable alternative: "From the point of view of the quantum theory, we may suppose that any particular quantum of X-rays is not scattered by all the electrons in the radiator, but spends all of its energy upon some particular electron. This electron will in turn scatter the ray in some definite direction, at an angle with the incident beam" (p. 484). Although radiation could be treated in terms of waves at some approximation, the experiments demonstrated that it was more likely that light-quanta, not waves, constituted its fabric. This was the beginning of the fundamental change in Compton's thinking, a point of view that would soon pervade the community of physicists working on quantum mechanics. As an offshoot of his arguments, Compton predicted the existence of recoil electrons scattered with a momentum equal to the change in the momentum of the X-ray. Although they initially served only as a useful hypothesis, Wilson (1923a, b) confirmed the existence of recoil electrons by tracking them down in his newly invented cloud chamber. Compton finally derived formulas for the energy of the recoil electron and for the difference between the angles of scattering and the angle of recoil, and concluded that the principle of conservation of momentum, analogous to that in ordinary corpuscular interactions, was applicable to the X-ray scattering.

Bohr was one of many physicists who found Einstein's quantum-corpuscular theory unacceptable. The overwhelming consensus was that if the nature of radiation was successfully explained in wave-theoretical terms then it was unacceptable that the conservation laws reflect what was essentially its quantum-corpuscular structure. And it was unclear that the radiation could be anything but spreading of the waves, a concept with which Einstein's quantum-corpuscular hypothesis could not come to terms.[7] The question was, then, what kind of waves could explain the results of Compton's experiments. Accordingly, together with Kramers and Slater, Bohr (1924a, b) proposed a theory of radiation in 1924, its underlying idea being that the radiation is emitted as probabilistic waves. Therefore, according to the Bohr–Kramers–Slater theory (BKS), an atom occupying a certain stationary state *communicates continually* with other atoms by means of a *virtual field*, a peculiar space–time mechanism equivalent to the field originating from the classical harmonic oscillators. Each stationary state corresponds to a virtual radiation field comprising as many monochromatic spherical waves as there are stationary states. Furthermore, the transition from one stationary state to another is associated with the probability of the frequencies of the virtual field, and not with the exact values, implying that *the laws of conservation of energy and momentum in atomic interactions can hold in the mean, but do not apply to individual micro-interactions.* BKS proponents claimed that "such an interpretation seems unavoidable in order to account for the effects observed, the description of which involves the wave-concept of radiation in an essential way" (Bohr, Kramers, & Slater, 1924b, p. 800). Regarding the Compton scattering of radiation by free electrons, and contrary to the corpuscular view, the BKS theory predicted that "the scattering of the radiation by the electrons is … considered as a continuous phenomenon to which each of the illuminated electrons contributes through the emission of coherent secondary wavelets. … the illuminated electron possesses a certain probability of *taking in unit time a finite amount of momentum in any given direction*" (Bohr et al., 1924b, p. 800). In other words, "a statistical conservation of momentum is secured in a way quite analogous to the statistical conservation of energy in the phenomena of absorption of light" (Bohr et al., 1924b, p. 800), and therefore, the inapplicability of the laws of conservation in individual processes eliminates the need for the quantum-corpuscular hypothesis.

Schrödinger (1924) was among the physicists who reacted very positively to BKS, praising a *commitment to continuity* that found its expression in communication between atoms in terms of virtual field. He also lauded its "*fundamental violation of the laws of conservation of energy and momentum in each radiation process*"—an aspect of the theory that he himself would bring to its final stage 2 yr later with the abandonment of the "path of the particle." Commenting on the notion of communicability in the BKS as early as 1924, Schrödinger hinted at the philosophical grounds for his view by suggesting that a holistic approach was an appropriate framework for the development of a general theory of quantum phenomena: "Thus one can also say: a certain stability in the world order *sub*

[7]See interview with Heisenberg on 15 February 1963 (AHQP). Bohr sarcastically responded to Einstein's hypothesis by pointing out that the telegram in which Einstein would inform him of an irrevocable proof of the existence of light-quanta, "could only reach me by radio on account of the waves which are there."

specie aeternitatis can only exist through the interrelationship of each individual system with the rest of the whole world."

Long before meeting Schrödinger in 1926, however, Bohr had started to doubt the validity of BKS. These doubts had to do with his renewed interest in Carl Ramsauer's (1921) experiment with the so-called cross-section of the atoms of gases, the results of which seemed to contradict the kinetic theory of gases. But it was the experiments of Bothe and Geiger, and later Compton and Simon, that finally led him to abandon the theory. In the postscript of a letter to R. H. Fowler in which he expresses the aforementioned doubts concerning Ramsauer's work, Bohr notes the arrival of Geiger's letter commenting on the experiment he has performed with Bothe:

> Just in this moment I have received a letter from Geiger, in which he tells, that his experiment has given strong evidence for the existence of a coupling in the case of Compton-Effect. It seems therefore, that there is nothing else to do than to give our revolutionary efforts [the BKS theory] as honourable a funeral as possible. (Bohr, 1984, Vol. 5, p. 81)

Compton's theory of particle interactions, which provided the framework for his 1922 experiments with X-ray scattering, depended on the hypothesis "that the quanta of radiation are received from definite directions and are *scattered in definite directions*" (Compton, 1922a). What the experiments undeniably demonstrated was that the energy and the momentum of the emitted radiation was conserved upon scattering, but one could not tell, based on these experiments alone, whether the conservation principle was applicable to the in-between stages of the process (i.e., to the individual micro-interactions). The aforementioned hypothesis could have turned out to be redundant in explaining the in-between stages, and hold only on the whole, if the conservation of the momentum and energy was a result of wave interactions. The BKS avoided this hypothesis by introducing the statistical nature of energy exchange, which directly inspired the experiments performed by Compton and Simon in 1924 and by Bothe and Geiger in 1925. Even Compton (1922b) hoped that such a hypothesis would be avoidable, and that the matter would turn out to be "a continuous series of waves of matter" (p. 716).

Shortly before he published the results of his scattering experiments, Compton (1923c) released the results of the experiments with the internal reflection of X-rays; according to these results, X-rays reflected as classical waves. This compelled Compton (1923b) to speculate about a way to reconcile the wave-like nature of X-rays with the findings of the scattering experiments. The idea was that, although the existence of the secondary quantum scattered by the electron, the momentum and energy of which confirmed the conservation laws, was unquestionable, the scattering process could be still regarded as a wave process, where the incident quantum "spreads" over a number of electrons, distributing its momentum and energy. This, in turn, results in interference, which finally produces the secondary quantum (p. 502). Compton notes, "For there would seem to be no possibility of refraction… unless the ray can spend a part of its energy in setting in vibration some of the electrons over which it passes so as to give rise to a secondary ray which will combine with the primary train" (Compton, 1923c, p. 1130).

Darwin (1923, p. 25) shared the expectation that the conservation laws do not hold in the individual micro-interactions, arguing that if the scattered radiation were emitted in the form of spherical waves in the Compton effect, then there would have to be a number of intermediate interactions resulting in such waves.

Along similar lines, Schrödinger (1926d) developed a wave-mechanical treatment of radiation scattering (i.e., "energy dispersion") in his fourth 1926 paper. He developed an expression for the waves with variable frequencies (i.e. the states with variable energy) in order to treat the scattering phenomena. In this view, the momentum of the secondary wave could be treated as a result of the scattering of the incident wave. The density integral was represented by the integral value over all the coordinates of the system, implying that "[t]he resultant density of charge at any point of space is then represented by the *sum* of such integrals taken over all the particles" (p. 109).

Thus, the diverse wave-theoretical approaches of Schrödinger, Darwin, Compton and Bohr, which were devised to accommodate the results of Compton's scattering experiments, all left out Einstein's quantum-corpuscular hypothesis in individual processes. Each physicist suggested an account of either classical or probabilistic waves, or a combination of the two, which he expected the experimental testing of individual micro-interactions to confirm. None, including Compton himself, expected a serious challenge to the wave-theoretical approach or a dramatic change in their thinking about the foundations of quantum mechanics.

One way to tackle the "quanta or waves" dilemma was to count recoil electrons. If the wave hypothesis were correct, and if the incident quantum spread along the number of the electrons, then, first, an incident quantum may eject a number of electrons, and second, a wide statistical distribution of the direction of recoil electrons would be detected, as they bounce off in all directions. This was in agreement with the BKS, but contradicted the result of Debye's formula (1923) based on the corpuscular hypothesis, for the angle of scattering of the recoil electron (Φ) and the angle of the resulting quantum (θ): $\tan \Phi = -1/((1+\alpha)\tan(\theta/2))$ (Stewer, 1975, p. 236). Debye's result predicted that *the recoil electron always scatters forward* with respect to the incident quantum, precisely as it is hit by a regular particle, and that the secondary quantum scatters in all directions. The formula determines a definite relationship between the angles of scattering of the resulting quantum and the recoil electron, and demonstrates that the electrons hit by the incident quanta are always the ones that bounce off (i.e. recoil electrons), and that no other electrons seem to be hit by the incident quantum.

The experiments of Bothe and Geiger (1926) with electron coincidence techniques probed the statistical hypothesis of the BKS theory. Their experimental apparatus consisted of two Geiger counters located opposite to each other; the incident radiation (X-rays) was directed in between them. The idea was that counters, one counting scattered photons (recorded by platinum folium) and the other counting recoil electrons (that ionized molecules of water), could detect simultaneous occurrences of the "splitting" of radiation into a recoil electron and a scattered (secondary) quantum. If the number of these occurrences were not co-incident, this would predict that individual interactions do not conserve energy and momentum, even if they do on average. However, the results unexpectedly demonstrated the opposite: the co-incidence counters (Geiger counters) were extremely accurate, and this disconfirmed the probabilistic wave treatment of energy and momentum in BKS. The conservation laws must hold for every individual interaction, and the authors concluded that the result of their experiment was "incompatible with Bohr's interpretation of the Compton effect."

Although aware that the BKS was starting to crumble, Bohr (1984, Vol. 5, p. 82) exercised caution. In a letter to Born dated 1 January 1925, (AHQP) he warned that one should not jump to conclusions about the corpuscular nature of radiation before it was determined whether the coincidences between quanta and recoil electrons appear only in cases when the counters are pointing in the direction of the incoming recoil electrons and the scattered quanta. In other words, the phenomenon of coincidences could result from interference that occurs only under these special circumstances (i.e., when the angle of scattering is 90°). Further tests would answer whether there was a *definite* relation between the angles of recoil electrons and secondary quanta in accordance with Debye's formula: the Compton–Simon experiments aimed to deliver the evidence of this.

Wilson (1923a), the inventor of the cloud chamber, had earlier realised that the matter could be addressed experimentally by his invention. He noticed that in some of his early experiments, several of the tracks left in the vapour by particles produced in the interactions, corresponded to the description of the recoil electrons predicted by Compton. Compton (1923d) analyzed the results of these and other related experiments and concluded that they were "insufficient to decide without ambiguity whether a quantum of radiation scattered by an electron is emitted in one direction only or with a continuous wave-front." Using Wilson's cloud expansion method, Compton and Simon (1925) designed an experiment that could address the issue by focussing on the recoil electrons. They photographed the tracks of scattered X-rays filtered through copper. Theoretically speaking, a track that each scattered quantum of radiation leaves in the vapour of the cloud chamber should correspond to a track of a single recoil electron, if the conservation laws hold for every individual processes and not only on average. They were testing X-rays ranging from 0.7 to 0.13Å, and discovered that the ratio of the number of two types of tracks, longer (*P*) and shorter (*R*) ones, varies with the varying wavelength from 0.10 to 0.72, and that the ratio of the X-ray energy of recoil and photoelectrically absorbed energy varies from 0.27 to 0.32. In other words, similarly to Bothe and Geiger, they concluded that the results were satisfyingly close to the idea of one *R* track (i.e., one recoil electron) being produced for each quantum of scattered radiation, and one *P* track being produced by each quantum of absorbed radiation. More importantly, they could measure the angle dependence between one electron (*R* track) and track made by scattered radiation (*P* track) and thus probe the accuracy of Debye's prediction for different angles. If Debye's prediction were accurate, the direction and the magnitude of the recoil electron momentum should be equal to the vector difference between the incident and the scattered quantum. This is precisely what they discovered, leading them to offer the following answer as to whether the energy of a scattered X-ray quantum is distributed over a wide solid angle, or whether it proceeds in a definite direction:

> [t]he results do not appear to be reconcilable with the view of statistical production of recoil and photo-electrons by Bohr, Kramers and Slater. They are, on the other hand, in direct support of the view that energy and momentum are conserved during interaction between radiation and individual electrons. (Compton & Simon, 1925, p. 299)

The tracks left in the chamber demonstrated that scattered X-rays "proceed in directed quanta of radiant energy." In other words, radiated energy in individual processes distributes in *definite directions*, as a projectile, not as a wave. The dependency of the angles taken up by the recoil electron and the secondary quantum is *fixed* in a fashion predicted by Debye's formula. Thus, only certain angles taken up by the secondary quantum "combine" with certain angles taken up by the recoil electrons. More precisely, they "combine" as if the scattered quantum "bounces off" an electron, and this very electron, and none other, recoils.

The results of the Bothe–Geiger and Compton–Simon experiments were greeted happily by Einstein, who was essentially alone in his pursuit of the quantum-corpuscular theory of light. To his satisfaction, in these experiments, the observed systems' displays of continuous particle-like properties could not be disregarded. Although these experiments had a profound impact on the fate of the BKS theory, they also determined the fate of Schrödinger's interpretation, not to mention the emergence of the complementarity principle—something often overlooked in the literature. For example, in her account of the history of this period in the development of quantum mechanics, M. Beller fails to point out the relevance of the experiments to Schrödinger's interpretation, and as I will argue, such an understanding is critical for comprehending Schrödinger's decision to refrain from pursuing his initial interpretation.

Bohr, greatly impressed by the results, finally realized the devastating consequences facing the BKS as a moderate wave account attempting to discard the corpuscular hypothesis. In a section entitled "Quantum Theory of Radiation" in his paper "Atomic Theory and Quantum Mechanics," published in a supplement to *Nature* in December 1925, Bohr stressed that, while "the constantly growing contrast between the wave theory of light … and the light-quantum theory" suggested a possible failure of the conservation laws at the atomic level, "the suggestion does not offer a satisfactory escape from the dilemma, as is shown by the experiments on the scattering of X-rays which have been undertaken recently with the beautiful methods permitting a direct observation of individual processes" (p. 848). He pointed to the results of the Compton–Simon experiments as demonstrating that "besides this pairing [between resulting quanta and recoil electrons], the connection demanded by the light-quantum theory between the direction in which the effect of the scattered radiation is observed and the direction of the velocity of the recoil electrons accompanying the scattering" (p. 848). He read the results as strong evidence for retaining the discontinuous aspect of micro-interactions: radiation interacts with the matter as classical corpuscles. This was a sufficiently good reason for Bohr to regard any attempt to invoke an exclusively wave-mechanical explanation of micro-systems that aims to reformulate singular micro-interactions in continuous terms, as deeply misguided. In essence, whereas the commitment to continuity was a line that Schrödinger struggled never to cross, Bohr was committed to the experimental situation and its results.

Bohr, to be sure, did not entirely abandon the wave-mechanical explanation of light. In a letter to Geiger dated 17 April 1925 (Bohr to Geiger, 17 April 1925, AHQP), he insisted that based on the results of the experiments with the scattering, "conclusions concerning an eventual corpuscular nature of radiation lack a satisfactory basis." Bohr became increasingly convinced that both the corpuscular picture and the wave-theoretical view must have explanatory merit. It is very likely that at some stage of the debate, he told Schrödinger that the insight delivered by the experiments on the scattering of electrons should not be diminished by the success of Schrödinger's wave-mechanical treatment of optical phenomena in his 1926 papers, published some 6 months after the Compton–Simon and Bothe–Geiger results appeared. (To Schrödinger's furious remark during their meeting about "darn quantum jumps" and his regret that he ever became involved in the matter, Bohr (1984, Vol. 6) kindly replied, "[W]e are all of us so grateful that you actually did!" (p. 10)). He believed that this treatment could be pushed quite far, but not so far as to discard the Bothe–Geiger and Compton–Simon experiments. Bohr felt that, instead of conceptualizing a general atomic theory in terms of the correspondence between wave-mechanical and classical expression, some kind of reconciliation between the two accounts was needed, given that the experimental evidence indicated that neither could apply to all quantum phenomena.

Heisenberg's note (a result of Pauli's mediation between Heisenberg's view of the uncertainty principle and Bohr's emerging complementarity approach) illustrates the crux of Bohr's strategy: "the requirement of doing justice at the same time to the different experimental facts which find expression in the corpuscle theory on the one hand and the wave theory on the other" (Bohr, 1984, Vol. 6, pp. 20–21). Bohr began thinking about the relationship between the two in terms of *complementarity*. His incorporation of the corpuscular picture in his account, however, was not a straightforward acceptance of Einstein's light quanta. Beller (1999) is right in pointing out that "Bohr did not adopt the idea of pointlike light quanta (as used in the explanation of the Compton effect), even after the Bothe–Geiger experiments" (p. 121). But, as I will argue shortly, although he abandoned the existence of localized full-blown particles, the results of the experiments on the scattering compelled him to devise an account that would acknowledge the indispensability of the "discontinuities" that they demonstrated (contrary to Schrödinger's wave-ontological approach).

Before the historic meeting with Bohr, Schrödinger was unconvinced that the experiments "buried everything that smells of classical waves" (Schrödinger to Sommerfeld, 21 July 1925 in AHQP) and reacted negatively to Bohr's hint at wave–particle duality and the abandonment of the causal law as a solution. As Wien wrote to Schrödinger, "[b]oth [Bohr and Heisenberg] are convinced that the photoelectric effect is not compatible with wave mechanics" (Mehra & Rechenberg, 1982, Vol. 6, p. 138). But Schrödinger suggested in his letter to Sommerfeld, dated 21 July 1925, that a closer look at the optical phenomena concerning the destructive interference could provide insight into the meaning of the Compton–Simon and Bothe–Geiger experiments, and this might be something "completely, completely different" than the burial of classical waves. His paper on the Compton effect (1927a) mentioned the mutual cancellation of wavelets, arguably in an effort to recover from the devastating effect of Bohr's argument.

Following the meeting, in 1927, Schrödinger published another related paper (1927b), which explicitly dealt with the conservation laws. The fact that he chose to address the issue of scattering in the Compton effect indicates the extent to which he had now realized the seriousness of the implications of the Bothe–Geiger and Compton–Simon experiments, either over the course of the meeting or within a short period afterwards. The message of Schrödinger's 1926 papers—that "we must treat the matter strictly on the wave theory" in dealing with the micro-interactions—seemed now to be an overstatement in light of the corpuscular nature of such interactions demonstrated by the Bothe–Geiger and Compton–Simon experiments. The path of the quantum interacting with the electron is a path of a classical corpuscle, not only in terms of the preservation of the magnitude of the transferred momentum, but also in terms of the definite direction of the transfer, as Compton and Simon demonstrated. One could talk of a quantum that indeed covers a definite path in these cases, contrary to Schrödinger's contention that "the true laws of quantum mechanics do not consist of definite rules for the single path." His idea of the manifold of paths of a system could not accommodate (at least in any obvious way that would not require a major reformulation of the results) the definiteness of the magnitude and the directedness of the quantum momentum in its interaction with classical matter.

Although Schrödinger explicitly conceded the failure of his own expectation to explain the relevant experimental results in wave-mechanical terms,[8] he still hoped that it was only a matter of time before Bohr's idea of complementarity, invoking discontinuity, would be replaced by the wave-mechanical account. In the same letter to Wien cited above, and commenting on Bohr's proposal that led to complementarity, Schrödinger noted, "I am quite unable to set my mind at rest with this preliminary solution. It appears to me *in general* as inapplicable as my own" (Bohr, 1984, Vol. 6, p. 13). Continuing his struggle, in the aforementioned 1927 paper on the Compton effect (1927a), Schrödinger offered a tentative general idea of how his views could agree with the new insights into the Compton effect. Specifically, he attempted to demonstrate that the alleged discontinuities could be deduced from the wave-mechanical account. Starting from de Broglie's expression, he demonstrated that a wave expression for two interacting waves could be derived from the corpuscular theory of momentum, energy, and direction change in the experiments with the scattering. This treatment, however, was limited to a stationary state. It did not indicate the source of the second interfering wave, given that the Compton effect concerned "only one electron moving in a specified way." Schrödinger ended with a somewhat desperate remark that "such simple considerations of phase as we have employed here are of course absolutely inadequate for the answering of such questions."

In a paper (1927b) that followed the one on the Compton effect, Schrödinger exercised the idea of extending the conservation laws of classical electromagnetic theory to the individual processes, and finally acknowledged that "[t]he exchange of energy and momentum between the electromagnetic field and "matter" does *not* in reality take place continuously as the expression $\partial/\partial x\sigma(T\rho\sigma + S\rho\sigma)$ [i.e., expression for the combined laws of conservation of energy and momentum] in terms of the field would lead us to believe" (p. 135).

Schrödinger's struggle, which came to a seven-year stall in 1927, was fuelled by his commitment to the principle of continuity, which, in turn, triggered grave doubts about Bohr's complementarity. Bohr's approach seemed to Schrödinger to sacrifice continuity, one of the cornerstones of physics, for a deeply unsatisfying murky syncretism. It is often assumed (as Schrödinger himself may also have assumed, given his dissatisfaction with Bohr's account) that not only did Bohr's approach of almost directly reading the interpretation of the experimental results fail to produce an acceptable ontology of quantum phenomena, but it was not even meant to be an ontological account. However, the fact that Bohr's approach more directly adhered to experimental results than did Schrödinger's does not imply that the latter was either instrumentalist or anti-realist. In his struggle to come to terms with the available experimental evidence, Bohr insisted on the possibility of the realist description of individual states, never fully adhering to Heisenberg's instrumentalism, although he considered it.

As Bohr explained to Fowler in October of 1926, his alleged "final recognition of the impossibility of ascribing a physical reality to a single stationary state" (which Schrödinger ascribed to him, and which was later identified by historians as one of the marks of the so-called Copenhagen interpretation) was "a confounding of the means and aims of

[8]For additional evidence of Schrödinger's admittance of the failure of his interpretation, other than that expressed in the aforementioned letter to Bohr, see (Mehra & Rechenberg, 1982, p. 9).

Heisenberg's theory'' (Bohr, Vol. 5, p. 15). What was at stake for Bohr was how, not whether, physical reality could be ascribed to individual states. Conflating Heisenberg's anti-realism and instrumentalism with Bohr's complementarity may be the main reason for the widespread understanding of Bohr as an anti-realist.[9] This is in part due to Bohr's habit of experimenting with different terminologies, Heisenberg's being one of them, as he struggled to best express the problems that he saw.

But equating Bohr's complementarity with the anti-realism of the Copenhagen interpretation may be an oversimplification or an inaccurate view (Hattiangadi, 1998), perhaps deliberately perpetuated by some philosophers, historians of science and even Heisenberg himself, for the purpose of promoting their own philosophical agendas (Howard, 2004). Regardless, it may be misleading to interpret Bohr's approach as leading him to give up the aim of devising a realist interpretation of quantum phenomena: instead, renouncing the principle of continuity as the fundamental ontological principle characterized a radically different starting point for accomplishing this.

The key difference between Schrödinger's and Bohr's approach then, may not be that the former insisted on the ontological, whereas the latter insisted on the epistemological grounds of interpreting quantum phenomena. Rather, it concerned their attitudes in responding to the inadequacy of postulating fully blown classical microphysical corpuscular entities that both acknowledged.[10] Unlike Schrödinger, Bohr argued that this lack should not simply imply the relinquishing of everything (classical) corpuscular from the ontology. As Bohr saw them, the experiments with the scattering clearly suggested that this would be unjustified, and that one should sacrifice continuity as the foundational principle rather than fail to acknowledge subtleties of the experimental results. This, in turn, gave rise to the complementarity approach.[11]

Due to the limitations of the present paper, I cannot go into the details of the interpretative problems of the complementarity approach here, but in interpreting it one should bear in mind that it emerged from the above-described experimental background, and that Bohr wanted it to result in an ontologically motivated conceptulization of quantum phenomena without the commitment to the principle of continuity. As a result, on the one hand, Bohr welcomed Schrödinger's wave-mechanical approach, as it reflected the holistic wave-like nature of microphysical entities, and on the other hand, he believed for experimental reasons, that this should not prompt the discarding of the corpuscular aspect of microphysical phenomena.[12]

[9]As Heisenberg himself writes: ''The difficulties in the discussion between Bohr and myself was that I wanted to start entirely from the mathematical scheme of quantum mechanics and use the Schrödinger theory perhaps as a mathematical tool sometimes, but never enter into Schrödinger's interpretation, which I couldn't believe. Bohr, however, wanted to take the interpretation in some way very serious and play with both schemes.'' (Bohr, 1984, Vol. 6, p. 15).

[10]Beller (1999, Chap.6) correctly points out their mutual agreement on this point, but she does not realize to what extent the experimental results were perceived by both Bohr and later Schrödinger as the constraint on the application of the idea of elimination of corpuscular aspect of quantum systems.

[11]Or to put it in terms of traditional terminology: if Schrödinger's wave-ontology is akin to *holism* as a general philosophical account that eliminates anything corpuscular, then Bohr's complementarity is closer to *neutral monism*, where individuality as a substance is illusory, but the individual (dynamic) characteristics observed are not.

[12]It may be that in an important sense Bohr's complementarity anticipated the idea of the entanglement between the instrument and the observed system. According to one interpretation that suggests this, Bohr thought that ''one can speak *as if* the measurement reveals a property of the object alone'' (Howard, 2004, p. 675) although

3. The 1930s revival of the wave-mechanical account

Following his encounter with Bohr and their subsequent correspondence, and sometime between 1927 and 1928, as the record of his publications indicates, Schrödinger retreated from publicly pursuing his original interpretation, and turned to other issues in quantum and wave mechanics. In 1935, however, he began to publicly question the orthodoxy, and to revive and modify his original convictions. This revival coincides with the appearance of the Einstein–Podolsky–Rosen (1935) paper, more than mere coincidence given the content of Schrödinger's three papers published in 1935 (Schrödinger 1935a,b,c) and a paper published in 1936, and the correspondence between Schrödinger and Einstein at the time (Prizbram, 1967).

Schrödinger's papers addressed the EPR paradox, arguing against the idea of the *discontinuous* collapse of the wave-function as one of the sources of the ''paradox,'' and criticizing the orthodox subjectivist interpretation of it. He suggested that, instead of separating the-observer-and-the-observed system—and treating the measurement as responsible for the collapse of the wave-function into a discrete value, and the information provided by the wave-function as being subjective—the coordinates of both the observing and the interacting systems should be treated as parts of the ''entangled'' system. In the wave-mechanical view, the interaction occurs in the space that contains both the measuring apparatus and the observed system, and thus, the measurement concerns *both elements of the entangled system*. The relation between the two is described in terms of wave mechanics as a *''catalog of expectations''* that can break at different points in different ways (i.e. following different preparations of the system), and thus occupy different states. Indeed, he argued that any state of the observed system could be measured, given the right preparation of the observing system. Therefore, the reading of a result does not have any special status—it is the observer *looking* at certain occupied states.[13] Or as Scott (1967) puts it: ''There is no discontinuous collapse of the wave for [the observed particle], but a continuous sorting out of the joint wave function into channels [of the spectrometer]'' (p. 92).

(*footnote continued*)
the objects are described in terms of the waves, because ''[r]elativizing to experimental context makes possible an unambiguous, ''objective'' account of the object as not entangled with the instrument and, in so doing, implies complementarity.'' In other words, although the microphysical objects are wave-like, the interactions between them are not random (as BKS theory suggested) but contextualizable precisely in a way that allows corpuscular-like (definite) descriptions of observed systems in the scattering experiments. It is important to emphasize, however, that what motivated complementarity principle was the realization that one must clearly distinguish between the nature of *interactions* between matter and radiation, and the nature of quantum *entities* that underlie it. This is precisely what Schrödinger's view that the wave-ontology suffices missed. The central idea that distinguished Bohr's approach from that of Schrödinger, was in effect that the observed classical causal activity (rather than interaction, which implies the existence of distinct localized interacting bodies) characterizing the momentum exchange should be postulated as an element of ontology in its own right, as a dynamic component of the field distinguished from the wave-like entities. The seeds of this distinction were planted in the BKS, and especially by Slater (1924) who insisted on ''two apparently contradictory aspects of the mechanism'' governing the radiation and matter, respectively, and the idea of a distinct ''activity of stationary states.'' Only recently this inventive view that was embraced by Bohr, that both entities and activities ''must be included in an adequate *ontic* account of mechanism,'' (Machamer, Darden, & Craver, 2000, p. 4) and that moreover ''activities are types of causes'' (p. 6) and thus ontologically independently relevant, has captured more general interest among philosophers of science in various contexts.

[13]For more on Schrödinger's views during this period see (Scott, 1967) and (Margenau, 1963).

Although reviving the wave-mechanical interpretation may have seemed (and still seems to many) a compelling way of dealing with the EPR paradox, and a promising starting point for the criticism of the orthodoxy, it is nevertheless curious that Schrödinger returned to his original ideas, given the fate of the 1926 interpretation. Did the reformulation of the original ideas provide a satisfying response to Bohr's critique?

The papers on entanglements adhered to wave-ontology, but unlike the 1926 interpretation, they emphasized the act of measurement as responsible for the seemingly discrete nature of micro-interactions. The suggestion was that the discrete and corpuscular-like aspect of the observed micro-system should be understood strictly as experimental outcome and not a property of the system. The system is properly described only as the entanglement between the various channels, some measured and some not, that are occupied by the entire wave. It seems that Schrödinger had adopted an idea similar to that suggested by Mott (1929), who argued that the introduction of discontinuities in the system should be postponed until the moment of observation. If the Schrödinger equation is solved for the entire system, up to the moment of the measurement, then the discontinuity arising in the measurement could be treated as a for-all-practical-purposes element of the system, without elevating it to an ontological status, which would imply violation of the continuity of the accounted system: "The wave mechanics unaided ought to be able to predict the possible results of any observation that we could make on a system, without invoking, until the moment at which the observation is made, the classical particle-like properties of the electrons or α-particles forming that system" (p. 79). Mott's paper addressed the kind of difficulty that Bohr had pointed out, and offered a suggestion of how "the most typically particle-like properties of matter can be derived from the wave mechanics" (Mott (1929)). We will return to this argument shortly.

Whether Schrödinger believed that the application of this approach could successfully overcome Bohr's criticism is the key question, but Shankland's (1936) experiment with scattering in the gamma-ray region of the spectrum may also have been responsible for a confident re-invoking of the wave-mechanical ideas. The results of the experiment were widely discussed at the time, and alarmed Bohr and others, as *they conflicted with the results of Bothe– Geiger and Compton– Simon*. It is not surprising that the 6 July 1936 issue of *Nature* published two letters in reaction to it, the first written by J. C. Jacobsen (Jacobsen and Bohr, 1936), expressing surprise at Shankland's results and demonstrating the results of his own repeated experiments, which he claimed agreed with those of Bothe–Geiger and Compton–Simon. A short letter from Bohr followed Jacobson's, restating his now-familiar theoretical conclusions concerning the Compton effect. But this did not resolve the issue at once. Shankland's result generated curiosity among the experimentalists, and other experiments followed. The final word came much later. Robert Hofstadter and John A. McIntyre (1950), and William G. Cross and Norman F. Ramsey (1950), performed experiments in the gamma-ray range of light. The latter stated that their results concurred "with simultaneity and the conservation of energy and momentum" (p. 929) in individual processes with far more precision than the original experiments.

Let us review how the idea of entangled states, broached in Schrödinger's 1930s approach, dealt with the experimental results. The concept of the path of a particle, even if it could be employed heuristically in interpreting experimental results, provides one with the longitudinal linkage between events (e.g., a law-like sequential occurrences of recorded positions in a definite direction) only. Schrödinger viewed this as an insufficient concept, as there is also a transversal linkage between events, such as the linkage between the "hits" on the screen in interference experiments. This is why his general argument of the 1930s was that it is better to insist on the all pervasive nature of the standing waves, which comprises both wave-like and corpuscular-like properties, since "[w]aves can do both jobs at once. Indeed, while the concept of particle path only bears longitudinal linkage, the concept of (multi-dimensional) wave synthesizes the *two* types of linkages" (Bitbol, 1995, p. 9). Following this approach, *"[t]he experimental discontinuities could not reflect any corpuscular aspect of the microscopic processes*; they had to arise from a peculiar feature of the interaction between the (wave-like) system and the (wave-like) apparatus" (Bitbol, 1995, p. 9).

The solution is akin to Mott's, who, in a manner very similar to Schrödinger's later treatment of entanglement, stated that "no mention should be made of the α-ray being a particle at all" (p. 79). Instead, a spherical wave leaves determinate tracks in the cloud chamber. Thus, it is not the case that a ray must be considered as a particle that leaves a definite track throughout the gas. What we really observe are ionized atoms, whose ionization probability is given by the wave equation. What "orthodoxy" regarded as a particle path, is really only a series of ionizations characterizing the atoms of the gas. Although atoms do not ionise at random, since ionization leaves determinate tracks, one might "consider the α-particle and the gas together as one system." As a result, "it is ionized atoms that we observe" (p. 79). This move abolishes any talk of particles prior to the actual observation.[14]

But following Bohr's approach, one could object that if we are serious about the observed system, given that the transversal linkage as an aspect of the supposed spherical wave does not become manifested in the experiments with the scattering, we need to talk only of the longitudinal linkage and a definite direction along which the incident quantum travels, and in respect to which the recoil electron (judging by the angle dependency) is ejected in the manner of an ordinary corpuscle ejection. The angle dependency between the angles taken up by the recoil electron and the secondary quantum suggests the occurrence of an interaction that takes place in a definite direction (i.e., we need to employ a law-like longitudinal linkage only), i.e., an ordinary classical causal interaction. In this case, the transversal linkage as one of "the two complementary features of wave [or phase] surfaces and of wave-normals or rays" (Schrödinger in Bitbol, 1995, p. 20) is observationally unsubstantiated. Bohr's response to the question of "Why talk of corpuscles when the waves suffice?" is that the transversal linkage does not manifest itself in the experiments with the scattering, reason enough to introduce a corpuscular aspect into the ontology. Moreover, Mott's suggestion that "interpreting the wave function should give us simply the probability that such and such an atom is ionized" (p. 79) may be presumptuous, given that, as Geiger–Bothe and Compton–Simon both demonstrated, quanta are too well orchestrated to be accidental.

Although a number of different calculations of the Compton effect, other than the original semi-classical treatment applied by Compton himself, can be successfully applied (Kidd, Ardini, & Anton, 1985; Wentzel, 1926), these attempts may seem to those sympathetic to Bohr's complementarity approach an unnecessary retraction from the firm

[14]See (Teller, 1984) and (Teller, 1995) for a similar approach.

and coherent experimental evidence.[15] The observed definite paths should translate into a substantial element of the explanation, an observationally veracious element of localized corpuscularian interactions. In Bohr's view, Schrödinger's avoidance of straightforward incorporation of the observed definite paths into the ontology (i.e. by avoiding to characterize them in corpuscularian terms) was a dangerous and forced sidelining of the experimental results for the sake of speculative ends.

The anchors of Bohr's and Schrödinger's approach to the problem were certainly different. The former was firmly grounded in the experiments, and the latter was in theoretical principles. Even so, because of the nature of the experimental evidence Schrödinger may have had an equally difficult choice in the 1930s as he had in the 1920s. Just as Bohr was aware of how difficult it would be to devise a satisfying interpretation, Schrödinger understood the importance of tempering his theoretical goals with the experimental background. Beller (1999) is correct in pointing out that "[i]mpressive theoretical advances were possible, despite the wave–particle dilemma, because the interpretation of "crucial experiments" was not constrained in one particular way, as the received narrative implies" (p. 224). Ultimately, however, in interpreting quantum theory, Bohr and Schrödinger agreed which of these constraints is more acceptable. Thus, it should come as no surprise that Schrödinger's approach lost the battle of the 1930s as well, as in light of the above-discussed underlying experimental evidence, the theoretical possibility of retaining continuity seems far-fetched.

Schrödinger delivered a seminar on quantum mechanics in Dublin in the late 1940s and early 1950s, as a part of a larger effort to clarify the conceptual grounds of the revived wave-mechanical account and to accommodate seemingly unfavourable experimental results. The seminar demonstrated Schrödinger's continued adherence to a wave-mechanical account. He explicitly advocated a kind of holism stemming from it as the ultimate description of the micro-physical world and criticized the atomist adherence to quantum jumps and discontinuity. He suggested that what atomists regard as alternative occurrences at the atomic level should be understood as simultaneous happenings following the wave-mechanical formalism (Schrödinger, 1995). But the fact that he never published these essays may indicate that he still had serious doubts regarding his conclusions and that for the time being, Bohr's experimentally driven interpretation seemed more acceptable. A further question to explore is whether more recent accounts of quantum mechanics, fundamentally similar to that of Schrödinger, are justified in "throwing overboard everything quantum mechanical."

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[15] These treatments concern only a special case of the 90° angle of scattering, and thus do not address the kind of evidence provided by Compton and Simon.

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