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Quantenspringerei: SchrÃűdinger vs. Bohr

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Quantenspringerei: Schrödinger vs. Bohr[*]

HELGE KRAGH[**]

1. Introduction: Two giants of quantum physics

When quantum mechanics burst on the scene of physics in the fall of 1925, it was sometimes informally referred to as *Knabenphysik* because of the tender age of pioneers like Werner Heisenberg, Pascual Jordan, Wolfgang Pauli and Paul Dirac all of whom were in their early twenties. But not all the architects of our present quantum understanding of the world were *Knaben*. Niels Bohr was at the time forty years old, and Erwin Schrödinger two years his junior, and yet both of these two "oldies" made seminal contributions to quantum mechanics and its interpretation. It is with these contributions, and more generally the scientific and intellectual relations between the two physicists, that the present essay is concerned.

The scientific roads of Bohr and Schrödinger crossed at a number of times, and they were at a few occasions engaged in important dialogues concerning the foundation of physics and the meaning of the new quantum mechanics. It is worth emphasizing from the very outset the differences between these two giants of modern physics, who had not only quite different

[*] This is an extended and revised version of an invited lecture given to the Erwin Schrödinger Symposium held 13-15 January at the ESI (Erwin Schrödinger International Institute for Mathematical Physics) in Vienna. The official occasion for the symposium was Schrödinger's death fifty years ago. I am grateful to Wolfgang Reiter and the Organizing Committee for the invitation.

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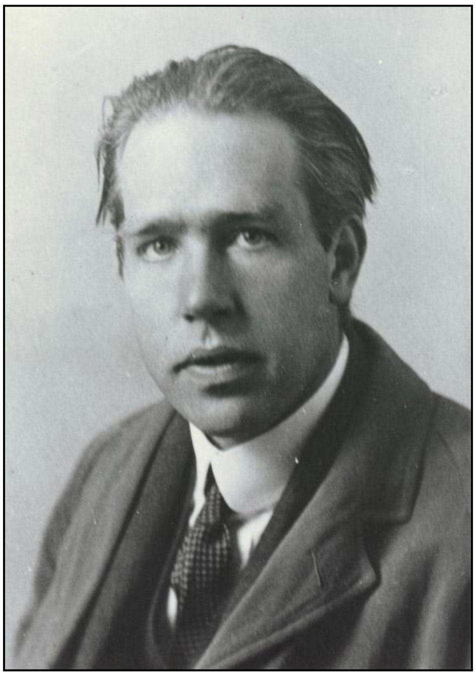
ideas about the nature of quantum physics but also different backgrounds and scientific careers. While Bohr was a successful school-builder who relied closely on interactions with colleagues and assistants, Schrödinger was much more his own. Although he agreed more with some physicists than others, he was basically not a member of any school, and he often worked in relative isolation, either because he wanted to or in some cases because he was forced to by external circumstances.[1]



Niels Bohr (1885-1962)

Erwin Schrödinger (1887-1961)

Ever since his stay in England before World War I, first with J. J. Thomson in Cambridge and subsequently (and more fruitfully) with Ernest Rutherford in manchester, Bohr followed a research programme focusing on atomic structure

[1] Raman and Forman 1969 (p. 297) describes Schrödinger in the early 1920s as "a marginal man, a loner, a member of no school," and argues that this independence was an important reason why he, contrary to other physicists, felt attracted by the ideas of a wave-particle dualism suggested by another "marginal man," namely Louis de Broglie in Paris.

and quantum theory. Later, in the 1930s, he successfully extended this programme into the realm of nuclear physics. These were the areas of physics on which he concentrated with great determination, and he stayed with them for fifty years. Schrödinger was, in a sense, a more versatile and less disciplined physicist with a broader range of scientific interests, which in his early years covered radioactivity, thermodynamics, statistical physics and colour theory. He had a deep interest in the general theory of relativity and unified field theories, and he also made noteworthy contributions to cosmology and the physics-biology interface. Naturally he considered atomic physics to be of great importance, but he also stressed that it was only part of a broader and more general picture of science and life, such as he confided in a letter to his colleague, the German physicist Wilhelm Wien. In this letter, written while he was struggling to understand the physical consequences of his new wave mechanics, he said:

> Bohr's standpoint, that a space-time description is impossible, I reject *a limine*. Physics does not consist only of atomic research, science does not consist only of physics, and life does not consist only of science. The aim of atomic research is to fit empirical knowledge concerning it into our other thinking. All of this other thinking, so far as it concerns the external world, is active in space and time. If it cannot be fitted into space and time, then it fails in its whole aim and one does not know what purpose it really serves.[2]

A broader view of the kind indicated in the letter was certainly not foreign to Bohr, who might well have agreed with the formulation in the second sentence,

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[2] Schrödinger to Wien, 25 August 1926, as translated in Moore 1989, p. 226.

but Bohr cultivated the view in a very different fashion. To borrow a term from historian Loren Graham, both were "expansionists" rather than "restrictionists," although Schrödinger's expansionism was considerably more limited and cautious than Bohr's.[3] Moreover, Bohr would not subscribe to the last sentence, the content of which constituted a major disagreement between the two physicists.

In spite of all their differences, both on a scientific and a personal level, Bohr and Schrödinger had much in common. First of all, they both endeavoured to understand nature on a basic level where physics could not be easily separated from philosophy. To put it differently, they were natural philosophers, not merely physicists in the ordinary and more narrow sense. Indeed, one can fairly say that they (together with Einstein and a few others) belonged to the last generation of this proud tradition of sages. It should also be pointed out that in spite of their disagreements they maintained a great deal of respect for one another on both a personal and professional level, which was an important element in and precondition for their dialogue concerning the meaning of quantum physics. For instance, Bohr was full of admiration for Schrödinger's works on wave mechanics, which he in 1931 nominated for a Nobel Prize. When Schrödinger received the prize two years later, sharing it with Dirac, it was to a large extent the result of Bohr's recommendation to the Swedish Academy of Science. Schrödinger was first proposed for the prize in 1928, but it was Bohr's nomination of him (together with Heisenberg) that gave the wanted result.

The Bohr-Schrödinger dialogue is an interesting chapter in the history of modern physics, but of course it was only one among several quantum dialogues involving also other physicists, of which the most important were

[3] For the notions of expansionism and restrictionism, see Graham 1981.

Einstein, Heisenberg, Pauli and Born. Although Bohr and Schrödinger met several times in the 1920s and 1930s, they were not really closely acquainted and they did not exchange a great many letters. Schrödinger was only once in Copenhagen and Bohr never visited Schrödinger either in Berlin or later during his long period in Dublin. As far as I know, the last time they met was during the 8th Solvay Congress in 1948. While their personal contacts were not extensive, the dialogue between them is nonetheless of great importance to historians and philosophers of science concerned with quantum mechanics.[4]

2. Before quantum mechanics

In the historiography of quantum theory is is customary to distinguish between two research programmes, one of which focused on atomic structure and led to the Göttingen quantum mechanics, and another one which was guided by radiation theory and led to wave mechanics. The first of these traditions had its roots in the old quantum theory of Bohr and Sommerfeld; it stressed the discontinuous nature of quantum processes, wanted to eliminate light quanta, and was mainly anchored in experimental and theoretical spectroscopy. On the other hand, the Einstein-de Broglie-Schrödinger programme was in favour of light quanta and a space-time description of atomic processes, it stressed the necessity of differential equations in quantum mechanics, and it wanted to do away with the discontinuous features of quantum theory. Apart from Bohr, the first research programme was followed by Heisenberg, Pauli and Born; while Max von Laue was among those who supported the second one. Whether this simplistic distinction is reasonable or not, I shall not discuss here, but it is important to point out that although Schrödinger belonged to the second of the

[4] Apart from the literature cited later in this essay, see also Chevalley 1992 and Harré 1982, which both are of a philosophical nature. Beller 1999 includes on pp. 122-131 an analysis of Bohr's dialogue with Schrödinger.

research traditions, in his younger days he had research interests that crossed the divide.

In spite of coming late to atomic theory, by the early 1920s Schrödinger was well acquainted with the Bohr-Sommerfeld tradition and had himself published a couple of important papers on atomic theory and theoretical spectroscopy. The one paper that I want to call attention to – because it was the one that established a direct connection to Bohr – is a paper of early 1921 in which Schrödinger explained the emission spectra of the alkali metals by suggesting that the valence electron in its elliptic motion would sometimes penetrate, or dip into, the inner stable shell of eight electrons in orbits which he called *Tauchbahnen*.[5] This paper, one of the more important ones in the old quantum theory, was squarely in the Bohr-Sommerfeld tradition of atomic structure and naturally attracted the attention of Bohr in Copenhagen. In fact, Bohr had himself, independently and a little earlier, come to the same idea of penetrating orbits. For example, it figured prominently in a lecture he gave in Copenhagen before the Physical Society in December 1920.[6]

Schrödinger included a reprint of the paper in the first of his letters to Bohr, of 7 February 1921, and Bohr responded belatedly some four months later, saying that the paper was indeed of great interest to him: "Naturally your paper in the *Zeitschrift für Physik* has interested me very much. As a matter of fact some time ago I worked out exactly the same thoughts and carried out the corresponding calculations."[7] Curiously, in this first exchange of letters Schrödinger wrote in English and Bohr responded in German. In his

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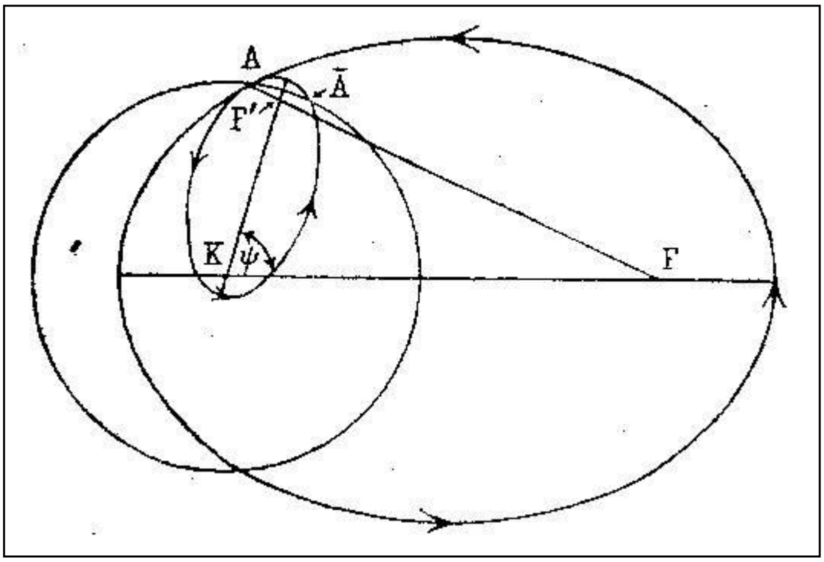
[5] Schrödinger 1921. For the significance of the paper, see for example Kragh 1979 and Darrigol 1992, pp. 152-154.

[6] The lecture was only published after Bohr's death, namely in Rud Nielsen 1977, pp. 43-70.

[7] Schrödinger to Bohr, 7 February 1921, and Bohr to Schrödinger, 15 June 1921, in Rud Nielsen 1977, pp. 737-738.

subsequent papers on atomic structure and the periodic system Bohr frequently quoted Schrödinger's contribution, which he also did in his Nobel Lecture of December 1922. While Schrödinger did not follow up his work on the penetrating orbits or apply it to the periodic system, it played an important part in Bohr's explanation of the system and of the transition groups in particular. Wheras Schrödinger had obtained 2_1 as the quantum state of the valence electron of sodium, Bohr concluded correctly that it was in a 3_1 state. (The n_k notation was the one commonly used as the time, *n* being the principal quantum number and *k* the azimuthal quantum number.)



The penetrating orbits of an alkali atom as pictured in Schrödinger's paper of 1921.

Apart from theoretical spectroscopy there was another field of common interest to the two physicists in this early period, namely the famous if short-lived BKS (Bohr-Kramers-Slater) theory that Bohr, together with Hendrik Kramers and John Slater, had proposed in May 1924 in an attempt to explain the Compton effect and other experiments without the unpalatable concept of light quanta.[8] The price Bohr had to pay was to abandon strict conservation of energy and momentum in individual atomic processes and replace it with conservation on a statistical level only. This was a price he was willing to pay, and it was also this feature of the BKS theory that for a while made Schrödinger adopt it with enthusiasm.

As he wrote in a long letter to Bohr of 24 May 1924, he was "extremely sympathetic" to the new radical ideas. He explained:

> As a pupil of old Franz Exner, I have long been fond of the idea that the basis of our statistics is probably not microscopic "regularity" but perhaps "pure chance" and that perhaps even the laws of energy and momentum may only have statistical validity. Your new point of view means a far-reaching return to classical theory, as far as radiation is concerned. I cannot quite go along when you always label the radiation "virtual." … Which is then the "real" radiation if not that which "causes" the transitions, i.e., creates the transition probabilities?[9]

But Schrödinger misunderstood Bohr's intentions, which he interpreted in accordance with his own ideas. Bohr had no wish to "return to classical theory." In September Schrödinger published a paper in *Die Naturwissenschaften* in which he endorsed the BKS theory, which, on the other hand, was severely criticized by Einstein, Sommerfeld and Pauli, among other physicists. At any rate, the theory turned out to be short-lived, as it was soon

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[8] The literature on the BKS theory is considerable. See, for example, Darrigol 1992, pp. 214-224 and Dresden 1987, pp. 159-214.

[9] Schrödinger to Bohr, 24 May 1924, in Stolzenburg 1984, p. 490. See also Moore 1989, pp. 159-163.

refuted by experiments made by Walther Bothe and Hans Geiger, and independently by Arthur Compton and Alfred Simon. Nonetheless, it was of great importance in the process that led to Heisenberg's quantum mechanics. Moreover, Schrödinger's attitude to it is of interest because it shows his relative independence from the Einstein camp. But it is time that I turn to the real dialogue between Bohr and Schrödinger, an exchange of views that focused on the new quantum mechanics that appeared in the fall of 1925 with the seminal papers of Heisenberg, Born, Jordan and Dirac.

3. A quantum confrontation

The new quantum mechanics, sometimes known as the Göttingen or matrix mechanics, was abstract and deliberately unvisualizable (*unanschaulich*), it rested on an unfamiliar mathematical formalism, and it was more impressive from a mathematical than from an empirical point of view. It was not a theory that appealed the slightest to Schrödinger, who did not originally develop his wave mechanics as an alternative to the Göttingen theory. Yet he knew about it, and what he knew he profoundly disliked. As he wrote in his paper from 1926 in which he proved the equivalence between quantum mechanics and wave mechanics, although naturally he knew about Heisenberg's theory, "because of the to me very difficult-appearing methods of transcendental algebra and because of the lack of visualizability, I felt deterred by it, if not to say repelled [*abgeschrecht, um nicht zu sagen absgestossen*]."[10]

His own theory rested on the entirely different foundation of wave packets propagating continuously in space and described by differential equations. What was most important in his dispute with Bohr, quantization was not introduced axiomatically but explained by reasonable mathematical

[10] Shrödinger 1926c, p. 735. On Schrödinger and *Anschaulichkeit*, see de Regt 1997.

requirements of the wave function satisfying the eigenvalue wave equation. Schrödinger believed he was able to restore continuity, not only by constructing the stationary states by continuous variations of non-stationary states, but to eliminate the need for sharp energy levels altogether by interpreting an emitted frequency as a resonance phenomenon involving only waves.

He was most pleased to have explained Bohr's discontinuous quantum jumps in terms of wave theory, for this was a notion he profoundly and emotionally disliked. It is much more gratifying, he said in his first paper on wave mechanics, "to conceive a quantum transition as an energy change from one vibrational mode to another than to regard it as a jumping of electrons. The variation of vibrational modes may be treated as a process continuous in space and time and enduring as long as the emission process persists."[11] Whereas Bohr was willing to abandon a space-time description of atomic processes, according to Schrödinger this was not an option, because our very thinking proceeds in space and time. "From a philosophical standpoint," he said in his second paper, "I should consider a conclusive decision in this sense as equivalent to a complete surrender. For we cannot really avoid thinking in terms of space and time, and what we cannot comprehend within it we cannot understand at all."[12] This is also what he implied in his letter to Wien of 25 August 1926 quoted above. To summarize, in 1926 Schrödinger believed that:

(i) Discrete energy states were artefacts of wave processes.
(ii) Quantum transitions were continuous changes in space-time from one vibrational mode to another.
(iii) Electrons and other point particles were wave packets.

[11] Schrödinger 1926a, p. 375.
[12] Schrödinger 1926b, p. 509.

Schrödinger's opposition to the reality of stationary states and quantum jumps was deep-seated, and over the years he returned to it at several occasions. As late as 1952 he published a paper entitled "Are There Quantum Jumps?" in which he repeated his arguments, only now in a much more rhetorical style and by placing the issue in a broad historical and cultural context. He said that the idea of discontinuous quantum transitions was the modern counterpart of the epicycles of ancient astronomy, thereby implying that they were figments of imagination.[13] While Bohr politely ignored Schrödinger's article in the *British Journal for the Philosophy of Science*, or may not even have been aware of it, Max Born offered a critical response to it in the same journal.[14] As to Schrödinger's question concerning the existence of quantum jumps, the modern answer is neither unequivocally pro-Bohr ("yes") or pro-Schrödinger ("no"). Since the 1980s quantum jumps between atomic stationary states have indeed been observed experimentally, but they are governed by the differential equations of quantum mechanics and therefore not abrupt in the sense originally suggested by Bohr.[15]

To return to the 1920s, Schrödinger sometimes described Bohr's model of light emittance as "monstrous", and he was happy to have put such a monster in the grave.[16] Thus, in an important letter to Lorentz of 6 June 1926 in which he pictured particles as nothing but concentrated wave packets, he wrote:

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[13] Schrödinger 1952.

[14] Born 1953.

[15] For the modern understanding of quantum jumps, see Carmichael 2009.

[16] Schrödinger was not alone in describing Bohr's atom as "monstrous." For example, in a letter to Sommerfeld of the spring of 1916 Paul Ehrenfest used the same expression (Klein 1970, p. 286).

> I was so extremely happy, first of all, to have arrived at a picture in which at least something or other really takes place with that frequency which we observe in the emitted light that, with the rushing breath of a hunted fugitive, I fell upon this something in the form it immediately offered itself, namely as the amplitudes periodically rising and falling with the beat frequencies. … The frequency discrepancy in the Bohr model, on the other hand, seems to me (and has indeed seemed to me since 1914) to be something so monstrous [*Ungeheuerliches*], that I should like to characterize the excitation of light in this way as really almost inconceivable.[17]

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ARE THERE QUANTUM JUMPS?

PART I*

E. SCHRÖDINGER

Schrödinger's wave mechanics was at first met with some scepticism, and sometimes even hostility, by physicists associated with the Copenhagen-Göttingen camp who tended to see the emphasis on classical virtues such as spatio-temporal continuity and visualizability as a retrograde step. In June 1926

[17] Schrödinger to Lorentz, 6 June 1926, in Przibram 1963, p. 56. Apparently Schrödinger first became aware of Bohr's atomic theory in 1914.

Heisenberg reported to Pauli that he found Schrödinger's theory to be nothing less than "disgusting" [*abscheulich*] and that he found his emphasis on visualizability to be just "bullshit" [*Mist*].[18] Pauli basically agreed, although he initially found Schrödinger's approach to be "perhaps not so crazy." He later teased Schrödinger by calling his belief that quantum phenomena could be understood in terms of continuum physics, such as he had argued in his discussions with Bohr in Copenhagen, for "the provincial Zurich superstition."[19]

However, after the equivalence proofs of Schrödinger and others, most physicists adopted a more pragmatic attitude toward the two competing versions of quantum mechanics. Bohr was one of them. He had followed closely and with some sympathy the publications on wave mechanics, and contrary to some of his colleagues he did not regard Schrödinger's theory as just a mathematical artifice, but stressed its physical significance which he thought brought the wave-particle dualism sharper into focus. He recognized early on that the wave features inherent in Schrödinger's formulation had to play an important role in a future conception of objects as possessing distinct and perhaps contradictory physical properties. On the other hand, there was a limit to his sympathy and compromising stand, for he disagreed emphatically with Schrödinger's attempt to avoid the discontinuous quantum jumps.

Schrödinger's visit to Copenhagen in October 1926 has become part of the physics folklore, but unfortunately we have no transcript or direct report of the memorable dialogue that took place during the few days of his visit. We have, on the other hand, some details about it from subsequent

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[18] Heisenberg to Pauli, 8 June 1926, in Hermann, von Meyenn and Weisskopf 1979, p. 328. The German word *Mist* has different connotations and may also be translated as "crap" or "deep fog."

[19] Pauli to Sommerfeld, 9 February 1926, and Pauli to Schrödinger, 22 November 1926, in Hermann, von Meyenn and Weisskopf 1979, p. 293 and p. 357.

correspondence, from Bohr's recollections, and not least from Heisenberg's colourful (and perhaps not entirely accurate) account in his book *Der Teil und das Ganze*. What we do know is that Schrödinger gave a superb lecture on the foundations of wave mechanics to the local physical society and a few days later a colloquium in which his views were severely criticized by the Copenhagen physicists. Apart from Schrödinger, Bohr and Heisenberg, also the young Swedish physicist Oskar Klein was present during the discussions. According to Klein's recollections, Bohr and Heisenberg demonstrated that if Schrödinger's ideas were correct, the probability of spontaneous emission from an upper to a lower state would be proportional to the product of the number af atoms in the two states, which contradicted experimental knowledge.[20] In general Bohr and Heisenberg tried to convince Schrödinger that although one could calculate transition probabilities for quantum jumps on the basis of wave mechanics, the discontinuous transitions would remain. Schrödinger then despaired and, according to one source, said: "Wenn wir zur dieser Herumspringerei zurückkehren müssen, dann bedaure ich, dass ich mich in die Sache eingemischt habe."[21]

According to Heisenberg's account, Bohr acted as a "relentless fanatic" who passionately and persistently sought to convince Schrödinger about his mistakes, but of course to no avail. He "was not prepared to make a single concession to his discussion partner or to tolerate the slightest obscurity." When Schrödinger fell ill and had to stay in bed, Bohr, sitting at the bedside, merely continued his arguments. Schrödinger is to have said that if he had to put up with the *verdammte Quantenspringerei*, then he was sorry that he ever got himself involved in quantum theory. Shortly after his return to Berlin, Schrödinger wrote Bohr a long letter in which he expressed in a highly

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[20] Dresden 1987, p. 77.

[21] Møller 1963, p. 59. For Heisenberg's account, see Heisenberg 1969, pp. 105-109.

personal language his gratitude to Bohr and called his conversations with him "a truly unforgettable experience" – and he did not mean it sarcastically. But with respect to the disagreements nothing had changed. As he said in the letter: "Even if a hundred attempts have failed, one ought not to give up hope of arriving at the goal of representing the true properties of space-time events through – I don't say classical pictures – but through logically consistent conceptions."[22]

We know from Bohr's correspondence at the time that Schrödinger's arguments made an impression on him and that they stimulated him to think deeper and more clearly about the foundation of quantum physics and to start preparing what the following year became the complementarity principle. One of Schrödinger's less known papers which both inspired and provoked Bohr was a paper on the transition from micromechanics to macromechanics that appeared in *Die Naturwissenschaften* in the summer of 1926.[23] In this paper Schrödinger claimed that the electron can be represented by a wave packet of proper vibrations and is therefore, in a sense, nothing but a wave packet. Bohr disagreed, incorporating his criticism into the complementarity principle that was enunciated the following year.

4. The principle of complementarity

This famous and sometimes controversial principle was first introduced in public at the Como conference in September 1927, a commemoration of the

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[22] Schrödinger to Bohr, 23 October 1926, in Kalckar 1985, p. 459. See also Schrödinger to Wien, 21 October 1926, as quoted in Pais 1991, p. 299, where Schrödinger said about Bohr that his "attitude works strongly sympathetically compared with what one often meets in stars of medium size in our profession."

[23] Schrödinger 1926d. This paper, in which Schrödinger introduced the idea of what later became known as coherent states, was not only of interest to Bohr; it also played an important role in Heisenberg's formulation of the uncertainty principle in 1927. See Steiner 1988.

centenary of the death of Alessandro Volta, of battery fame. Most of the leading physicists attended the meeting, but not Schrödinger, who was invited but unable to attend and who did not, as far as I know, respond directly to Bohr's address. (Also Einstein was invited, but he did not attend either.)

In Como, Bohr dealt critically and at length with Schrödinger's position and his hope to remove the "irrational element" – a reference to the quantum postulate – by means of a mechanism based on wave resonances. But, as Bohr argued, this was not possible, for Schrödinger's wave mechanics was "a symbolic transcription of the problem of motion of classical mechanics, adapted to the requirements of quantum theory," and for this reason it could only be interpreted by "an explicit view of the quantum postulate."[24] In his arguments for the complementarity principle Bohr made extensive use of the wave picture of particles based on the theories of de Broglie and Schrödinger, but that was his only concession to Schrödinger's point of view.

The dialogue with Schrödinger continued during the important 5th Solvay congress the following month, October 1927. By that time Schrödinger had abandoned his original identification of electrons with wave packets (because he realized they would disperse), and he recognized that in some experiments electrons behaved as particles and in other as waves. But this did not make him accept the wave-particle dualism that was at the heart of Bohr's conception of quantum mechanis. As he said during the general discussion in Brussels: "I regard the compromise … of assuming a combination of waves and point electrons, as simply a provisional manner of resolving the difficulty."[25]

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[24] Bohr's Como address, entitled "The Quantum Postulate and the Recent Development of Atomic Theory," was published in 1927 and is reprinted in Kalckar 1985, pp. 113-136, where the quotation is on. p. 127.

[25] Bacciagaluppi and Valentini 2009, p. 118.

The Solvay conference of 1927 is best known for Bohr's famous discussions with Einstein, not for Schrödinger's contributions.[26] Both Bohr and Schrödinger gave addresses, but it is probably fair to say that Schrödinger did not play a very active role in the memorable discussions at this conference except that he, in a general way, joined Einstein and de Broglie in their opposition against the probability interpretation argued by Bohr, Heisenberg and Born. Schrödinger did not participate in the 1930 congress, but he did attend the 1933 congress, which was however mainly devoted to the atomic nucleus, a subject he had little interest in and he consequently kept a low profile during the discussions. Bohr participated in both 1930 and 1933.

Although Schrödinger in his later writings often referred to and used the concept of complementary, he rejected Bohr's more synthetic conclusions and never embraced the principle. On the contrary, he evolved into a sharp critic of the complementarity principle, which he, much like Einstein, came to see as merely a word or rhetorical concept used by the Copenhageners to smoothe out the interpretative problems of quantum mechanics.

When the young Danish physicist Christian Møller visited Berlin in the summer of 1928, Schrödinger told him that he was still of the opinion that "Die Heisenbergsche Matrizenmechanik ist ein Gerippe von abschreckender Kahlheit." As to Bohr's complementarity interpretation he was no less hostile: "Bohr will alle Schwierigkeiten wegkomplementieren," he said.[27] He kept to this view, which he was not alone in arguing. In a letter more than thirty years later, two years before his death, Schrödinger denounced vigorously what he called the "thoughtless slogan" of complementarity: "If we were not thoroughly convinced that [Bohr] is honest and really believes in the relevance

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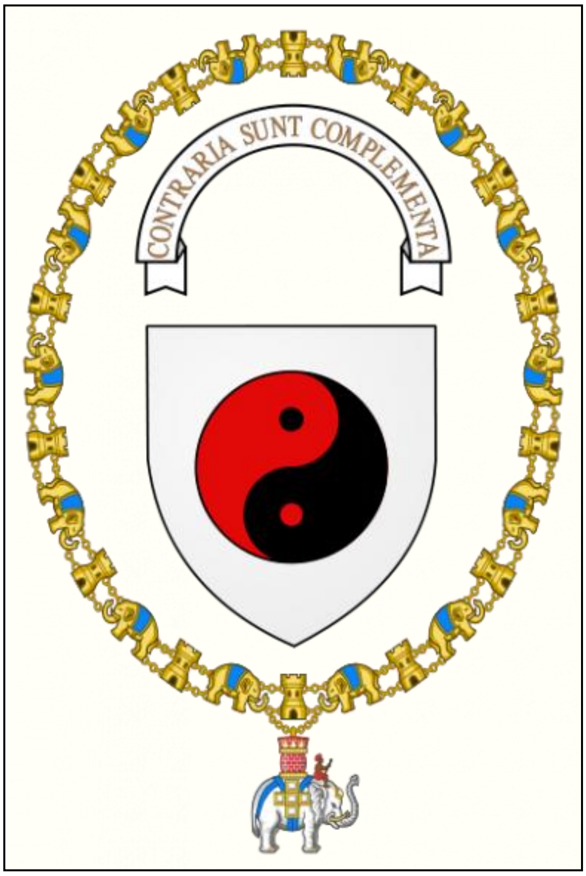
[26] A comprehensive historico-critical account of the 1927 Solvay Congress has recently been published as Bacciagaluppi and Valentini 2009.
[27] Møller 1963, p. 61.

of his – I do not say theory but – sounding word, I should call it intellectually wicked." He sarcastically said that "Old-age dotage closes my eyes towards the marvelous discovery of 'complementarity'."[28] Whenever Schrödinger used the words "complementarity" or "complementary" he emphasized that it was not in the over-arching and, to his mind, self-contradictory and mystical sense promoted by Bohr. Bohr used the term as denoting features which are both mutually exclusive and jointly indispensible for a complete description of a phenomenon. Schrödinger, on the other hand, insisted that complementary features had to be compatible aspects of the phenomenon.



Bohr's coat of arms, exhibiting the message
of the principle of complemnetaity.

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[28] Schrödinger to John Synge, an Irish mathematical physicist, 9 October 1959, in Moore 1989, p. 473. See also Bitbol 1996, pp. 212-213.

5. Schrödinger's cat

The fundamental disagreements between Bohr and Schrödinger can be further illustrated by two famous thought experiments of the 1930s, the one being the EPR argument and the other Schrödinger's cat. As is well known, in May 1935 Einstein and his two collaborators at Princeton, Boris Podolsky and Nathan Rosen, published a paper in which they argued that a quantum mechanical description of reality cannot be complete. This paper, more famous today than it was at the time, caused Bohr to develop an equally famous counterargument in support of completeness and the Copenhagen view of quantum mechanics.

While the general impression among physicists was that Bohr had once again countered Einstein's objections satisfactorily, it was not Schrödinger's impression. On the contrary, he immediately sided with Einstein, writing him that he was happy that Einstein had "evidently caught dogmatic q.m. [quantum mechanics] by the coat tails."[29] Einstein, on his side, saw Schrödinger as a brother in arms and called him "the only person with whom I am actually willing to come to terms," such as he wrote in a letter of 8 August 1935. "Almost all the other fellows do not look from the facts to the theory but from the theory to the facts," Einstein complained; "they cannot extricate themselves from a once accepted conceptual net, but only flop about in it in a grotesque way.[30]

As one might expect, Schrödinger was not satisfied with Bohr's response to the EPR argument, such as he made clear in a long letter of 13 October 1935 in which he questioned Bohr's firm conviction that measurements must ultimately and according to their very nature be interpreted in classical terms. Schrödinger saw no reason why this should be the case and asked Bohr to come up with such reasons: "You have repeatedly expressed your definite

[29] Schrödinger to Einstein, 7 June 1935, quoted in Moore 1989, p. 304.
[30] Einstein to Schrödinger, 8 August 1935, quoted in Moore 1989, p. 305.

conviction that measurements must be described in terms of classical concepts. … It must be among your firmest convictions – and I cannot understand what it is based upon."[31] In his somewhat evasive letter of reply Bohr merely repeated that a classical description of experiments is unavoidable because measuring instruments cannot be of a quantum nature. Bohr later expressed the same view more clearly:

> We must recognize above all, that even when phenomena transcend the scope of classical physical theories, the account of the experimental arrangement and the recording of observations must be given in plain language, suitably supplemented by technical physical terminology. This is a clear logical demand, since the very word "experiment" refers to a situation where we can tell others what we have done and what we have learned.[32]

In whatever formulation, Bohr's answer did not satisfy Schrödinger. In effect, the two physicists could only agree that they disagreed.

The EPR argument motivated Schrödinger to publish a remarkable paper in *Die Naturwissenschaften* in which he illustrated his arguments against the Copenhagen view by means of what has become known as "Schrödinger's cat."[33] His point was, to put it briefly, that it seems to follow from the Copenhagen view that a cat, subjected to a diabolical experiment, could be "half dead" and ascribed a wave function which was a superposition of the wave functions of the live cat and the dead cat. Clearly, this was not only

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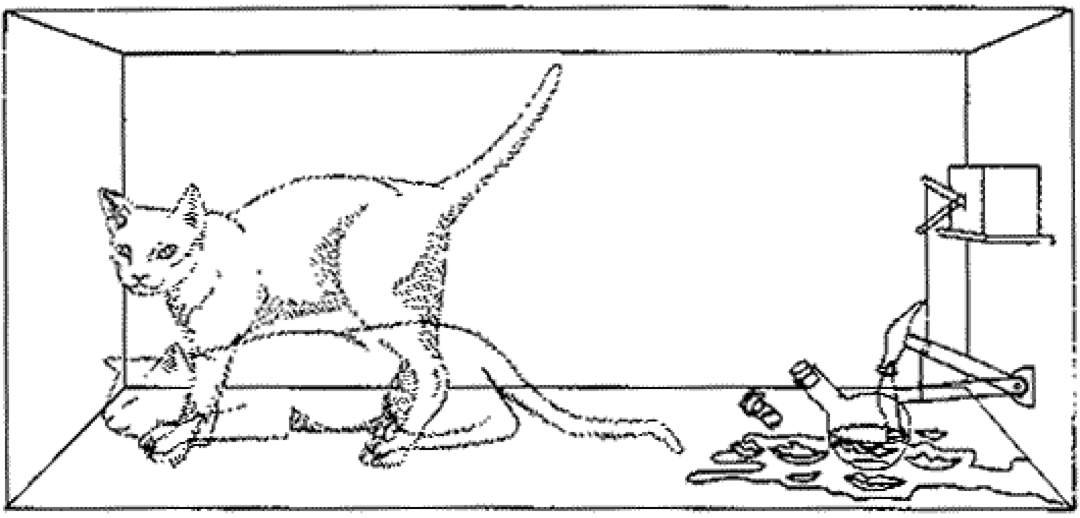
[31] Schrödinger to Bohr, 13 October 1935, in Kalckar 1996, p. 508.

[32] Bohr 1959, p. 72. From address on "The Unity of Knowledge" delivered at a conference at Columbia University in 1954.

[33] Schrödinger 1935. English translation in Wheeler and Zurek 1983, pp. 152-167.

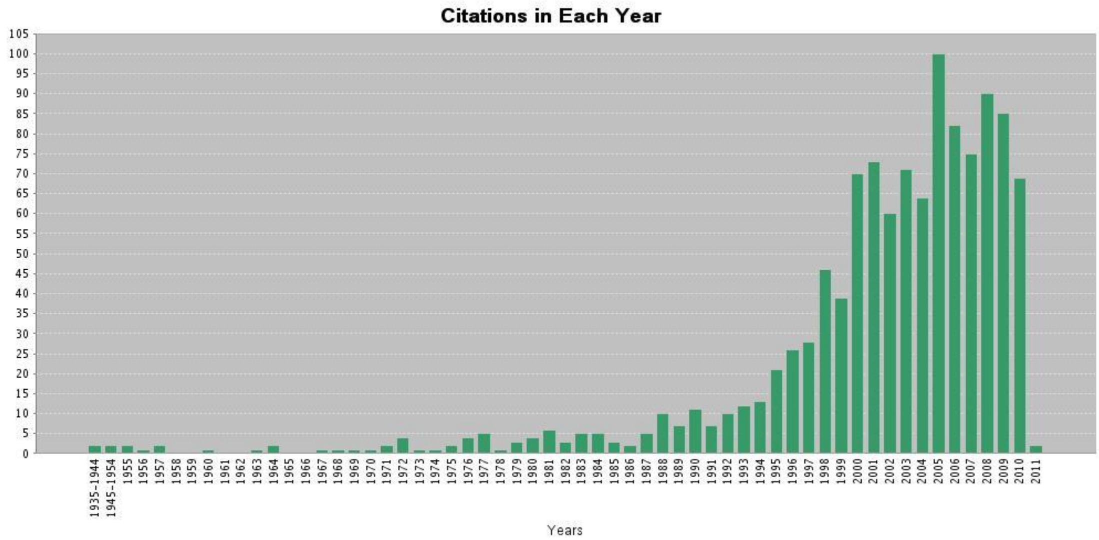

Sonderdruck aus Die Naturwissenschaften 1935. 23. Jahrg., Heft 48
(Verlag von Julius Springer, Berlin W 9)
Printed in Germany

Die gegenwärtige Situation in der Quantenmechanik.
Von E. Schrödinger, Oxford.



The front paper of Schrödinger's cat paper, and a later illustration showing the poor cat being killed (?) because of the hydrogen cyanide being randomly produced by a radioactive device following the laws of quantum mechanics.

counter-intuitive, it was nonsensical. Bohr never responded to the challenge. He apparently chose to ignore the cat paradox, perhaps because he found its premises so evidently flawed. After all, according to Bohr a macroscopic body such as a cat, or a bottle of sodium cyanide, could not be assigned a wave function, and so there was no paradox to solve. Schrödinger's cat did not pose a problem for the Copenhagen physicists, it merely confirmed them in their view that the waves of Schrödinger's theory were abstractions that could be used only to compute expectations about human experience.



Citations to Schrödinger's cat paper, which for a long time failed to attract attention.

The following year, 1936, Schrödinger met Bohr in London, and in a letter to Einstein he told him about Bohr's strong conviction that the Copenhagen interpretation was the one and only correct understanding of quantum mechanics. Bohr even found the opposition of Einstein, Schrödinger and Laue to be "high treason." Of course Schrödinger did not plea guilty in high treason, but he was not unreceptive to Bohr's arguments. As he wrote to Einstein: "He [Bohr] speaks with the deep inner conviction of an extraordinarily intelligent man, so that it is difficult for one to remain unmoved in one's position."[34]

6. Schrödinger and Eddington

I would like to end with briefly mentioning an episode in Schrödinger's career which did not involve Bohr directly, but which indirectly illustrates the difference in mentality between the two physicists. In the 1930s the famous British astronomer and specialist in relativity, Arthur Stanley Eddington,

[34] Schrödinger to Einstein, 23 March 1936, quoted in Moore 1989, p. 314.

embarked on an ambitious programme to unify, in a sense, the physics of the microcosmos with that of the universe at large, a research programme that culminated in his *Relativity Theory of Protons and Electrons* of 1936 and, posthumously, the unfinished *Fundamental Theory* of 1946. Eddington's grand but unorthodox theory of everything was either ignored or dismissed, sometimes ridiculed, by the majority of physicists who found his idiosyncratic use of quantum mechanics totally unacceptable. Eddington lectured on his new theory at a conference in Warsaw in 1938, where most of the peers of quantum physics were present, including Bohr. They all agreed that his unified theory was not only unorthodox, but also that it bordered on pseudoscience. It was nothing but "romantic poetry," as Pauli phrased it in an earlier letter to Klein.[35]

Had Schrödinger been present in Warsaw (which he was not), he would undoubtedly have disagreed, for as the only leading quantum physicist he strongly supported Eddington's theory, at least for a while. The philosophical grandeur of Eddington's project resonated with his romantic temperament, and so did Eddington's attempt to extend wave mechanics to the whole universe. Schrödinger was convinced, as he wrote in a paper of 1937, that "for a long time to come, the most important research in physical theory will follow closely the lines of thought inaugurated by Sir Arthur Eddington."[36]

For a couple of years he was quite enthusiastic about the theory, which he defended at a conference in Bologna in front of Bohr and other quantum physicists. In a letter to Eddington he wrote:

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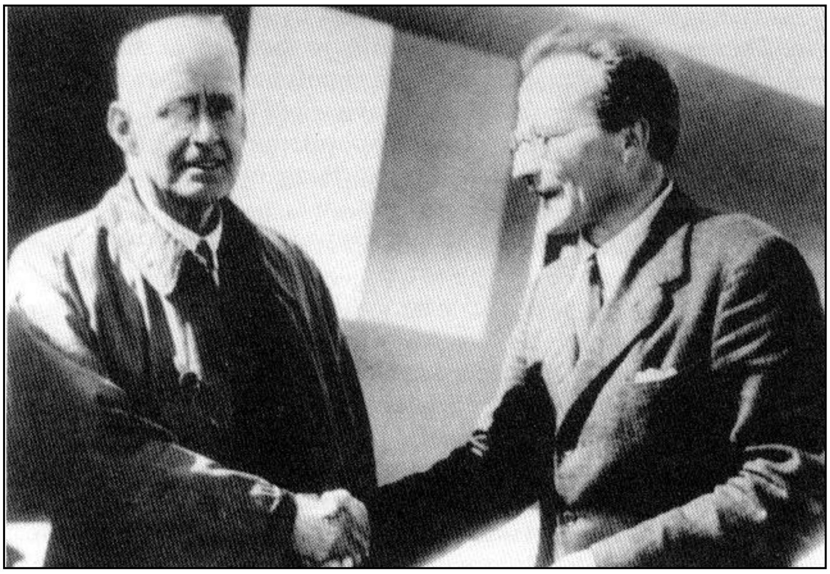
[35] Eddington 1939. Pauli to Klein, 18 February 1929, in Hermann, von Meyenn and Weisskopf 1979, p. 491.

[36] Schrödinger 1937, p. 744, a glowing essay review of Eddington's *Relativity Theory of Protons and Electrons*. On Schrödinger's work on cosmology within the framework established by Eddington, see Rüger 1988.

> I met with an unvanquishable incredulity of the important group Bohr, Heisenberg Pauli and their followers. I was in an extremely difficult position … because many of your arguments are as ununderstandable to me as they are to them. … But the trouble is, that ordinary language is a very imperfect instrument for explaining entirely new ideas in physics. My suspicion is, that there exist a few very important points, which you explain orderly in the right place, but for some reason or other we misinterpret your words just as if they were Chinese.[37]

Schrödinger's enthusiasm eventually cooled, as it dawned upon him that Eddington's theory might as well have been in Chinese. Yet the episode is worth mention because it illustrates his willingness to consider unpopular and highly unorthodox views, something that Bohr would never dream of.



Eddington and Schrödinger shaking hands in Dublin in 1942.

[37] Schrödinger to Eddington, 23 October 1937 (Archive for History of Quantum Physics).

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