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Claus Kiefer
Editor

Albert Einstein, Boris Podolsky, Nathan Rosen

Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?

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Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?

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Preface

The year 2015 not only marked the 100th anniversary of the general theory of relativity but also the 80th anniversary of one of the most relevant papers of theoretical physics: the paper by Albert Einstein, Boris Podolsky, and Nathan Rosen (EPR) from 1935 printed and annotated in this edition. While the theory of relativity has become part of the textbook canon and thus the historical works of Einstein are cited less frequently, the EPR paper is quoted quite regularly in renowned journals such as Physical Review and Nature. This shows that EPR's question as to the completeness of quantum mechanics is still relevant. The present annotated edition details the historical context and reception of the EPR paper as well as the impact it had on modern research and the conceptual fundamentals of quantum theory, which are still being discussed. While Niels Bohr and others initially dismissed the EPR paper as irrelevant and as based on misunderstandings, it is experiencing an unending renaissance. Turns out, it really is a significant paper!

The text itself is a discussion on theoretical physics and requires prior physical and mathematical knowledge for better understanding. However, since its content stretches an arc far into philosophy, I wanted to do it justice and keep this annotated edition as easily comprehensible as possible under the circumstances. I thus wrote it also with a more general reader in mind, who does not necessarily understand the mathematical aspect of the paper and is rather interested in its epistemic aspects.

The book also includes the full text of Bohr's paper with the same title and published in the same year, as well as a translation of Einstein's article from 1948 published in the journal *Dialectica.*

I would like to thank Prof. Dr. Jürgen Jost for asking me to write this book and accompanying the writing process with kind and constructive support; my thanks also goes to Springer-Verlag for the efficient help and to Sebastian Linden and Anna Katharina Hudert for their excellent translation into English. Last but not least I want to thank H.-Dieter Zeh, Erich Joos, Klaus Volkert, and Paul Busch for a critical review of the original German manuscript and for helpful discussions.

Cologne, Germany Claus Kiefer
February 2020

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Backstory

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In 1934, three physicists came together in Princeton, United States, to author a scientific paper that would turn out to be one of the most cited publications of the twentieth century. They were Albert Einstein, Boris Podolsky, and Nathan Rosen. Einstein (1879–1955) was already world-famous back then for developing his theory of relativity. Unwilling to live in Nazi-Germany, he had settled at Princeton's newly founded *Institute for Advanced Study* in October 1933, where he remained until his death in 1955.

Boris Podolsky, born in 1896 in Taganrog, Russia (where also the writer Anton Chekhov was born), had emigrated to the United States in 1913. In 1928, he received a PhD from the *California Institute of Technology* (Caltech) and came to Princeton with a fellowship from the Institute for Advanced Study in 1933, after detours to *i.a.* Leipzig in Germany, Kharkov in Ukraine (back then USSR), and again to Caltech. In Kharkov, he had worked on the then brand new theory of quantum electrodynamics with Vladimir Fock, and Paul Dirac, one of the pioneers of quantum mechanics, who was travelling through the USSR at the time.

Podolsky and Einstein knew each other from Einstein's earlier visits to the United States. Einstein's first trip to the United States was mainly a visit to Caltech. It took place from December 1930 to March 1931 following an invitation by physicist Richard Tolman, who contributed greatly to the theory of relativity. During that time, Tolman, Podolsky, and Paul Ehrenfest (1880–1933), who was visiting from the Netherlands, were working on an application of general relativity, namely on the gravitational field produced by light [156]. They submitted their work for publication in January 1931. Einstein spent most of his second trip to the United States at Caltech, too, from late December 1931 to early March 1932. This time he collaborated with Podolsky, the result being a joint two-paged publication by Einstein, Tolman, and Podolsky on quantum theory [67]. This work, however, was later described by Einstein's biographer Abraham Pais as less-than-successful [122, p. 494].

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The third physicist, Nathan Rosen, was born in New York City in 1909. Having received a PhD from the *Massachusetts Institute of Technology* (MIT) in 1932, Rosen came to the University of Princeton in 1934. His work had focused on atomic and molecular physics, but he had also taken an interest in Relativity and had published a paper on the unified field theory of gravitation and electromagnetism that Einstein was pursuing back then. It therefore comes as little surprise that Rosen contacted Einstein in Princeton, hoping for his advice in this matter. Max Jammer describes in his well known book on quantum mechanics that Rosen was quite surprised by how friendly Einstein was when they discussed his work [98, p. 181]. When they met in the institute's courtyard the following day, Einstein asked him: "Young man, what about working together with me?"

This is the personal backstory to the collaboration of Einstein, Podolsky, and Rosen that would go down in history as EPR. The scientific backstory is much more intricate and leads us back to the beginning of the twentieth century. Planck's paper in 1900 and Einstein's in 1905 quietly initiated what would later become quantum theory in 1925 to 1927 – a theory that Einstein, Podolsky, and Rosen were still struggling to understand in Princeton in 1934/1935.

No theory has ever changed our physical world view as much as quantum theory has. Aside from not incorporating gravitation, the theory provides successful descriptions of all interactions, ranging from macroscopic bodies to elementary particles, such as the ones explored at the particle accelerator LHC in Geneva, Switzerland. The basic equations of quantum theory have been tested in countless experiments, so no one doubts their validity. However, there is no mutual consent on how to interpret the theory, not least shown by the numerous citations of the EPR paper. What is it that stirs such a feeling of unease in a theory whose formalism is beyond controversy? We will see that the debate essentially centres on what reality is or rather what we want reality to be.

The impulse for the EPR paper clearly came from Einstein. He was, for one, the threesome's senior and generally distanced Podolsky, and Rosen scientifically, but he had also contributed substantially to the primary stage of quantum theory and accompanied the development of the actual theory with intense attention and criticism since 1925. We will see that quantum theory is a recurring theme in Einstein's work from 1925 to EPR and even further on. However, Einstein depended on critical dialogue with colleagues to work out his theories, which is why the paper wouldn't have been written without Podolsky and Rosen, at least not this way.

1.1 Einstein's Contributions to Early Quantum Theory

Einstein's liaison with quantum theory began about thirty years before the three physicists met in Princeton. Struggling to find a position in academic teaching or research, Einstein took a job as patent examiner (third class) at the Federal Office for Intellectual Property (Swiss patent office) in Bern in 1902. Some, both privately and scientifically, turbulent years followed. He married his fellow student Mileva Marić in early 1903. At the time the two already had a daughter, Lieserl, that Mileva had delivered during a stay in her hometown Novi Sad in Serbia the year before. Einstein never saw his daughter, whose fate remains unknown. In May 1904, Einstein and Mileva's first son Hans Albert was born in Bern.[1]

Despite his turbulent private life and his 48-hour-week at the patent office, Einstein actively pursued his scientific work. In 1905, he published no less than five outstanding papers, all of which made history.[2] 1905 is often referred to as Einstein's *annus mirabilis*, echoing Isaac Newton's *anni mirabiles* 1664 to 1666, during which he laid the groundwork for his theory of gravitation. Of those five papers from 1905, the one that concerns us most is the one on the light quantum hypothesis. It was the first major contribution to quantum theory since Planck's initial papers in 1900 and 1901 and the only one that Einstein himself qualified as revolutionary. In a letter to Conrad Habicht in May 1905,[3] Einstein wrote (The Collected Papers of Albert Einstein vol. V, Doc. 27):

> I promise you four papers in return, the first of which I might send you soon [...]. The paper deals with radiation and the energy properties of light and is very revolutionary, as you will see [...].[4]

What was so revolutionary about this paper? Einstein starts out by expressing his discomfort with an obvious incoherence in the description of nature: the simultaneous occurrence of continuous and discrete quantities. The electromagnetic field strengths are continuous functions and are empirically well described by Maxwell's equations. Matter, however, consists of a finite number of atoms and is, therefore, discrete by nature. The first lines of Einstein's paper read as follows [51, p. 132]:

> There exists a profound formal difference between the theoretical conceptions physicists have formed about gases and other ponderable bodies, and Maxwell's theory of electromagnetic processes in so-called empty space. While we conceive of the state of a body as being completely determined by the positions and velocities of a very large but nevertheless finite number of atoms and electrons, we use continuous spatial functions to determine the electromagnetic state of a space [...].[5]

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[1] It is worth reading the detailed account of Einstein's life by Fölsing [74].

[2] Cf., e.g., Stachel [153] or Kiefer [104].

[3] Habicht, Einstein, and Romania-born Maurice Solovine regularly met in Bern for informal debates on physics and philosophy, which they called the 'Akademie Olympia'. Fölsing [74, p. 99] wrote in his biography of Albert Einstein: "The three would meet regularly in the evening for a frugal meal of sausage, some Gruyère cheese, a little fruit, honey, and tea. That, according to Solovine's recollections, was enough for them to 'brim over with merriment.' "

[4] "Ich verspreche Ihnen vier Arbeiten dafür, von denen ich die erste in Bälde schicken könnte [...]. Sie handelt über die Strahlung und die energetischen Eigenschaften des Lichtes und ist sehr revolutionär, wie Sie sehen werden [...]."

[5] "Zwischen den theoretischen Vorstellungen, welche sich die Physiker über die Gase und andere ponderable Körper gebildet haben, und der Maxwellschen Theorie der elektromagnetischen Prozesse

This discrepancy in the roles of fields and matters kept him preoccupied his entire life. Einstein's later efforts to construct a unified field theory were mainly driven by his desire to eliminate this discrepancy. In his 1905 paper he introduced the heuristic point of view[6] that not only the energy of matter but also the energy of electromagnetic radiation should be discontinuously distributed. This assumption gave Einstein the means to better describe certain observations, including black-body radiation and the photoelectric effect, i.e., the emission of electrons from a metal surface by infalling ultraviolet light. Einstein wrote [51, p. 133]:

> Indeed, it seems to me that the observations [. . .] can be understood better if one assumes that the energy of light is discontinuously distributed in space. According to the assumption to be contemplated here, when a light ray is spreading from a point, the energy is not distributed continuously over ever-increasing space, but consists of a finite number of energy quanta that are localised in points in space, move without dividing, and can be absorbed or generated only as a whole.[7]

The term energy quanta that Einstein introduced in this paper would later give its name to quantum theory. Einstein was, of course, aware of Planck's pioneering work from 1900; we know from his letters to Mileva that he had been exploring the topic since 1901.

In his now famous lecture at the German Physical Society (*Deutsche Physikalische Gesellschaft*) on December 14, 1900, Planck had presented a derivation of the black-body radiation formula.[8] Black-body radiation is the electromagnetic radiation within a completely enclosed cavity when the walls are held at a constant temperature T. Back in 1859, German physicist Gustav Robert Kirchhoff, whom Abraham Pais called the

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im sogenannten leeren Raume besteht ein tiefgreifender formaler Unterschied. Während wir uns nämlich den Zustand eines Körpers durch die Lagen und Geschwindigkeiten einer zwar sehr großen, jedoch endlichen Anzahl von Atomen und Elektronen für vollkommen bestimmt ansehen, bedienen wir uns zur Bestimmung des elektromagnetischen Zustandes eines Raumes kontinuierlicher räumlicher Funktionen [. . .]. "

[6] As highlighted in the title of the paper. A heuristic point of view is a working hypothesis or a preliminary assumption; the Oxford Dictionary defines 'heuristic' as "enabling a person to discover or learn something for themselves, rather than being directed". It is derived from the Greek *heurískein*, meaning 'to find'. Think of the story about Archimedes exposing a dishonest goldsmith who had added silver to a crown supposedly made of pure gold. He did so by use of the principle that today bears his name, which, according to legend, he found while taking a bath and then proclaimed *eureka* ("I found it").

[7] "Es scheint mir nun in der Tat, daß die Beobachtungen [. . .] besser verständlich erscheinen unter der Annahme, daß die Energie des Lichtes diskontinuierlich im Raume verteilt sei. Nach der hier ins Auge zu fassenden Annahme ist bei Ausbreitung eines von einem Punkte ausgehenden Lichtstrahles die Energie nicht kontinuierlich auf größer und größer werdende Räume verteilt, sondern es besteht dieselbe aus einer endlichen Zahl von in Raumpunkten lokalisierten Energiequanten welche sich bewegen, ohne sich zu teilen und nur als Ganze absorbiert und erzeugt werden können."

[8] The story of Planck's discovery has often been told, see, e.g., Giulini's [78] highly readable account.

grandfather of quantum theory, had already concluded that black-body radiation[9] could be described by an energy density function $\rho(\nu, T)$ dependent on temperature T and radiation frequency ν, but independent of the material. The only task left for future physicists was to find said energy density function – as it turned out, this was a very difficult and seemingly interminable task. Planck, too, dedicated himself to it. Finding a solution meant letting go of some of his valued convictions and distancing himself from significant parts of his prior research. He had no choice but to include statistical arguments into his search for the energy function that were brought forward by his colleague and rival in Vienna, Ludwig Boltzmann. Planck had been generally sceptical towards atomism and had seen no significant role for statistics in physical theories. Now, he was forced to completely readjust his views. Planck used simple oscillators ('resonators') to model the behaviour of the cavity walls. This made sense, as the radiation is independent from the nature of the walls, and it made calculations easier. He took a detour and made use of entropy for his calculations. Planck knew how the radiational energy was related to the resonator's mean energy, but he had no idea what the resonator's energy looked like. What he did have, was an idea how to calculate the resonators' entropy, namely using Boltzmann's definition of entropy as the number of real microstates corresponding to a given macrostate. In this particular case, the total energy E needed to be distributed to the individual oscillators. Continually distributed energy would result in an infinite number of real microstates. An obvious absurdity. Planck therefore took a heuristic approach and postulated the existence of a minimal energy value, thus achieving a finite number of real microstates. The key part of his lecture was [133, p. 239]:

> If E is considered to be a continuously divisible quantity, this distribution is possible in infinitely many ways. We consider, however – this is the most essential point of the whole calculation – E to be composed of a very definite number of equal parts and use thereto the constant of nature $h = 6,55 \cdot 10^{-27}$[erg × sec]. This constant multiplied by the common frequency ν of the resonators gives us the energy element ϵ in erg, and dividing E by ϵ we get the number P of energy elements which must be divided over the N resonators.[10]

So this is where the energy quantum used by Einstein in 1905,

$$\epsilon = h\nu, \tag{1.1}$$

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[9] The black-body radiation is characterised by a certain energy distribution across all frequencies called energy spectrum.

[10] "Wenn E als unbeschränkt teilbare Grösse angesehen wird, ist die Verteilung auf unendlich viele Arten möglich. Wir betrachten aber – und dies ist der wesentlichste Punkt der ganzen Berechnung – E als zusammengesetzt aus einer ganz bestimmten Anzahl endlicher gleicher Teile und bedienen uns dazu der Naturconstanten $h = 6,55 \cdot 10^{-27}$[erg × sec]. Diese Constante mit der gemeinsamen Schwingungszahl ν der Resonatoren multiplicirt ergiebt das Energieelement ϵ in erg, und durch Division von E durch ϵ erhalten wir die Anzahl P der Energieelemente, welche unter die N Resonatoren zu verteilen sind."

appeared first. Honouring Planck's work, the constant h was later named Planck's constant. Using the above considerations, Planck went on to introduce his famous energy density formula of black-body radiation.

Although Einstein mentioned Planck's formula in his 1905 paper, he followed a completely independent line of reasoning to conclude his light quantum hypothesis. He started by considering Wien's law of radiation, which, though not valid for all frequencies, adequately describes the observed energy density for high frequencies. Einstein considered the radiation inside of a cavity that is in equilibrium with charged oscillators in the walls at a temperature T. He found that the entropy according to Wien's law had the same form as the entropy of an ideal gas. He went on to show that this entropy could be interpreted using the statistics proposed by Boltzmann. In fact, when applying Boltzmann's equation, the radiation entropy can be formulated directly as the entropy of a gas consisting of particles of energy ϵ. Einstein thus concluded [51, p. 143]:

> Monochromatic radiation of low density (within the range of validity of Wien's radiation formula) behaves thermodynamically as if it consisted of mutually independent energy quanta of magnitude $h\nu$.[11]

Einstein thereafter applied his light quantum hypothesis to the interaction of light and matter and showed how elegantly it explains the photoelectric effect. This paper, in particular, highlights Einstein's creative genius.[12] In a follow-up paper in 1906, Einstein took a clear position on Planck. He showed how Planck had implicitly used the light quantum hypothesis in his derivation and that the resonators' energy is a multiple of $h\nu$. Einstein wrote [52, p. 203]:

> In my opinion the above considerations do not at all disprove Planck's theory of radiation: rather, they seem to me to show that with his theory of radiation Mr. Planck introduced into physics a new hypothetical element: the hypothesis of light quanta.[13]

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[11] "Monochromatische Strahlung von geringer Dichte (innerhalb des Gültigkeitsbereiches der WIENschen Strahlungsformel) verhält sich in wärmetheoretischer Beziehung so, wie wenn sie aus voneinander unabhängigen Energiequanten von der Größe $h\nu$ bestünde." Instead of $h\nu$ Einstein used the equivalent expression $R\beta\nu/N$. It should also be pointed out that these independent energy quanta are *not* yet what would later be called photons, objects obeying the Bose-Einstein statistics; those are not independent.

[12] "Every one is agreed that genius is entirely opposed to the *spirit of imitation*." (I. Kant, *Critique of Judgment*, § 47: "Darin ist jedermann einig, daß Genie dem *Nachahmungsgeiste* gänzlich entgegenzusetzen sei." English translation by J. H. Bernard, 1914.)

[13] "Die vorstehenden Überlegungen widerlegen nach meiner Meinung durchaus nicht die Plancksche Theorie der Strahlung; sie scheinen mir vielmehr zu zeigen, daß Hr. Planck in seiner Strahlungstheorie ein neues hypothetisches Element – die Lichtquantenhypothese – in die Physik eingeführt hat."

Planck, for his part, did not want to accept this hypothesis; at least in the non-interacting case, he had never doubted Maxwell's equations with their continuous quantities. Planck was not alone in rejecting the light quantum hypothesis – the majority of physicists reacted the same way. The reason, of course, was the hypothesis' incompatibility with Maxwell's equations. These equations were able to correctly describe numerous phenomena; physicists trusted in them. And didn't the observed phenomena of wave interference contradict the description of light as consisting of particles? Einstein knew this all too well, which is why he qualified his paper as "very revolutionary" in the above-quoted letter. Of course, he did not doubt that Maxwell's equations provided a (very satisfying) approximative validity on a macroscopic scale. Indeed, this approximative validity was crucial for the theory of special relativity he had developed the same year.

Several years were to pass before the light quantum hypothesis became generally accepted. American physicist Robert Millikan was able to measure the photoelectric effect with very high precision in 1916, the results confirming Einstein's hypothesis. Yet it took until 1923 and Arthur Compton's experiments on the effect later named after him for the criticism on light quanta to die down. The magnitude of the Compton effect, i.e., the scattering of light and electrons, can only be explained by assuming a discrete nature of light. In a last attempt to avoid the light quantum hypothesis in 1924, Bohr, Kramers, and Slater even tried to allow for a violation of the law of energy conservation on the microscopic scale – a futile attempt, as became clear very soon. Under the name *photon*, which was introduced into physics in 1926, the light particle is one of the central concepts of physics today. It is an ironic twist of history that Einstein received his 1921 Nobel prize (awarded in 1922) mainly for his light quantum hypothesis and not for his theory of relativity.

The inability of either classical concept, wave or particle, to explain all optical phenomena on its own was made obvious by Einstein in 1909. In his work "On the present status of the radiation problem" he calculated the fluctuations of the black-body radiation energy in a small frequency interval between ν and $\nu + \Delta\nu$ and a small volume V. He found

$$(\Delta E)^2 = h\nu E + \frac{c^3}{8\pi\nu^2\Delta\nu}\frac{E^2}{V}\,, \tag{1.2}$$

where E is the mean value of the energy in this volume and this frequency interval, and c is the velocity of light. The first term appearing on the right-hand side is a direct consequence of the existence of light quanta of energy $h\nu$; the second term is a prediction of classical electrodynamics (Maxwell's equations). The first term thus corresponds to a particle point of view, the second to a wave point of view. Both are needed to obtain the correct result. This is the origin of *wave–particle duality*, a heuristic principle that played a pivotal role in the development of quantum theory, considerably influenced Bohr's reaction to EPR's work in 1935, and continues to be subject of debate even today.[14]

In the days of the 'old quantum theory' from 1900 to 1925, Einstein completed several more remarkable papers on this theory. However, these papers are of little consequence for our discussion of EPR's work. Already in 1907, he had considered the energy quantisation of the oscillators in a solid body in order to calculate the specific heat of said body. At low temperatures, his calculations yielded results that deviated significantly from the results in classical physics when applying the Dulong-Petit law. These deviations were confirmed experimentally by Walther Nernst in 1911. Nernst noted: "Without a doubt, the entirety of these observations is a striking confirmation of Planck's and Einstein's quantum theory."[15] Further papers contain an alternative derivation of Planck's radiation formula by considering emission and absorption of light by atoms (1917), a discussion of generalised quantisation conditions (1917), and a contribution to derive the later named Bose-Einstein statistics applying to bosons, i.e., to particles with integer spin values (1924/25). Accounts of these works are given, e.g., by Pais [122] and Pauli [126]. Einstein's work on the Bose-Einstein statistics had a significant influence on his subsequent general approach to quantum theory, as pointed out by Don Howard. The validity of the statistics unambiguously ascertains that photons (and any 'particle' or molecule) are no classical particles; as they are not statistically independent.[16] In his second paper on the subject Einstein wrote:

> It is easily recognised that by this calculation approach the distribution of molecules over the cells is not treated as statistically independent. [. . .] Consequently, the formula indirectly expresses a certain hypothesis about an initially completely puzzling mutual influence of the molecules [. . .]. [17]

The mentioning of a "completely puzzling mutual influence" might well be the first hint at some kind of action at a distance contained within the quantum mechanical formalism. The EPR paper would be about excluding action at a distance. In fact, in this context talking about action at a distance only makes sense when thinking in terms of (distinguishable) particles and not in terms of wave packets, which seemed to be the case with Einstein in 1925. Otherwise, in order to avoid action at a distance, he would have

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[14] For an account of wave–particle duality and its fate in the completed quantum theory, cf. Zeh [180].

[15] Translated from Stachel [153, p. 196].

[16] Einstein wrote to Schrödinger in February 1925: "According to Bose, the molecules sit together relatively more frequently than according to the hypothesis of the molecules being statistically independent." (The Collected Papers of Albert Einstein vol. 14, Doc. 447).

[17] "Daß bei dieser Rechnungsweise die Verteilung der Moleküle unter die Zellen nicht als eine statistisch unabhängige behandelt ist, ist leicht einzusehen. . . . Die Formel drückt also indirekt eine gewisse Hypothese über eine gegenseitige Beeinflussung der Moleküle von vorläufig ganz rätselhafter Art aus [. . .]" [55, p. 6].

had to conclude that photons actually are not particles but (as proposed by Planck) energy quanta of the field, whose states do not vary under permutations.

After 1925, Einstein did not take part in the formal development of quantum theory but rather accompanied this development with conceptual critique. This critique might well have originated in his work on the Bose-Einstein statistics.

To conclude this section we would like to direct our attention to a remark Einstein once made concerning the relation of gravitation and quantum theory. After completing his theory of general relativity in 1915, Einstein soon (in 1916) realised that his new theory allowed for the existence of gravitational waves, in close analogy to electromagnetic waves. In the classical model of the atom, where electrons speed around a core, the emission of electromagnetic waves would lead to an instability, as the electrons would crash into the core when losing the emitted energy. Quantum theory modifies the classical model and prevents the instability. The same should work for gravitational waves. This is what Einstein wrote on the subject:

> Nevertheless, due to the inner atomic movement of electrons, atoms would have to radiate not only electromagnetic but also gravitational energy, if only in tiny amounts. As this is hardly true in nature, it appears that quantum theory would have to modify not only Maxwellian electrodynamics, but also the new theory of gravitation.[18]

This is the first published reference to the need of a quantum gravity theory, see Chap. 6.

1.2 Interpretations of Quantum Theory Before 1935

The currently accepted formalism of quantum mechanics was essentially developed between 1925 and 1927. The intensity and creativity of this development was extraordinary. Besides two well established scientists, Max Born and Erwin Schrödinger, a group of very young physicists were the driving force: Werner Heisenberg, Wolfgang Pauli, Paul Dirac, and Pascual Jordan.

The first version of quantum mechanics, which is referred to as matrix mechanics, was quite abstract. Austrian physicist Erwin Schrödinger then put forward another formulation fully equivalent to matrix mechanics, as became evident soon. In 1923, French physicist Louis de Broglie had extended Planck and Einstein's quantum hypothesis to all forms of matter. According to de Broglie, each particle is assigned a specific frequency and wavelength, also referred to as de Broglie wavelength. The frequency ν is related to

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[18] "Gleichwohl müßten die Atome zufolge der inneratomischen Elektronenbewegung nicht nur elektromagnetische, sondern auch Gravitationsenergie ausstrahlen, wenn auch in winzigem Betrage. Da dies in Wahrheit in der Natur nicht zutreffen dürfte, so scheint es, daß die Quantentheorie nicht nur die Maxwellsche Elektrodynamik, sondern auch die neue Gravitationstheorie wird modifizieren müssen." [54, p. 696]

the particle's energy via equation (1.1); the de Broglie wavelength and the particle's momentum p are related through:

$$p = \frac{h}{\lambda}. \tag{1.3}$$

After giving lectures on de Broglie's work in Zürich, Schrödinger began searching for an equation governing the de Broglie's matter waves. He succeeded in finding this equation during a winter holiday in the Swiss Alps in 1925/1926, accompanied by his mistress who remains unknown to this day. This equation is one of the most famous equations of the twentieth century and bears the name of its discoverer. The Schrödinger equation reads

$$\mathrm{i}\hbar\frac{\partial \Psi}{\partial t} = H\Psi. \tag{1.4}$$

On the left hand side we have the imaginary unit i, Planck's constant in the now commonly used form $\hbar = h/2\pi$, and the partial derivative of the wave function Ψ with respect to time t. The wave function allows for the description of all 'particles' on an atomic scale. On the right-hand side we have an operator H acting on the wave function; H is called the Hamilton operator or simply the Hamiltonian and is the quantum mechanical counterpart of classical energy.

The wave function featured in equation (1.4) will play a central role in the discussion of Einstein, Podolsky, and Rosen's paper. The function generally does *not* describe a wave in normal three-dimensional space. Instead, it is defined in a space of higher dimension called configuration space. Only in the case of one single particle this space has three dimensions, for two particles it has six dimensions, for three particles it has nine dimensions and so forth. Whether a quantum object in normal space has more of a particle-like or more of a wave-like character can be derived from its wave function Ψ in configuration space, see Sect. 5.4. For example, 'particles' are described by narrow wave packets.

Since Max Born's proposal in 1926, the wave function is generally interpreted as a probability amplitude function. In a "measurement' of a classical quantity like position or momentum, the square of the magnitude of Ψ is the probability of finding the measured value within a given interval. Defining what a measurement is and how it differs from other interactions is a crucial point in every discussion on how to interpret quantum theory.

The use of wave functions sets fundamental limits on the simultaneous measurability of quantities like position and momentum. Heisenberg expressed these limits in his famous uncertainty relations (sometimes referred to as indeterminacy relations) in 1927. This will be relevant in EPR's work.

There's a physicist who played a unique role in the history of quantum theory: Danish physicist Niels Bohr. His contributions to the formal development of quantum theory are limited to the so-called 'old quantum theory'. The term refers to the developments before 1925 primarily driven by heuristic notions and a tremendous pioneer spirit in search of a consistent and empirically successful theory of atomic phenomena. Next to the above-mentioned, groundbreaking works of Planck and Einstein, this era was defined by the contributions of Arnold Sommerfeld, Louis de Broglie, and most notably said Niels Bohr.[19]

Bohr earned his reputation from a trilogy of papers published in the British *Philosophical Magazine* in 1913. Following his PhD in 1911, Bohr had worked, *i.a.,* with Ernest Rutherford in Manchester, whose epoch-making experiments had shown that electrons in an atom are moving around a quasi point-like core with a positive electric charge. According to classical electrodynamics, the electrons would move in circular orbits, thereby emitting electromagnetic radiation, thus losing energy and falling onto the core – the stability of matter remains a mystery in classical physics. To assure the stability of atoms, Bohr ad hoc postulated the existence of discrete energy levels for the atomic electrons. His model allows for transitions ('quantum jumps') from one level to another, stating that a transition from a higher energy level E_2 to a lower energy level E_1 emits a light quantum according to Planck's formula (1.1):

$$E_2 - E_1 = h\nu.$$

Most notably, there is to be a stable state of lowest energy (the ground state), in which emission of light quanta is no longer possible. In his 1917 derivation of Planck's radiation formula, Einstein made extensive use of Bohr's ideas.

Bohr's model relies on heuristic notions as well. While it can very well be used to describe the spectrum of the hydrogen atom, its use for more complex atoms is limited. Bohr's consideration involved a heuristic principle that he would later call the *correspondence principle* [30, 97]). It states that the models of old quantum theory should correspond to classical physics insofar that the classical equations should be contained within them as limiting cases. This should be obvious, as we know classical physics to be valid within its scope of application. The derivation of the classical limiting case in full quantum theory will be shown in Sect. 5.4. In reference to probabilities that already appeared in the old quantum theory, Bohr later wrote [29]:

> The only guide in estimating such probabilities was the so-called correspondence principle which originated in the search for the closest possible connection between the statistical account of atomic processes and the consequences to be expected from classical theory [...].

Bohr made no contributions to the development of quantum theory from 1925 to 1927. But he is the central figure in the development of the Copenhagen interpretation of the theory. Its name stems from the fact that this interpretation emerged from a number

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[19] Jammer [97] gives an extensive and knowledgeable historic account of the 'old quantum theory'.

of debates on the topic, mainly between Bohr and Heisenberg, in Bohr's hometown Copenhagen. Bohr and the Copenhagen interpretation play an important role in the reception of the EPR paper; in particular, Bohr's notion of *complementarity*, that he would later put forward as a reply to the EPR paper. What does this notion contain and how is it related to the history of quantum theory?

According to Jammer [98, p. 91], Bohr's first thoughts on *complementarity* date back to autumn 1926. He made them public in Como (northern Italy) on September 16, 1927, when an illustrious group of physicists met in celebration of Como-born Alessandro Volta's death a hundred years earlier (Volta was buried in Como, too). Max Born, Louis de Broglie, Werner Heisenberg, Wolfgang Pauli, Max Planck, and Arnold Sommerfeld were present in Como. Of those who contributed significantly to the development of quantum theory, only Einstein and Ehrenfest were missing.

Bohr's Como lecture was later published in a Supplement to *Nature* on April 14, 1928.[20] Bohr's intention was to help "harmonise the different views, apparently so divergent, concerning this subject", as he wrote in the introduction to his lecture. He was, of course, referring to the sharply divided positions taken by the proponents of matrix mechanics, primarily Heisenberg, Pauli, and Born, and the inventor of wave mechanics, Schrödinger. Bohr began his lecture with the definition of his 'quantum postulate', which expresses the fundamental limits of classical concepts in quantum theory [26, p. 580]:

> [...] it seems, as we shall see, that [quantum theory's] essence may be expressed in the so-called quantum postulate, which attributes to any atomic process an essential discontinuity, or rather individuality, completely foreign to the classical theories and symbolised by Planck's quantum of action.
>
> This postulate implies a renunciation as regards the causal space-time and co-ordination of atomic processes.

What kind of renunciation was Bohr talking about? As long as a quantum system remains unobserved, Bohr said, a description in terms of space and time makes no sense due to the discontinuities required by the quantum postulate. Here, Bohr's thoughts were still guided by his old model of the atom and the discrete quantum jumps of the electrons. A space-time description, Bohr continued, is possible only when the system is made to interact with a second system serving as measurement apparatus. But then the concept of causality loses its meaning, because the interaction with an external (macroscopic) system inevitably leads to an uncontrollable disturbance of the system, thus rendering a causal description impossible. Bohr, then, introduced the idea of complementarity [26, p. 580] using it as an adjective:

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[20] The publication of Dutch, French and German versions followed. An extensive discussion of the Como lecture and its reception can be found in Jammer [98, p. 85–107] and Beller [17, chap. 6].

> The very nature of the quantum theory thus forces us to regard the space-time co-ordination and the claim of causality, the union of which characterises the classical theories, as complementary but exclusive features of the description [...].

Later, when talking about complementarity, it usually refers to the complementarity of describing quantum objects as particles or waves in the sense of the historical wave–particle duality (cf., e.g., Pauli [129, p. 31*f.*]). However, as Mara Beller [17, chap. 6] convincingly demonstrated, this meaning is not explicitly expressed in Bohr's Como talk. According to Beller, Bohr only talked about complementarity of causality and space-time description, which according to Bohr would be expressed, *i.a.,* in the uncertainty relations. Beller also claims that Bohr's aim was to prove the compatibility of his quantum postulate and the stationary electron states he postulated in 1913 with Schrödinger's wave mechanics. In his talk he spoke in favour of wave mechanics, which, as stated by Beller, might have been part of the reason for later disagreements with Heisenberg concerning the interpretation of quantum physics. At the end of his talk, Bohr said [26, p. 590]):

> In the quantum theory we meet this difficulty [of adapting our modes of perception borrowed from the sensations to the gradually deepening knowledge of the laws of Nature] at once in the question of the inevitability of the feature of irrationality characterising the quantum postulate. I hope, however, that the idea of complementarity is suited to characterise the situation, which bears a deep-going analogy to the general difficulty in the formation of human ideas, inherent in the distinction between subject and object.

Bohr's comment on the irrationality of the quantum postulate seems odd – should not science be governed by rational postulates only? Such remarks, however, are not out of character, as they often appear in his work.

Bohr was frequently criticised for the incomprehensibility of his propositions, which also is true for the Como talk. His incomprehensible way of presenting his ideas leaves room for different, even contradicting, interpretations. Interestingly enough, Bohr's devoted student Léon Rosenfeld[21] made the following remark about his mentor's way of work:[22]

> It was impressive to watch him thus at the height of his powers, in utmost concentration and unrelenting effort to attain clarity through painstaking scrutiny of every detail—true as ever to his favourite Schiller aphorism "Nur die Fülle führt zur Klarheit."[23]

Maybe Schiller erred.

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[21] John Bell called Rosenfeld a *consistent traditionalist* [15, p. 93].

[22] More specifically, the quote refers to how Bohr prepared his reply to the EPR paper, which we will discuss in detail further on.

[23] "Only abundance leads to precision." (Schiller, *The sayings of Confucius*)

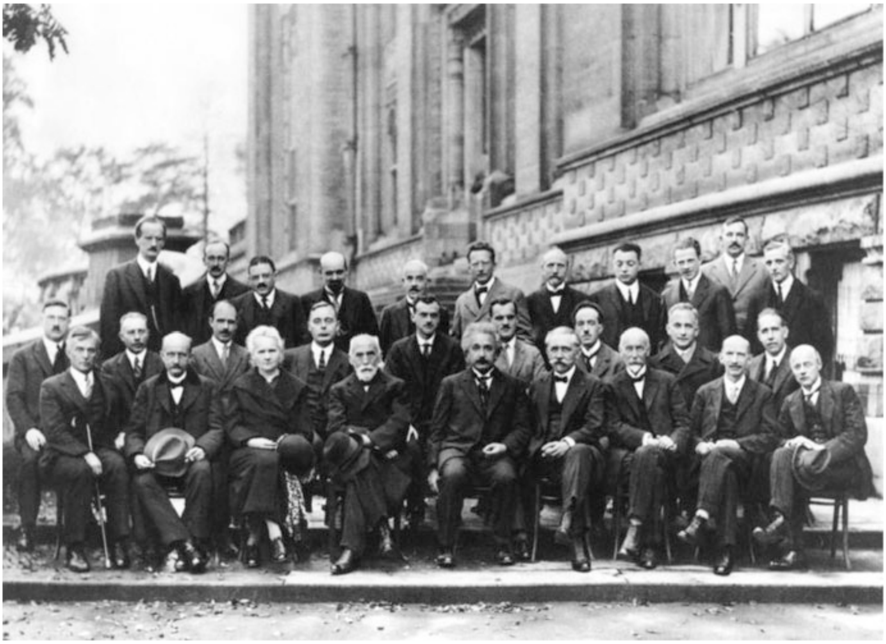

Fig. 1.1 The participants of the Solvay Conference in October 1927. (Photograph by Benjamin Couprie. Courtesy of the Solvay Institutes, Brussels)

1.3 The Bohr-Einstein Debate During the Solvay Conferences

One month after the Como conference, where Bohr had published his ideas on complementarity, probably the most famous conference in the history of quantum physics took place in Bruxelles, Belgium. It was the fifth edition of the Solvay Conference,[24] run by Hendrik Antoon Lorentz from 24–29 October 1927, bringing together the crème de la crème of quantum theory (Fig. 1.1). Amongst others, there were: Planck, Einstein, Bohr, Heisenberg, Born, Dirac, Schrödinger, de Broglie, and Pauli. Officially, the conference topic was *Electrons and Photons* (one year after the introduction of the term photon, meaning light quantum), but really, it was dedicated to the only recently completed quantum mechanics.[25]

In fact, the 1927 Solvay Conference played a pivotal role in two respects. On the one hand, it marked the completion of quantum theory's *formal* development. At the end of

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[24] Einstein had already participated in the first Solvay Conference in 1911, where the main topic was the early development of quantum theory. Cf., e.g., Straumann [154] for a worth reading account of Einstein's role.

[25] Bacciagaluppi and Valentini [5] offer an extensive presentation of this conference and an English translation of the *Proceedings* (originally mostly in French).

the conference summary, Born and Heisenberg proudly proclaimed quantum theory to be a completed theory, whose fundamental physical and mathematical assumptions could now remain unchanged. On the other hand, it sparked a debate concerning the correct interpretation of quantum theory that continues to this day. Since this debate is relevant for the discussion of the EPR paper, we will lay out the relevant contributions in some detail now.[26] At the center of the debate were the disputes between Bohr and Einstein that did not take place during the official program, but in between sessions. Bohr and Einstein discussed in the lobby (of hotel "Metropole") or while taking a walk, see further below.

In his conference lecture, Louis de Broglie, who had postulated the wave-like nature of matter and to whom we owe the relation (1.3) between momentum and wavelength, attempted to bring into line observations of localised particles with Schrödinger's wave mechanics and Born's probability interpretation of the wave function Ψ. To this end, he interpreted Ψ not only as a probability wave, but also as a *pilot wave*[27] guiding the particles. Herewith, he wanted to contribute to a deterministic theory of the atomic phenomena. De Broglie's pilot wave was not well received by the conference participants, especially Pauli fiercely criticised it. His only supporter was Einstein, who in an effort to establish a unified field theory attempted to describe particles as singularities of waves.[28] Because of the opposition, de Broglie decided not to pursue his work on pilot waves for the time being. Later, David Bohm was the one to pick up these ideas and develop them (Sect. 5.1). It should be emphasized, however, that de Broglie's work is the first example of a theory with hidden variables (see the remarks at the end of this chapter and later).

In the general discussions, Einstein made some remarks about the interpretation that are relevant for EPR's work (Bacciagaluppi and Valentini [5, pp. 440–442]). He presented two points of view on the probability density $|\Psi|^2$. In the first point of view, this quantity has a purely statistical meaning and can thus only be used to describe a whole ensemble of particles ("ensemble interpretation"). Post EPR, Einstein came to prefer this interpretation. Einstein thought the statistical interpretation of the wave function consistent with EPR's conclusion that quantum theory is incomplete. It can help to complete quantum theory by understanding *individual* processes, be it by a unified field theory such as the one Einstein was searching for or by looking for hidden variables, as was later the case.

In the second point of view, the wave function is interpreted individually. According to Einstein, only this interpretation would guarantee the conservation of energy and momentum in elementary processes. But, Einstein further states, it would not explain why $|\Psi|^2$ could be localised in a single point (e.g., of a photographic plate) and not in many, the way it should be when dealing with waves. In this localisation, Einstein sees an action at a distance in violation of the theory of relativity. This is why he sympathises with de Broglie's idea of pilot waves, which introduces an additional particle. The cause

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[26] A more detailed discussion was given by Jammer [98, pp. 109–158].

[27] French: "Onde pilote".

[28] This concept first appears in Einstein [53].

for unease concerning this second interpretation is the fact that $|\Psi|^2$ is not defined in an ordinary three-dimensional space but in a configuration space of higher dimension. Harvey Brown has pointed out that Einstein did not turn against the uncertainty relations in his published conference contributions; he rather presented an early version of EPR's arguments [32].

The only source available on the Einstein and Bohr debate about the consistency of quantum theory is Bohr's account published in a later anthology edited by Schilpp honouring Einstein [29]. In his contribution to the anthology, Bohr embedded a recount of the debate into the presentation of his notion of complementarity, which had undergone several changes in the follow-up of EPR's work. Though it probably does not accurately reflect the original spirit of his debate with Einstein in 1927 and 1930, it is, nonetheless, an important source to understand their diverging points of view. Yet, when recounting the 1930 debate in particular, it seems that Bohr misses the point of the debate [91, p. 91*ff.*]; [32]; we will return to this point further below.

Einstein had met Bohr for the first time when the latter visited Berlin in 1920. Back then, they already had diverging understandings of quantum theory, not least of the light quantum hypothesis. Bohr simply did not accept it at the time. (He would change his mind later in view of experimental evidence.) However, they were both impressed with each other and shared a deep mutual respect.[29] They continued their discussion in December 1925, when they met in Leiden, Netherlands. Paul Ehrenfest, the Austrian physicist who had been working there since 1912, mediated their meeting.

The Einstein and Bohr debates during the 1927 Solvay Conference cannot be understood without considering another event that year – the publication of Heisenberg's paper on the uncertainty relations in *Zeitschrift für Physik*, the leading physics journal at the time, on 23 March [86].[30] In this paper, Heisenberg demonstrated the existence of characteristic limits to the simultaneous measurements of position x and momentum p. This, of course, is related to the fact that classical trajectories no longer exist in quantum mechanics, as they would require assuming the simultaneous and exact knowledge of position and momentum. If Δp and Δx denote these uncertainties, Heisenberg's relation reads (in modern notation):

$$\Delta p \cdot \Delta x \geq \frac{\hbar}{2}. \tag{1.5}$$

---

[29] Cf. Jammer [98, p. 123].

[30] In this original paper, Heisenberg does not use the terms "Unschärfe" (only roughly translates to uncertainty; it rather means fuzziness) or "Unbestimmtheit" (indeterminacy), but uses "Ungenauigkeit" (inaccuracy).

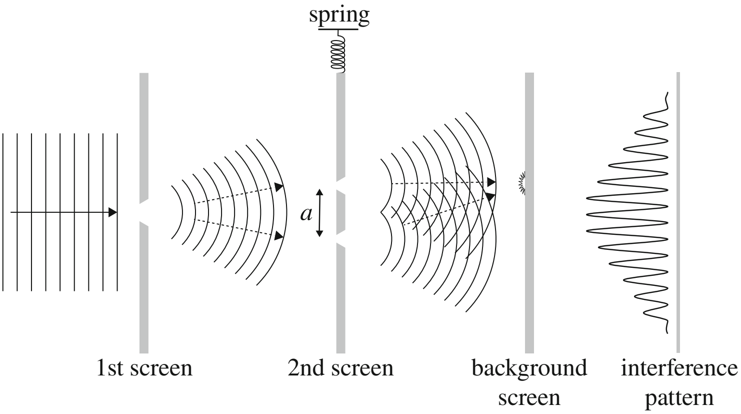

Fig. 1.2 Illustration of Einstein's thought experiment on the position-momentum-uncertainty

Once again it is Planck's constant $\hbar$ that is responsible for the fundamental limit. Heisenberg also presented an uncertainty relation between energy E and time t:

$$\Delta E \cdot \Delta t \geq \frac{\hbar}{2}. \tag{1.6}$$

Einstein knew of Heisenberg's paper early on, since Bohr sent him a manuscript in April 1927. It comes as no surprise that the paper caused Einstein much unease. If the knowledge of simultaneous values of position and momentum is, by principle, impossible, there is no room for a space-time description of particle trajectories, to which Einstein so firmly clung. He therefore brooded over thought experiments that should show ways to circumvent Heisenberg's relations. This was, what his debates with Bohr in the hotel Metropole were about.

Einstein discussed the uncertainty relation (1.5) between position and momentum.[31] In one of the thought experiments he considers two screens with narrow slits and a background screen (Fig. 1.2). A beam of particles with a given de Broglie wavelength λ encounters the first screen with one slit. It then encounters the second screen, with two slits (a double-slit), that is freely suspended on a spring. The slits are separated by a distance a. Because the particles passing the slits are described by a wave function, interference occurs and can be observed on the background screen (e.g., by blackening a photographic plate). The interference pattern consists of maxima and minima that are each separated by λ/a. The separation between maxima (or minima) therefore is a measure of wavelength, and from this the particles' momentum can be obtained via (1.3). Now that that's understood, Einstein argues as follows. Since the second screen is suspended on a

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[31] Countless summaries of this can be found in relevant literature, cf., e.g., Jammer [98, chap. 6]. However, all of them are ultimately based on Bohr's [29] account.

spring, it moves freely in the vertical direction. Through this motion, you can measure the transfer of momentum in the exact moment the particle passes the slit. This transfer, however, depends on which slit the particle passes through. In addition to the interference pattern (which, we recall, gives the particle's momentum), this would render the particle's exact position – contradicting the claim of Heisenberg's relation (1.5).

Initially, this argument caused Bohr quite some headache. After a sleepless night though, he had resolved the supposed contradiction with (1.5) and reported it to Einstein in the morning. The key was to apply the uncertainty relation (1.5) not only to the particle, but also to the second screen, i.e., to a macroscopic object. Bohr's solution was revolutionary, as it abolished the separation of micro- and macro-physics that had been considered a given until then. After the publication of EPR's work, he would no longer stand behind that point of view.

Bohr was now able to demonstrate how measuring the screen's momentum precisely enough to determine which slit the particle passed through, would destroy the interference pattern and the information on the momentum with it, thus saving the uncertainty relation. Einstein admitted his defeat, at least for the moment. Clearly, Einstein was still attached to a classical definition of a particle, wherein a particle really is some sort of small spherule that can only go through one of the slits. In quantum theory, however, all objects are described by a wave function; so the "particle" *does* go through both slits at the same time.

Even before Heisenberg found the uncertainty relations, it was clear that a classical point of view of a particle would not hold up consistently. Pauli, for example, had written the following, now famous lines to Heisenberg on October 19, 1926:

> It's always the same: due to scattering, beams cannot be arbitrarily small in the wave optics of the ψ-field [...]. You can either observe the world with your p-eye or with your q-eye. But if you try to open both eyes simultaneously, you go nuts.[32]

It is true that, in some cases, one can construct wave packets that closely follow a classical trajectory, but you can never construct the trajectory itself; such a trajectory simply does not exist in quantum theory.

In 2013, a group of German and French physicists [144] presented a modern realisation of Einstein and Bohr's thought experiment that fully takes into account the double slit quantum behaviour using an ionised hydrogen molecule (HD^+) as double slit. The scattered particles were helium atoms. The scientists succeeded in measuring the momentum of the scattered particles. In addition, they determined the orientation of the molecular double slit at the time of scattering. Yet this does not determine the trajectory of the helium atom, because only the molecule's orientation and the slit-width are of

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[32] "Es ist immer dieselbe Sache: es gibt wegen Beugung keine beliebig dünnen Strahlen in der Wellenoptik des ψ-Feldes [...] Man kann die Welt mit dem p-Auge und man kann sie mit dem q-Auge ansehen, aber wenn man beide Augen zugleich aufmachen will, dann wird man irre." ([127, English translation by S. Linden and A. K. Hudert])

relevance, the molecule's exact position in space is not; the latter is rendered uncertain by the scattering. This is why the scientists found a distinct interference pattern for the helium atoms.

This experiment obviously is in full agreement with the predictions of quantum theory. The goal was to confirm Bohr's point of view that the double slit needed to be described quantum mechanically.[33] Neither the uncertainty relations nor the notion of complementarity were part of the discussion. Einstein's wish to determine the trajectory of a particle by simultaneously measuring momentum transfer and interference pattern cannot be realised in quantum mechanics. The reason for this being, once again, that no such trajectory exists.

Such 'which-way' experiments have become somewhat of a tradition. Scully et al. [148] proposed an experiment that was supposed to render information on a particle's trajectory without actually disturbing it by an uncontrollable measurement. The interference pattern only disappears due to a correlation between the particle and the apparatus; the momentum transfer onto the particle can be kept arbitrarily small. Such experiments have been conducted at the University of Konstanz, Germany, in 1998, cf., e.g., Rempe [134]. The interference pattern was created by rubidium atoms scattering from a standing light wave. The scientists used rubidium atoms, because they have an external valence electron with a spin that can take on two different orientations with respect to the nuclear spin. It is this spin state that constitutes the measurement apparatus, as it can be entangled with the atom's momentum. The two trajectory options are then correlated to the two spin options. Once this entanglement is established, the information on the taken trajectory is contained physically in the electron's spin state, and the interference pattern vanishes. In order for this to happen, the information does not have to be "read"; the entanglement alone suffices.

Discussions on how to interpret the uncertainty relation (1.5) have not ceased. Time and again, someone claims that a thorough discussion of the relation between a measurement and the disturbance of the measured system caused by this measurement leads to inequalities that violate (1.5). That the arguments leading to this alleged violation are untenable is shown in Busch et. al. [36].

It was Einstein, Podolsky, and Rosen's publication in 1935 that shifted the focus away from considering direct disturbances of a system and asserting a central role to the uncertainty relations towards the formation of an entanglement of two systems. Before turning to this paper, however, let us talk about the last time Bohr and Einstein intensively discussed the uncertainty relations. The dispute took place, once again, in Brussels, during the sixth Solvay Conference on *magnetism* in October 1930. This time, their discussion focused on the uncertainty relation (1.6) between energy and time and contained thoughts that would later be involved in EPR's arguments, as has been shown conclusively by Howard [91] and Whitaker [166].

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[33] "[...] the double slit is part of the quantum mechanical system and has to be treated accordingly." [144]

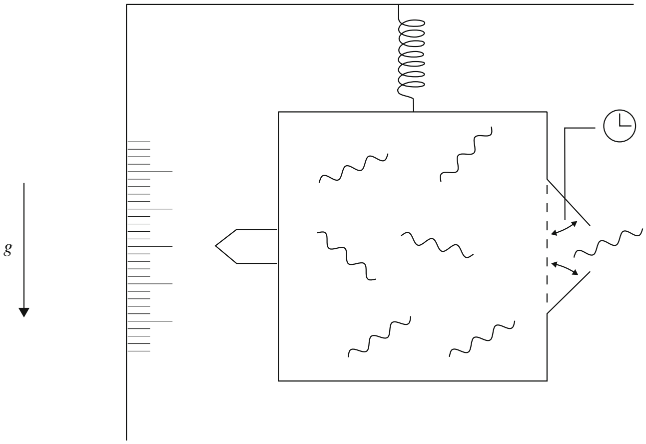

Fig. 1.3 Illustration of Einstein's thought experiment on the energy-time-uncertainty

What exactly did Bohr and Einstein discuss at the 1930 Solvay Conference? As before, the only source on the disputes remains Bohr's account in Schilpp's publication [29]. According to this account, Einstein considered a box containing electromagnetic radiation with an opening in one of the walls of the box, often called *Einstein's box* or *photon box* (Fig. 1.3). A clock controls the shutter of the opening so that exactly one single photon can escape from the box in a given time t. Yet the photon's energy E is also known, as it can be deduced from weighing the box before and after the photon escaped; which is achieved by suspending the box on a spring. It looks like a contradiction of Heisenberg's uncertainty relation (1.6) between energy and time.

Bohr answered as follows. The balanced state position of the box shall be known with an uncertainty Δq. According to (1.5) this leads to an uncertainty of momentum $\Delta p \sim \hbar/\Delta q$. Bohr assumes that Δp must be smaller than the amount of momentum that is transferred to the box's mass uncertainty Δm by the gravitational field during the weighing process; because otherwise, no reasonable weighing would be possible. If T denotes the duration of the weighing process, g the box's acceleration due to earth's gravity and v its velocity, this means:

$$\Delta p < v\Delta m = gT\Delta m \,. \tag{1.7}$$

Ironically, Bohr then brought into play Einstein's own theory, namely the theory of general relativity, according to which the rate of a clock depends on its position in a gravitational field. So there is an uncertainty ΔT associated with the uncertainty Δq, the exact relation being

$$\frac{\Delta T}{T} = \frac{g\Delta q}{c^2} \,. \tag{1.8}$$

By use of inequality (1.7), the uncertainty ΔT after the weighing procedure is then given by

$$\Delta T = \frac{g\Delta q}{c^2} T > \frac{\hbar}{\Delta m c^2} = \frac{\hbar}{\Delta E}\,, \tag{1.9}$$

in full agreement with (1.6). The general understanding of this discussion is that Bohr beat Einstein at his own game.

Was it really all about the uncertainty relation? Ehrenfest wrote a letter to Bohr on July 9, 1931, giving a different perspective. He had just seen Einstein in Berlin and told Bohr:[34]

> He [Einstein] said to me that, for a very long time already, he absolutely no longer doubted the uncertainty relations, and that he thus, e.g., had BY NO MEANS invented the 'weighable light-flash-box' (let us call it simply L-F-box) 'contra uncertainty relation,' but for a totally different purpose.[35]

Ehrenfest went on to explain Einstein's true intention. According to Ehrenfest, Einstein considered a "machine" emitting a projectile. Once the projectile is far out (Ehrenfest refers to half a light-year), a measurement is taken on the machine that allows to predict either the quantity A or the quantity B on the projectile, where A and B can correspond to non-commuting operators (which in quantum mechanics means that the quantities cannot be measured simultaneously). The projectile shall be reflected at an astronomical distance and return to the observer after a long period of time. That is when, quantity A and quantity B can be measured, obviously not simultaneously. In his letter, Ehrenfest comments on this:

> It is interesting to get clear about the fact that the projectile, which is already flying around isolated 'for itself,' must be prepared to satisfy very different 'non-commutative' predictions, 'without knowing as yet' which of these predictions one will make (and test).[36]

The photon box serves this purpose, if the photon plays the role of the projectile and the quantities A and B are the photon's time of return and its energy (or frequency).

The passage from Ehrenfest's letter quoted above somewhat resembles the thought experiment on 'delayed choice' presented much later by John Wheeler (cf., e.g., the

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[34] For the following, also see the detailed discussion in Howard [91].

[35] "Er sagte mir, dass er schon sehr lange absolut nicht mehr an der Unsicherheitsrelation zweifelt und dass er also z. B. den "waegbaren Lichtblitz-Kasten" (lass ihn kurz L-W-Kasten heissen) DURCHAUS nicht "contra Unsicherheits-Relation" ausgedacht hat, sondern für einen ganz anderen Zweck." [91, p. 98])

[36] "Es ist interessant das Projectil, das da schon isoliert 'für sich selber' herumfliegt darauf vorbereitet sein muss sehr verschiedenen "nichtcommutativen" Prophezeiungen zu genügen, 'ohne noch zu wissen' welche dieser Prophezeiungen man machen (und prüfen) wird." [91, p. 99]

discussion in Kiefer [105], p. 92*f.*). It deals with an apparent paradox resulting from thinking of the photon as a classical spherule rather than as a wave.

While staying in California, United States, from December 1930 to March 1931, Einstein worked with Boris Podolsky and Richard Tolman on fundamental aspects of quantum mechanics. This collaboration resulted in the above mentioned paper on “Knowledge of Past and Future in Quantum Mechanics” [67]. In this paper, they demonstrated that the past behaviour of a particle cannot be determined more precisely than its future behaviour. The contrary of this had been suggested in the literature, but given the time-reversal symmetry of Schrödinger’s equation this result should not come as a surprise. For our purposes though, it is important to mention a different aspect of the paper. It contains a slightly modified version of the photon-box thought-experiment, where the authors do not consider the correlation between the box and the photon, but between two photons emitted from the box. As Howard pointed out, this version can be used to predict the second photon’s time of return *or* its energy (frequency) through alternative measurements on the first photon. The second photon shall be so far out that it cannot in any way be influenced by the measurement taken on the first photon. The experiment works on the assumption of separability, which was of pivotal interest to Einstein. This assumption explains the motivation for EPR’s work and the following debate. Bohr, in particular, will right out reject the assumption that systems that have interacted in the past could be dealt with separately.

Before turning to the EPR paper from 1935, we now want to complete the backstory by introducing an important contribution by the mathematician John von Neumann.

1.4 John von Neumann and the Wave Function Collapse

John von Neumann (1903–1957) was one of the most famous mathematicians of the twentieth century. He contributed substantially to the completion of quantum theory’s mathematical formalism; it was von Neumann who emphasised that the state of a system is described by a vector in a Hilbert space, and that the wave function Ψ is only a special way of representing that vector. In his classic book *Mathematische Grundlagen der Quantenmechanik*[37] from 1932 [159], he managed to express the complete mathematical formalism in a way that the basics of it are still taught in most university courses today.[38]

In his book, von Neumann was the first to discuss the fact that two very different dynamics are being used in quantum theory [159, p. 186*ff.*]. One is the time evolution of states as described by Schrödinger’s equation; this dynamics is applied to isolated

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[37] “Mathematical Foundations of Quantum Mechanics”.

[38] “For the time being, I am trying to delve into Neumann’s book. He truly is the brightest of them all.” (“Vorläufig versuche ich, etwas tiefer in von Neumanns Buch einzudringen. Er ist doch der Schärfste von allen.” Born in a letter to Schrödinger, June 28, 1935, cf. von Meyenn [158].)

systems. The other dynamics occurs when the system in question interacts with an external observer (measurement apparatus); of all components of the wave function, this dynamic sorts out all those that correspond to the observed results of the measurement. The sort-out, however, needs to be done by hand, there are no equations for it. Only recently, attempts have been made to formulate equations for this dynamics, although it is not completely clear if such a dynamics is needed at all. We will get back to this later.

Interestingly, von Neumann spoke of Schrödinger's dynamics as the "second intervention" (although no one really intervenes at this point), and the dynamics while measuring as the "first intervention". Later, von Neumann's first intervention was mostly referred to as the wave function collapse or wave function reduction. Von Neumann also highlighted the important fact that Schrödinger's equation is time reversible, but the reduction is not. In the reduction, the state non-causally transforms into another according to Born's probability interpretation, where the resulting state is the "observed" one. This is why in his book, von Neumann also covers thermodynamics and the increase of entropy as expressed in the second law of thermodynamics.

Heisenberg had already spoken of such a reduction, but without further detailing his thoughts. This is what Pauli wrote about Heisenberg's thoughts on reduction in a letter to Bohr on October 17, 1927 [127, p. 411]:

> This is a part that was not entirely satisfactory in the Heisenberg paper; the 'reduction of the wave packet' seemed a bit mysterious.[39] It needs to be emphasised that such reductions are not necessary at first, when all the measurement apparatuses are included in the system. However, in order to be able to theoretically describe any observations, it is necessary to ask what can be said about a *part* of the whole system on its own. Then, when interpreting the complete solution, it is pretty clear that, indeed, omitting the means of observation can in many cases (not always, for sure) be formally replaced by such reductions.[40]

With some imagination, the last two phrases can be interpreted as the central idea of decoherence (see Sect. 5.4), a concept that, after 1970, eliminated significant parts of the mysticism surrounding the 'reduction of the wave packets'. Pauli himself, however, could

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[39] Also cf. the following extract from a letter from February, 1927 (emphasis by Heisenberg): "It seems to me that one can now concisely express the solution: *The trajectory is created the moment we observe it.*"("Die Lösung kann nun, glaub' ich, prägnant durch den Satz ausgedrückt werden: *Die Bahn entsteht erst dadurch, daß wir sie beobachten.*)" [127, p. 379].

[40] "Dies ist ja gerade ein Punkt, der bei Heisenberg nicht ganz befriedigend war; es schien dort die 'Reduktion der Pakete' ein bißchen mystisch. Nun ist ja zu betonen, daß solche Reduktionen zunächst nicht nötig sind, wenn man alle Messungsmittel *mit* zum System zählt. Um aber Beobachtungsresultate überhaupt theoretisch beschreiben zu können, muß man fragen, was man über einen *Teil* des ganzen Systems allein aussagen kann. Und dann sieht man der vollständigen Lösung von selbst an, daß die Fortlassung des Beobachtungsmittels in vielen Fällen (nicht immer natürlich) formal durch derartige Reduktionen ersetzt werden kann." (English translation by S. Linden and A. K. Hudert)

not interpret his own words in that sense, as he was missing the dynamical role of the environment, which is much needed for decoherence.

Von Neumann described the measurement process as dynamically as possible, meaning that he decided to ascribe quantum states to measurement apparatuses and observers. Bohr would never have thought of such a thing. According to his idea of complementarity, which he modified as a reaction to EPR's work, a measurement apparatus always needs to be described in classical terms (cf. Sect. 4.2). But the description of measurement apparatuses by means of quantum states provides an important contribution to understanding the classical limit (Sect. 5.4).

The central principle of quantum theory is the superposition principle (also cf. the appendix). According to this principle, quantum states can be added together ('superposed') and the result will be another valid quantum state. In general, the resulting state will not have a meaningful classical interpretation. In his famous textbook, Paul Dirac comments on this as follows [49, p. 12]:

> The nature of the relationships which the superposition principle requires to exist between the states of any system is of a kind that cannot be explained in terms of familiar physical concepts. One cannot in the classical sense picture a system being partly in each of two states and see the equivalence of this to the system being completely in some other state. There is an entirely new idea involved, to which one must get accustomed and in terms of which one must proceed to build up an exact mathematical theory, without having any detailed classical picture.

Now, if you describe the measurement apparatus with quantum mechanical states as von Neumann did, the superposition principle will hold for those states, too. The formal result can be superpositions of macroscopically different pointer positions. Obviously, one cannot actually observe such non-classical states: This is what Schrödinger expressed so aptly with his cat example (see Sect. 4.3). Von Neumann was well aware of this problem. It is noteworthy that von Neumann made the final observer's consciousness responsible for the vanishing of the superposition via the 'first intervention' (the wave function collapse). After having mentioned the two different dynamics at work (reversible Schrödinger equation and irreversible collapse), he wrote [159, p. 223]:

> Let us now compare these circumstances with those which actually exist in nature or in its observation. First, it is inherently entirely correct that the measurement or the related process of the subjective perception is a new entity relative to the physical environment and is not reducible to the latter. Indeed, subjective perception leads us into the intellectual inner life of the individual, which is extra-observational by its very nature [...]. Nevertheless, it is a fundamental requirement of the scientific viewpoint – the so-called *principle of the psycho-physical parallelism* – that it must be possible so to describe the extra-physical process of the subjective perception as if it were in reality in the physical world [...].[41]

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[41] "Vergleichen wir nun diese Verhältnisse mit denjenigen, die in der Natur bzw. bei ihrer Beobachtung wirklich bestehen. Zunächst ist es an und für sich durchaus richtig, daß das

Von Neumann then went on to show that formally it does not matter where the cut of the "first intervention" occurs, as long as it occurs in the macroscopic realm; it is of no importance if that boundary lies inside the measurement apparatus or inside the 'actual observer' (as von Neumann put it). Later, the physicist Eugene Wigner (1902–1995), Hungarian as was von Neumman, also ascribed a similar role to the consciousness and gave it up only after the concept of decoherence became clear to him (Sect. 5.4). London and Bauer [113] shared a similar view.

One other topic from von Neumann's book became of importance for the debate on how to interprete quantum theory – the 'proof' of the impossibility of hidden variables. Hidden variables mean variables that, in a hypothetical consideration of quantum theory, complement the wave function in a way that it would, e.g., allow for a simultaneous determination of location and momentum, thus circumventing the uncertainty relations. Proving the impossibility of such hidden variables would be equivalent to proving the completeness of quantum theory. Von Neumann's 'proof' is directly related to Einstein, Podolsky, and Rosen's work, which dealt with exactly that completeness of quantum theory; even though von Neumann is not mentioned in the EPR paper. Only later it became clear that von Neumann's proof was not applicable to reality (Sect. 5.2), as some of his assumptions were too narrow. Anyway, the stage is now set for EPR's work, which was about to disturb the peace that had begun to reign over the debate on how to interpret quantum theory.

Messen, bzw. der damit verknüpfte Vorgang der subjektiven Apperzeption eine gegenüber der physikalischen Umwelt neue, auf diese nicht zurückführbare Wesenheit ist. Denn sie führt aus dieser hinaus, oder richtiger: sie führt hinein, in das unkontrollierbare, weil von jedem Kontrollversuch schon vorausgesetzte, gedankliche Innenleben des Individuums [...]. Trotzdem ist es aber eine für die naturwissenschaftliche Weltanschauung fundamentale Forderung, das sog. Prinzip vom psychophysikalischen Parallelismus, daß es möglich sein muß, den in Wahrheit außerphysikalischen Vorgang der subjektiven Apperzeption so zu beschreiben, als ob er in der physikalischen Welt stattfände [...]." (English translation by Robert T. Beyer, p. 418–419 of his 1955 translation of von Neumann's book.)

The Einstein, Podolsky, and Rosen Paper

2

A. Einstein, B. Podolsky, and N. Rosen, Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?,
Physical Review, **47**, 777–780 (1935).

2.1 Reprint of the Paper

Can Quantum-Mechanical Description of Physical Reality Be Considered Complete? A. Einstein, B. Podolsky, and N. Rosen, Physical Review, Volume 47, Page 777–780, published in 1935 by the American Physical Society. Reprinted with permission https://doi.org/10.1103/PhysRev.47.777

2.2 Critical Summary

The paper by Einstein, Rosen and Podolsky, that we shall call the EPR paper in the following, is not long. It is a little less than four pages long and does not contain any references to other publications. It was submitted to *Physical Review* on March 25, 1935, and published on May 15, 1935.

Besides an introduction, the EPR paper consists of two chapters without heading. In the first part, the authors claim that there is a difference between an assumed objective reality and its description through a physical theory:

> Any serious consideration of a physical theory must take into account the distinction between the objective reality, which is independent of any theory, and the physical concepts with which the theory operates.

C. Kiefer (ed.), *Albert Einstein, Boris Podolsky, Nathan Rosen*, Classic Texts
in the Sciences, https://doi.org/10.1007/978-3-030-47037-1_2

They continue to stress that the success of a theory should be determined by asking the following questions. First: Is the theory correct? Second: Is the description given by the theory complete? To answer the first question, the authors refer to the agreement with measurement outcomes; this question is not going to be the subject of their paper. The EPR paper focusses on the question of completeness, as implied in the paper's title.

What is completeness supposed to mean? Quantum mechanical states are described through wave functions (more generally: vectors in Hilbert space). If this description is complete, i.e., if no further ('hidden') variables exist that would allow for a simultaneous determination of, e.g., position and momentum, then, the theory shall be called complete.

EPR propose the following, necessary criterion for completeness, which according to them seems unavoidable:

> [...] every element of the physical reality must have a counterpart in the physical theory.

In a letter to Schrödinger on June 19, 1935, Einstein wrote more specifically:

> In quantum mechanics, one describes a real state of affairs of a system by means of a normed function Ψ of the coordinates (of configuration space). The temporal evolution is uniquely determined by the Schrödinger equation. One would now very much like to say the following: Ψ stands in a one-to-one correspondence with the real state of the real system. The statistical character of measurement outcomes is exclusively due to the measurement apparatus, or the process of measurement. If this works, I talk about a complete description of reality by the theory. However, if such an interpretation doesn't work out, then I call the theoretical description 'incomplete'.[1]

In order to apply this criterion one needs to know what elements of physical reality are. Then, according to the authors, the question of completeness can be easily answered. On these grounds they present a criterion that they consider sufficient for their purpose. It is this criterion of reality that would cause much concern and many misunderstandings later on. It goes like this (italics by EPR):

> *If, without in any way disturbing a system, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity.*

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[1] "Man beschreibt in der Quantentheorie einen wirklichen Zustand eines Systems durch eine normierte Funktion ψ der Koordinaten (des Konfigurationsraumes). Die zeitliche Änderung ist durch die Schrödinger-Gleichung eindeutig gegeben. Man möchte nun gerne folgendes sagen: Ψ ist dem wirklichen Zustand des wirklichen Systems ein-eindeutig zugeordnet. Der statistische Charakter der Meßergebnisse fällt ausschließlich auf das Konto der Meßapparate bzw. des Prozesses der Messung. Wenn dies geht rede ich von einer vollständigen Beschreibung der Wirklichkeit durch die Theorie. Wenn aber eine solche Interpretation nicht durchführbar ist, nenne ich die theoretische Beschreibung 'unvollständig'." (von Meyenn [158], English translation taken from Fine, A. [73]. *The Shaky Game*. Second edition. University of Chicago Press, Chicago, p. 71.)

The key passage here is “without in any way disturbing a system”. We will get back to this.

The first part of the EPR paper focuses on the quantum mechanical description of a particle in a single space dimension. The authors highlight the fundamental importance of the notion of a state, “which is supposed to be completely characterised by the wave function Ψ.” According to quantum mechanics, Ψ renders a complete description. Further, the authors assume the validity of the probability interpretation. It states that the probabilities of measuring certain values of classical quantities can be obtained from the square of Ψ’s absolute value.

Now, if Ψ is the eigenfunction of an operator A with eigenvalue a, the probability interpretation holds that the physical quantity given by the operator A (such a physical quantity is called an observable in quantum theory) will in this state have the value a with certainty. EPR apply their reality criterion to this situation and infer from it that in this eigenstate an element of physical reality exists which corresponds to the physical quantity A. They take a momentum eigenstate with eigenvalue p_0 as an example[2] and conclude that it is thus reasonable to say that momentum of the particle in this state is real.

If the state is not an eigenstate of the operator A, one cannot deduce this anymore: There is no longer a certain value ascribed to the physical quantity described by A. Take the case where you ask for coordinate values of a particle (corresponding to its position) that currently is in a momentum eigenstate. As a matter of fact, in this case all coordinate values have the same probability! According to EPR, the only way to obtain a specific value for the coordinate is through a direct measurement, which, however, disturbs the particle and its state; the particle then no longer is in the momentum eigenstate. Interestingly, EPR assume a collapse (or reduction) of the wave function, i.e., a violation of Schrödinger’s equation, cf. Sect. 1.4; as was common back then, they do not describe this collapse dynamically but insert it by hand. EPR go on to generalise their conclusions and emphasise that, according to quantum mechanics (where the description through a wave function is considered complete), when the operators corresponding to two physical quantities do not commute, the two quantities cannot have simultaneous reality.

The conclusion of the first part of the EPR paper can thus be summarised as follows. Consider the two propositions:

P_1 The description of reality with wave functions is complete.
P_2 When the operators corresponding to two physical quantities do not commute, the two physical quantities have simultaneous reality.

EPR’s conclusion then is that either P_1 or P_2 is false. That is, either the description of reality with wave functions is incomplete, or non-commuting quantities cannot be

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[2] Such a state is described by a wave function that in every measurement yields the value p_0 for the momentum.

simultaneously real. It is important to note that the phrasing is “not simultaneously real” instead of “not simultaneously measured”: the authors do not doubt that non-commuting quantities cannot be *measured* simultaneously. Up to this point, the paper contains a mostly non-controversial application of the quantum formalism as would be expected in 1935. The second part ist the controversial one. Herein, EPR find that the result of the first part leads to a contradiction.

They demonstrate this with the help of a thought experiment (remember Einstein’s fondness for thought experiments). In it, they consider two systems I and II that interact for a certain time, but are disconnected ever after. Think, e.g., of a decay of a particle resulting in two new particles that fly away in opposite directions and that, in principle, can be separated by arbitrarily large distances. EPR assume that at the beginning both systems are described by states on their own (each by its own wave function), and that these states are known. Following the interaction, there is only one (today we call it entangled) wave function Ψ for the combined system I plus II. The two subsystems do not have a state (a wave function) on their own anymore.

EPR now assume that a measurement is taken on one of the two systems (system I), leading to a collapse (or reduction) of the wave function. As they put it in the first paragraph of part 2.:

> We cannot, however, calculate the state in which either one of the two systems is left after the interaction. This, according to quantum mechanics, can be done only with the help of further measurements, by a process known as the reduction of the wave packet. Let us consider the essentials of this process.

EPR now consider (as part of the thought experiment) measurements that are conducted on system I alone. To this end, they consider two different physical quantities (observables) A and B in I. The combined wave function[3] $\Psi(x_1, x_2)$ can then be expanded either in terms of the eigenfunctions of the operator A (EPR’s equation (7)) or in terms of the eigenfunctions of the operator B (EPR’s equation (8)). If one now measures in system I the quantity A, one finds a certain value a_k and the corresponding eigenfunction $u_k(x_1)$. According to the postulate of the reduction of a wave function, the wave function in EPR’s equation (7) is thus reduced to a product $\Psi(x_1, x_2) = \psi_k(x_2)u_k(x_1)$. But that means that the system II, on which no measurements can be taken (and that by consequence was not ‘disturbed’), now is in a concrete state, namely the one given by $\psi_k(x_2)$. The entanglement of the two systems is broken; the new state of the system is a product and therefore no longer describes a correlation between systems I and II.

But instead of A one can also measure quantity B in I, yielding a value b_r and an eigenfunction $v_r(x_1)$. The combined wave function thus reduces to a different product, namely $\Psi(x_1, x_2) = \varphi_s(x_2)v_s(x_1)$. EPR conclude from this:

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[3] Let x_1 denote the coordinate of the first, x_2 the coordinate of the second particle.

> We see therefore that, as a consequence of two different measurements performed upon the first system, the second system may be left in states with two different wave functions.

According to EPR, one can therefore assign two different wave functions to the same reality. They consider especially important the case where the alternative wave functions ψ_k and φ_s of system II are eigenfunctions of non-commuting operators P and Q. EPR's example for this case is so important that we shall discuss it explicitly. This means that our discussion will become somewhat formal now. However, the conclusions drawn from it can be understood without a detailed knowledge of the formalism.

In their example, systems I and II are two particles I and II that have a common wave function $\Psi(x_1, x_2)$. EPR choose the following form of the wave function:

$$\Psi(x_1, x_2) = h\delta(x_1 - x_2 + x_0) \equiv \int_{-\infty}^{\infty} \mathrm{d}p\; \mathrm{e}^{2\pi \mathrm{i}(x_1 - x_2 + x_0)p/h}, \tag{2.1}$$

where x_0 is a constant and h is Planck's constant. Due to the delta function the difference of x_2 and x_1 is virtually fixed to x_0.

Now A is the momentum operator[4] of particle I, whose eigenfunctions read:

$$u_p(x_1) = \mathrm{e}^{2\pi \mathrm{i} p x_1/h}, \tag{2.2}$$

where the usual normalisation constant is missing and where the number p is the eigenvalue. The combined state (2.1) can now be expanded in terms of these momentum eigenfunctions:

$$\Psi(x_1, x_2) = \int_{-\infty}^{\infty} \mathrm{d}p\; \psi_p(x_2) u_p(x_1), \tag{2.3}$$

where

$$\psi_p(x_2) := \mathrm{e}^{-2\pi \mathrm{i}(x_2 - x_0)p/h} \tag{2.4}$$

is the eigenfunction of the momentum operator of particle II, itself given by

$$P := \frac{h}{2\pi \mathrm{i}} \frac{\partial}{\partial x_2}.$$

P's eigenvalue is $-p$, which is of course a direct consequence of momentum conservation: The total momentum of the initial state is zero and retains that value (as long as no measurement of position is taken on any of the particles).

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[4] Given explicitly by $(\hbar/\mathrm{i})\, \partial/\partial x_1$.

Now EPR consider an alternative where B is the position operator of particle I, whose (improper) eigenfunction v_x is the delta function:

$$v_x(x_1) = \delta(x_1 - x). \tag{2.5}$$

Then one can expand the combined state (2.1) in terms of these position eigenfunctions instead of the momentum eigenfunctions:

$$\Psi(x_1, x_2) = \int_{-\infty}^{\infty} \mathrm{d}x \; \varphi_x(x_2) v_x(x_1), \tag{2.6}$$

where

$$\varphi_x(x_2) := \int_{-\infty}^{\infty} \mathrm{d}p \; \mathrm{e}^{2\pi \mathrm{i}(x - x_2 + x_0)p/h} = h\delta(x - x_2 + x_0) \tag{2.7}$$

is the eigenfunction of the position operator $Q = x_2$ of particle II. Its eigenvalue is $x + x_0$, this being the coordinate value of particle II.[5] Because

$$[P, Q] \equiv PQ - QP = \frac{h}{2\pi \mathrm{i}},$$

$\Psi_p(x_2)$ and $\varphi_x(x_2)$ are indeed eigenfunctions of non-commuting operators, namely of position and momentum of particle II. Non-commuting operators correspond to physical quantities that cannot be measured simultaneously and are therefore subject to an uncertainty relation.

This is EPR's example, a special case of the general situation they had presented in the beginning of their second part. EPR now get back to their general discussion and reach a conclusion. They assume that the wave functions Ψ_k and φ_r of system II are eigenfunctions of non-commuting operators P and Q with eigenvalues p_k and q_r, respectively. The (thought of) experimenter now has a free choice to take on system I a measurement of A *or* B. If he chooses to measure A, system II will be described by the wave function $\Psi_k(x_2)$ (in the above example this is $\Psi_p(x_2)$ in (2.4)) with eigenvalue p_k (above: p). If he chooses to measure B in I, the wave function of II will be $\varphi_r(x_2)$ (above: $\varphi_x(x_2)$) with eigenvalue q_r. But in neither case system II is being disturbed! EPR now bring forward their reality criterion a second time:

> In accordance with our criterion of reality, in the first case we must consider the quantity P as being an element of reality, in the second case the quantity Q is an element of reality. But, as we have seen, both wave functions Ψ_k and φ_r belong to the same reality.

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[5] Note that the difference $x_1 - x_0$ and the sum $p_1 + p_2$ correspond to commuting operators and are thus simultaneously 'measurable'.

The EPR paper now concludes as follows. In the first part, EPR found that of the two propositions P_1 and P_2 either P_1 or P_2 is false, i.e., either the description with wave functions is incomplete or non-commuting quantities do not have simultaneous reality. In the second part, they found $P_1 \Rightarrow P_2$, i.e., that the simultaneous reality of physical quantities corresponding to non-commuting operators follows from the assumption of completeness. According to the rules of elementary logic the findings of both parts of the paper are both true only if either P_1 and P_2 are both false or if P_1 is false and P_2 is true. Since EPR's thought experiment showed P_2 to be true (under the assumption of separability), P_1 must be false; i.e., the description of reality with wave functions is incomplete. This is the essential conclusion of the EPR paper.

As a matter of fact, the trueness of proposition P_2 follows directly from the locality criterion or from separability (although these notions[6] are not mentioned directly in the paper): events in system I cannot affect the spatially separated system II (it is too far out). In their second part, EPR only show P_2 to be true if one assumes locality: P_1 is not really involved. One can deduce directly from the result of part I (either P_1 or P_2 false) that P_1 must be false, i.e., that i.e., that the description with wave functions is incomplete. These are EPR's closing remarks:

> While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question of whether or not such a description exists. We believe, however, that such a theory is possible.

However, they did not conclude their paper without having presented a possible loophole through which one could avoid the conclusion of quantum theory's incompleteness (second last paragraph):

> Indeed, one would not arrive at our conclusion if one insisted that two or more physical quantities can be regarded as simultaneous elements of reality *only when they can be simultaneously measured or predicted.*

Since the simultaneous measurement of non-commuting quantities A and B in system I is not possible, P and Q could not have simultaneous reality in system II, although system II is arbitrarily far out. Here is what EPR have to say about this:

> This makes the reality of P and Q depend upon the process of measurement carried out on the first system, which does not disturb the second system in any way.

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[6] These notions are sometimes used synonymously, and sometimes as meaning something different. We apply the meaning that d'Espagnat later called *Einstein-separability* [47, p. 132] and that was described by Einstein in 1949 in the following words[59, p. 84 (85)]: "Now, however, the real situation of S_2 must be independent of what happens to S_1." ("Der reale Sachverhalt (Zustand) des Systems S_2 ist unabhängig davon, was mit dem von ihm räumlich getrennten System S_1 vorgenommen wird.")

EPR do not expect this to be a reasonable definition of reality. Bohr, however, clearly thought differently (cf. further below).

2.3 Bohm's Version of the Thought Experiment

The combined state (2.1) that EPR used in their thought experiment shows some disadvantageous traits. For example, it is not only mathematically quite intricate to include the delta function in the formalism of quantum mechanics (it is not a state in Hilbert space that can be normalised), it is also dynamically unstable. By this we mean that a state described by this function widens with time – a strongly localised wave packet delocalises rapidly. An easier version of EPR's thought experiment, mathematically and conceptually, was introduced by David Bohm (1917–1992) in his textbook on quantum mechanics [21, p. 610–623], written under strong influence of the Copenhagen interpretation. It uses the particle's spin and has since its appearance replaced EPR's original thought experiment in most discussions of their arguments. Interestingly, it is also the version that is more accessible to an experimental realisation. However, the experiments could only be realised decades after Bohm's introduction of the thought experiment. Yet, also EPR's original state (2.1) can be realised experimentally as a two-mode squeezed state[7] (see, e.g., Leonhardt [111, p. 74]). The realisation was reported in a paper by Ou et al. [121]. In fact, this state plays an important role in cosmology and the physics of black holes (see discussion in chap. 6). It exhibits, in a well-defined way, maximal correlation [8].

Now, Bohm in his thought experiment considers a molecule consisting of two atoms, each of spin $\hbar/2$. The total spin of the combined system is assumed to be zero. So there is an anticorrelation for the orientation of the atoms' spins with respect to a given but arbitrary direction.

Now the molecule is thought to be fragmented into the two atoms by some dissociation process, and the atoms then separated to an arbitrarily large distance (in principle, their distance can be of astronomical magnitude). 'Arbitrarily large distance' means that an interaction between the two atoms is made impossible, in complete analogy to the two particles in EPR's original thought experiment. Note that the value of the total spin of the combined system is conserved.

The argument then continues as in the EPR paper. If one takes a measurement on the z-component of the first atom's spin, then one can deduce from it the z-component of the second atom's spin due to their anticorrelation; if one finds $+\hbar/2$ for atom 1, atom 2 must have $-\hbar/2$. Now the point is that one can of course take a measurement on atom 1's spin with respect to any other direction, for example the x-direction. If one finds $+\hbar/2$ for

---

[7] A squeezed state is a state with a very small uncertainty in either the position or the momentum coordinate, resulting in a very large uncertainty of the conjugated quantity (i.e., momentum and position, respectively) due to the uncertainty relation.

atom 1, atom 2's spin component in x-direction must be $-\hbar/2$. So just like EPR one can conclude that all spin orientations of atom 2 have simultaneous reality. The atom's spin orientations play the role of the conjugated quantities position and momentum. Just like those, the spins in different orientations do not commute and can therefore not be measured simultaneously, according to the laws of quantum mechanics. Thus, one can conclude the incompleteness of quantum mechanics from this version, too.

What does the mathematical description look like (for the formalism, also cf. appendix A)? Following the rules of quantum mechanics, one has the following four basic states at one's disposal when constructing the combined state of a system consisting of two spin-½-systems:

$$\begin{aligned} |\Psi_a\rangle &= |\uparrow\rangle_1 |\uparrow\rangle_2, \qquad |\Psi_b\rangle = |\downarrow\rangle_1 |\downarrow\rangle_2, \\ |\Psi_c\rangle &= |\uparrow\rangle_1 |\downarrow\rangle_2, \qquad |\Psi_d\rangle = |\downarrow\rangle_1 |\uparrow\rangle_2. \end{aligned} \tag{2.8}$$

The last two of these basic states,$|\Psi_c\rangle$ and $|\Psi_d\rangle$, describe situations in which each of the two atoms has a certain spin value in z-direction and the two spins are antiparallel. But neither of those two states corresponds to a well defined total spin value. In other words: Those states are no eigenfunctions of the total spin operator. The state with zero total spin is only obtained through an interference of $|\Psi_c\rangle$ and $|\Psi_d\rangle$ with a very specific phase relationship; this is the 'singlet-state':

$$|\Psi\rangle = \frac{1}{\sqrt{2}}\left(|\Psi_c\rangle - |\Psi_d\rangle\right). \tag{2.9}$$

This is the state that takes over the role of EPR's state (2.1) in Bohm's version of the thought experiment. If one chose a minus sign instead of a plus sign in (2.9), one would obtain a state with total spin 1; so the phase relationship[8] between the two components is essential.

Whereas the state (2.9) corresponds to a definite total spin (of zero value), the individual spins of the atoms are undetermined. In a measurement of atom 1's spin, this state with definite total spin but undetermined individual spins turns into a state with undetermined total spin but definite individual spins. Namely, it turns into the state $|\Psi_c\rangle$ or $|\Psi_d\rangle$ (this is the wave function collapse as discussed above).

The form of the state (2.9) can be generalised to all spin orientations. For two antiparallel spins in x-direction and total spin zero, *the same* state (2.9) can be decomposed into spin eigenfunctions with respect to the x-direction:

$$|\Psi\rangle = \frac{1}{\sqrt{2}}\left(|\rightarrow\rangle_1 |\leftarrow\rangle_2 - |\leftarrow\rangle_1 |\rightarrow\rangle_2\right). \tag{2.10}$$

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[8] Because the wave functions are complex quantities, they are characterised by an amplitude and a phase (an angle). The phase relationship then is the *relative* angle between wave functions; in (2.9), this is 180 degrees due to the minus sign. With a plus sign one would have 0 degrees.

An equal expression holds for any direction; the quantum state is independent of direction.

If a measurement shows that atom 1's spin points to the right, so atom 2's spin must point to the left after following that measurement, and vice versa. The argument continues as shown above. Since you can measure atom 1's spin *either* in z- *or* in x-direction without disturbing atom 2, both spin directions must have a physical reality. Because the state is independent of direction, this argument holds for all spin directions. The description with a wave function that doesn't allow that must thus be incomplete.

Remember that in the measurement it is not possible to send signals from atom 1 to atom 2.[9] For atom 2, both state decompositions (2.9) and (2.10) yield the same reduced density matrix (cf. appendix). Whatever the direction, this matrix has always the form

$$\rho_{\text{red}} = \frac{1}{2}\begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix}. \tag{2.11}$$

For the z-direction, this reduced density matrix corresponds to an ensemble of 50% atoms with spin in the positive and 50% atoms with spin in the negative direction. The same is true for all directions. So a measurement on atom 2 cannot help to decide whether a measurement on atom 1 has taken place or not.

How did Bohm interpret EPR's thought experiment? He questions EPR's local reality criterion and states that only on a classical level there is a unique correspondence between a mathematical theory and "elements of reality". In contrast to this, all there is in quantum theory is a statistical relation between the wave function and the system (Bohm uses the term *potentiality*). Because of this purely statistical trait of nature, one just cannot talk about a precisely defined element of reality for, e.g., the position of an electron. And because EPR's assumption does not apply to quantum theory, one cannot deduce from it this theory's incompleteness. In Bohm's own words [21, p. 622]:

> [...] the present form of quantum theory implies that the world cannot be put into a one-to-one correspondence with any conceivable kind of precisely defined mathematical quantities, and that a complete theory will always require concepts that are more general than that of analysis into precisely defined elements.

It should, however, be stressed once again that for EPR it is precisely this statistical character of quantum theory that embodies its incompleteness.

Bohm's discomfort with the quantum theory led him to his own interpretation in the following year, that is known today as de Broglie-Bohm or Bohm-interpretation. In it, the election is described by a wave function and an *additional* position variable; we are going to get back to this further below.

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[9] A general proof of this can be found in d'Espagnat [47, p. 117*ff.*], where this fact is referred to as 'parameter independence'. In particular, there can be no communication with superluminal velocity.

Bohm concludes this chapter of his textbook with a short argument to prove that the concept of local reality (represented by hidden local variables) is incompatible with quantum theory. A convincing mathematical expression of this incompatibility are the Bell inequalities that shall be discussed further below (Sect. 5.2). With them, it can be decided *experimentally* if the concept of local reality is correct or not.

2.4 The Contributions of Einstein's Co-Authors

We have already briefly discussed the life journey of Einstein's co-authors up to their meeting in Princeton in the first chapter. How did their journey continue?

There are indications that Podolsky was the one who did the actual writing of the paper. Einstein was not really happy with it on a linguistic level.[10]

For example, he wrote to Schrödinger on June 19, 1935:

> For reasons of language it was written by Podolsky, after several discussions. It did not become clear what I actually intended, though; rather, the essential thing was, so to speak, smothered in learnedness. The true difficulty lies in the fact that physics is a kind of metaphysics; physics describes 'reality'. But we do not know what 'reality' is; we only know it through the physical description![11]

That Podolsky was the one responsible for language[12] can also be guessed from the missing definite article in the title of the article. In "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" there is a *the* missing after *can*. An omission that would not be atypical for a scientist of Russian origin. While Bohr in his answer chose to quote the title as it is, the American physicist Arthur E. Ruark inserted the definite article in his commentary [139]. In his biography of Einstein, Abraham Pais

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[10] Whitaker [166, p. 78] noted: "And it was Podolsky who put the argument together and wrote the account of the ideas that was published. But unfortunately in doing so, he irritated Einstein very much, because Podolsky was an expert in logic, and wrote the paper rather as an exercise in formal logic, instead of the comparatively straightforward argument that Einstein thought was possible." This is why Einstein later sought to bring his argument forward in his own terms, his contribution to the journal *Dialectica* that is reprinted in this book being an example for these attempts.

[11] "Diese ist aus Sprachgründen von Podolsky geschrieben nach vielen Diskussionen. Es ist aber nicht so gut herausgekommen, was ich eigentlich wollte; sondern die Hauptsache ist sozusagen durch Gelehrsamkeit verschüttet. Die eigentliche die Physik eine Art Metaphysik ist; Physik beschreibt 'Wirklichkeit'. Aber wir wissen nicht, was ist; wir kennen sie nur durch die physikalische Beschreibung!" (von Meyenn [158], parts of the English translation taken from Fine, A. [73]. *The Shaky Game*. Second edition. University of Chicago Press, Chicago, p. 35).

[12] It might be suspected that the only American in the trio, Rosen, was not chosen to write the article because he was judged too young and unexperienced or, what seems to be more probable, because Podolsky was too dominant a personality.

furthermore noted that Einstein would have used the term Ψ*-function* instead of *wave function* [122, p. 499].

After the EPR paper was finished, it seems Einstein had no further contact with Podolsky. This however was not so much due to the fact that the "essential thing was [...] smothered in learnedness", but was rather provoked by a capricious act of Podolsky's that left Einstein in anger. The *New York Times* in its Saturday issue on May 4, 1935 (ten days before the EPR paper was published in *Physical Review*!) published an article entitled "Einstein Attacks Quantum Theory", reporting to quite some extent on EPR's work.[13] The article was supplemented by a résumé of the paper, written by Podolsky. Podolsky was the one to initiate this article, without having talked about it to Einstein or Rosen. Einstein's unease with this can be read off his following statement, published in the *New York Times* on May 7, 1935 [quote taken from Jammer [98, p. 190]]:

> Any information upon which the article 'Einstein Attacks Quantum Theory' in your issue of May 4 is based was given to you without authority. It is my invariable practice to discuss scientific matters only in the appropriate forum and I deprecate advance publication of any announcement in regard to such matters in the secular press.

Boris Podolsky was appointed professor at the University of Cincinnati, United States, in 1935 and went on to the Xavier University, also in Cincinnati, in 1961. He died in 1966. His scientific work focused on generalisations of electrodynamics. His later statements on EPR's work emphasised its main point, namely the incompleteness of quantum theory, cf. especially his contributions to a conference at the Xavier University from October 1–5 in 1962 that he had helped organise and that was devoted to the foundations of quantum theory. Among the conference participants were Rosen, Dirac and Wigner. The talks and discussions contributions are available online and offer plenty of interesting thoughts [172].[14]

Nathan Rosen's contribution to the EPR paper seems to have been to propose the entangled state (2.1) and to have worked out the details of the actual calculation. In view of Rosen's preliminary experiences this should not come as a surprise. In 1931, he had published a paper on the hydrogen molecule [135], related to his dissertation at MIT, wherein he had used the wave function

$$\Psi = \Psi(a1)\Psi(b2) + \Psi(b1)\Psi(a2), \tag{2.12}$$

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[13] It says in the article: "Professor Einstein will attack science's important theory of quantum mechanics, a theory of which he was a sort of grandfather. He concludes that while it is 'correct' it is not 'complete'." Quote taken from Jammer [98, p. 189]

[14] Photo (Fig. 2.1) taken from: http://www.titanians.org/about-bob-podolsky/
The page also includes a photograph of Boris Podolsky.

Fig. 2.1 Group photograph taken at the conference at Xavier University in October 1962. Bottom row (from left to right): Eugene Wigner, Nathan Rosen, Paul Dirac, Boris Podolsky, Yakir Aharonov and Wendell Furry. (Photograph by Babst Photographic Services found in University Archives and Special Collections, Xavier University Library, Cincinnati, Ohio)

where a and b refer to the two nuclei and 1 and 2 refer to the two electrons (this is equation (10) of Rosen's paper).

At first sight, (2.12) looks like an entangled state similar to the state (2.1) that was later used in the EPR paper. But in fact, it is only the result of a formal symmetrisation that becomes necessary if one is working with the classical construct of ascribing numbers to particles (cf., i.e., Zeh [180, p. 10]). A true entanglement involves the entanglement of relative coordinates, in the general case (where the spin-orbit coupling cannot be neglected) also the spin. In 1931, Rosen probably had not yet recognised this difference. An entangled state for the hydrogen molecule was used by Hylleraas [94], who had already in 1929 formulated an entangled state for the helium atom, see equation (11) in Hylleraas [93].[15] Only when taking into account this entanglement, one obtains the correct energy levels for the ground state. This important remark was already made by Heisenberg in 1935:

> Furthermore, one can point to the fact that the natural character of quantum mechanics is very tightly bound to the formal circumstance that its mathematical frame-work of wave functions operates in multi-dimensional configuration space, not in ordinary space, and that

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[15] A discussion of Hylleraas' method can, e.g., be found in Sommerfeld [152, p. 677*ff.*] or in Bethe and Salpeter [20, p. 232 *ff.*].

> precisely this feature of quantum mechanics has been exactly confirmed through the correct reproduction of the more complicated atomic spectra. [16]

In contrast to Podolsky, Einstein did not cut off contact to Rosen. As a matter of fact, from 1935 on, a fruitful collaboration developed between the two on the subject of problems related to general relativity. While working on the EPR paper, they had worked on the perhaps most important publication on this subject. This article, entitled "The Particle Problem in the General Theory of Relativity" was submitted to *Physical Review* on May 8, 1935 (one week before the publication of the EPR paper) and was published on July 1 of the same year [64]. In it, the authors present what would later be called the 'Einstein-Rosen bridge' or the 'Einstein wormhole'.

What is this about? According to general relativity, the exterior geometry of a spherical mass distribution is described by a solution of the field equations that had been found by the astronomer Karl Schwarzschild in 1916 and is known as the Schwarzschild solution. In its originally adopted form this solution features an irritating trait: at a certain distance from the centre, it becomes singular and therefore meaningless.[17] While trying to eliminate this singularity, Einstein and Rosen found a solution that connects two copies of the exterior geometry by a small bridge (a 'wormhole') (see Fig. 2.2).

Because such a solution also exists in the presence of an electromagnetic field, Einstein and Rosen interpreted it as a possible model for describing elementary particles (such as protons and electrons). This would offer a way to infer the existence of matter directly from the field equations, i.e., without having to insert it into the equations by hand. Towards the end of their paper, Einstein and Rosen write:

> ...one does not see *a priori* whether the theory contains the quantum phenomena. Nevertheless one should not exclude *a priori* the possibility that the theory may contain them.

And in his letter to Schrödinger on June 7, 1935, Einstein wrote [158, vol. 2, p. 536]:

> I have found that in general relativity neutral and charged particles can be described as singularity-free fields, with no need of additional terms. From the point of view of principles, I absolutely do not believe in a statistical basis for physics in the sense of quantum mechanics,

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[16] "Ferner kann man darauf hinweisen, daß der natürliche Charakter der Quantenmechanik aufs engste mit dem formalen Umstand verknüpft ist, daß ihr mathematisches Schema von Wellenfunktionen im mehrdimensionalen Konfigurationsraum, nicht im gewöhnlichen Raum handelt, und daß eben dieser Zug der Quantenmechanik durch die korrekte Wiedergabe der komplizierteren Atomspektren eine genaue Bestätigung erfahren hat." [87, p. 418]. English in: W. Heisenberg, Elise Crull, Guido Bacciagaluppi (2011), Translation of: W. Heisenberg, 'Ist eine deterministische Ergänzung der Quantenmechanik möglich?' Available online: halshs-00996315.

[17] We are talking about the coordinate singularity occurring at the Schwarzschild radius.

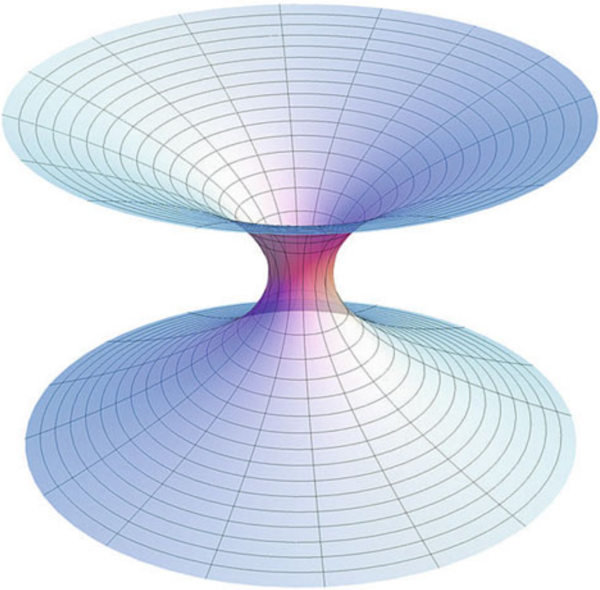

Fig. 2.2 Einstein-Rosen bridge. (Figure drawn by Allen McC.; redistribution permitted under a *Creative Commons Attribution-Share Alike 3.0 Unported* license)

> despite the singular success of the formalism of which I am well aware. I do not believe such a theory can be made general relativistic.[18]

And that is where their work is related to the EPR paper! With the help of concepts like the Einstein-Rosen bridge Einstein sought to complete the quantum theory. We will get back to this. It was found later that Einstein's and Rosen's solution from 1935 is not suited for this purpose, because it is unstable and therefore not able to describe (stable) elementary particles. Also, being a classical concept it could not explain the EPR situation.[19]

Einstein and Rosen cooperated on two further papers. One dealt with the two-body problem in general relativity [65], the other one with cylindric gravitational waves [66].

Following the year 1936, Rosen was initially appointed professor at the University of Kiev, Ukraine (back then USSR), and from 1941 on at the University of North Carolina in

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[18] "Ich habe gefunden, daß allgemein relativistisch neutrales Massenteilchen und elektrisches Teilchen sich ohne Zusatzglieder als singularitätsfreie Felder darstellen lassen. Es besteht aber eine ernst zu nehmende Möglichkeit, die Atomistik relativistisch-feldtheoretisch darzustellen, wenn es auch mathematisch überaus schwierig erscheint, zu den Mehrkörper-Problemen vorzudringen. Ich glaube vom prinzipiellen Standpunkt absolut nicht an eine statistische Basis der Physik im Sinne der Quantenmechanik, so fruchtbar sich dieser Formalismus im Einzelnen auch erweist. Ich glaube nicht, daß man eine derartige Theorie allgemein relativistisch durchführen kann." ([158, vol. 2, p. 536, parts of the English translation are taken from Fine, A. [73]. *The Shaky Game*. Second edition. University of Chicago Press, Chicago, p. 68])

[19] Still, Maldacena and Susskind [115] did speculate about a possible connection between the two.

Chapel Hill, United States. From 1953 until his death in 1995, he was a professor at the Israel Institute of Technology (Technion) in Haifa, Israel. Just like Einstein and Podolsky, throughout the rest of his career, Rosen kept emphasising the main point of the EPR paper: the incompleteness of quantum mechanics (see, e.g., Rosen [137]). One of his students was Asher Peres (1934–2005), who contributed to the foundations of quantum theory, especially to the comparatively young field of quantum information.

2.5 Critical Evaluation

In their paper, EPR concluded the incompleteness of quantum theory. We have discussed the details of this conclusion in the preceding section. Now, we shall discuss the implicit and explicit assumptions on which EPR based their conclusion and will, to this end, deviate from the historic course of events in favour of a more systematic approach. We will rejoin the chronological path of events in the following chapter on the reception and impact of the EPR paper.

First, there is the reality criterion, according to which there is an element of reality corresponding to a physical quantity, if we can predict with certainty the value of this quantity without disturbing the system, cf. the beginning of Sect. 2.2. As Beller and Fine [18] have pointed out, EPR use this criterion merely once in their paper, and only indirectly. Following their equation (1), EPR mention the undisputed quantum mechanical fact that a physical quantity A has with certainty the value a, whenever the wave function of the system is the eigenfunction with associated eigenvalue a of the operator A that corresponds to the physical quantity ('eigenfunction-eigenvalue link'). Then, EPR continue, "in accordance with our criterion of reality [...] there is an element of physical reality corresponding to the physical quantity A." When EPR mention the reality criterion in part 2 of their paper, they really only talk about this eigenfunction-eigenvalue link. They do *not* show that P and Q have *simultaneous* reality in system II; in order to show this through the reality criterion, A and B would need to be *simultaneously* measurable in system I, but quantum mechanics prohibits that; and EPR do not doubt the validity of quantum mechanics. They only *do* show that system II has a reality that can be described by a momentum eigenfunction as well as by a position eigenfunction. This fact will be essential when evaluating Bohr's response in Sect. 4.2.

According to Fine [73, p. 5], Einstein never again mentioned EPR's reality criterion. It appears that – at least to Einstein – the criterion did not much matter for EPR's line of argument. As he put it in the letter to Schrödinger quoted above, for him "the essential thing was [...] smothered in learnedness." However, what happens if the learnedness were pushed aside and the core of EPR's argument laid bare? What was Einstein's key assumption of their work? He repeatedly commented on this, initially in his letters to Schrödinger, later in his essay on "Physik und Realität" ("Physics and reality") [57], and in the article published in the journal *Dialectica* [58] included in this book, as well as in his contributions to the anthology edited by Paul Arthur Schilpp [59, 60] and the Born

Festschrift [61]. All these texts make it quite clear that Einstein's point was based on the locality or separability of physical systems; in EPR's case, of systems I and II. It is worth taking another, detailed look at Einstein's letter to Schrödinger on June 19, 1935 – i.e., one month after the publication of the EPR paper–, in which Einstein highlights the important points giving a simple example. Einstein wrote:

> I have in front of me two boxes, with lids that can be opened. I can look into the boxes, when the lids are open; this is called 'to make an observation'. There is also a ball that is always either in one or the other box when making an observation. I will describe the situation as follows: 'The probability to find the ball in the first box is ½.' Is this a complete description?[20]

Einstein then goes on to discuss the two possible answers. If one considers the statement that the ball is either in the first box or it is not, a complete description, then according to Einstein the answer must be *no*. But if one assumes (as most of the quantum theorists did at the time) that the ball is in neither of the boxes before the lid is opened, then according to Einstein the answer must be *yes*. The state then is completely described by the probability ½, and there is no reality beyond the statistical character of the world of experience. Einstein then establishes the analogy with quantum theory:

> We face similar alternatives when we want to explain the relation of quantum mechanics to reality. With regard to the ball-system, naturally, the second, 'spiritualist' or Schrödinger interpretation is hackneyed, and the man on the street would only take the first, 'Bornian' interpretation seriously. But the Talmudic philosopher dismisses 'reality' as a bogy of naiveté and declares the two conceptions differing only in their terminology.[21]

Now, Einstein lets the cat out of the bag and presents his separation principle, from which in the EPR paper followed – though smothered in learnedness – the incompleteness of quantum theory:

> Now *my* mode of thought is thus: One cannot get by the Talmudic philosopher without the help of an additional principle: the 'separation principle'. Namely: 'The second box including

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[20] "Vor mir stehen zwei Schachteln mit aufklappbarem Deckel, in die ich hineinsehen kann, wenn sie aufgeklappt werden; letzteres heißt "eine Beobachtung machen". Es ist außerdem eine Kugel da, die immer in der einen oder anderen Schachtel vorgefunden wird, wenn man eine Beobachtung macht. Nun beschreibe ich einen Zustand so: *Die Wahrscheinlichkeit dafür, daß die Kugel in der ersten Schachtel ist, ist 1/2.* Ist dies eine vollständige Beschreibung?" ([158], volume 2, p. 537, English translation by S. Linden and A. K. Hudert)

[21] "Vor der analogen Alternative stehen wir, wenn wir die Beziehung der Wirklichkeit deuten wollen. Bei dem Kugel-System ist natürlich die zweite, 'piritistische' oder Schrödingersche Interpretation sozusagen abgeschmackt und nur die erste 'Bornsche' würde der Bürger ernst nehmen. Der talmudische Philosoph aber pfeift auf die 'Wirklichkeit' als auf einen Popanz der Naivität und erklärt beide nur der Ausdrucksweise nach verschieden." (ibid., English translation following Fine, A. [73]. *The Shaky Game*. Second edition. University of Chicago Press, Chicago, p. 69)

> everything concerning its content is independent of what is happening with the second box' (separated subsystems). If we hold fast to this separation principle, then the second (the 'Schrödinger') interpretation is excluded and only the Bornian one remains according to which, however, the above given description of the state is an *incomplete* description of *reality* or of the real states, respectively.[22]

Einstein then, by using the mathematical formalism and explicitly applying his separation principle, once again summarises the essential line of thought of the EPR paper.

So the locality or separability criterion is of pivotal importance to Einstein; when you dispose of it, says Einstein, you lose the basis for a reasonable description of nature. In his later essays on the topic, he elevated the thoughts he had formulated in the letter to Schrödinger on a philosophical level of general significance. For example, in his *Dialectica* contribution [58, p. 321] he writes:

> Without assuming such an independence of the existence (of the 'being-thus') of spatially distant objects, which first originates from everyday thinking, physical reasoning would not be possible in the manner familiar to us.[23]

From this, Einstein concludes the incompleteness of quantum theory because different wave functions can be attributed to the same local reality. Assuming the completeness would correspond to implying "the hypothesis of action-at-a-distance, an hypothesis which is hardly acceptable"[24], which for Einstein is incompatible with the theory of relativity. Only later, in connection with the formulation of the Bell inequalities some years after Einstein's death, it became completely clear that the assumption of local reality not only contradicts the completeness of quantum theory, but more than that also contradicts its consistency and feasible experiments. It seems pointless to retrospectively try to imagine Einstein's reaction to this finding, had he still lived.

It also follows from Einstein's general remarks that it is indeed sufficient to consider only one variable (e.g., the position coordinate). To consider two non-commuting and therefore not simultaneously measurable variables sharpens the argument, but is rooted mostly in the historical tide of events, namely that it was preceded by the discussion about

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[22] "*Meine* Denkweise ist nun so: An sich kann man dem Talmudiker nicht beikommen, wenn man kein zusätzliches Prinzip zu Hilfe nimmt: Nämlich: 'die zweite Schachtel nebst allem, was ihren Inhalt betrifft, ist unabhängig davon, was bezüglich der ersten Schachtel passiert' (getrennte Teilsysteme). Hält man an dem Trennungsprinzip fest, so schließt man dadurch die zweite ('Schrödingersche') Auffassung aus und es bleibt nur die Bornsche, nach welcher aber die obige Beschreibung des Zustands eine *unvollständige* Beschreibung der *Wirklichkeit*, bzw. der wirklichen Zustände ist. " (ibid., English translation by S. Linden and A. K. Hudert)

[23] "Ohne die Annahme einer solchen Unabhängigkeit der Existenz (des 'So-Seins') der räumlich distanten Dinge voneinander, die zunächst dem Alltags-Denken entstammt, wäre physikalisches Denken in dem uns geläufigen Sinne nicht möglich."

[24] "[. . .] die Hypothese einer schwer annehmbaren Fernwirkung" [58, p. 323].

the uncertainty relations. When we consider an entangled wave function $\Psi(x_1, x_2)$, we can conclude as follows. Because of the entanglement, x_1 and x_2 do not have a reality on their own. Only when the state is reduced to a product by taking a measurement on, e.g., x_1, and when one also applies Einstein's separation principle, a reality can be ascribed to x_2. Edward Teller (1908–2003) was one to very early highlight this fact, as we can see in a letter he wrote to Schrödinger in June 1935 [158, p. 530]. For this reason, Teller did not even want to speak of a reality.

Einstein expressed his 'epistemological credo' in his contributions to Schilpp's Festschrift. Therein, he wrote [59, p. 31]:

> Physics is an attempt conceptually to grasp reality as it is thought independently of its being observed. In this sense one speaks of 'physical reality'.[25]

In his remarks to the essays appearing in this collective volume, Einstein pointed out [60, p. 236]:

> 'Being' is always something which is mentally constructed by us, that is, something which we freely posit (in the logical sense).[26]

Here, Einstein essentially turned against classical positivism and its motto *esse est percipi* ('to be is to be perceived'), according to which being results from being perceived. The thinking conveyed in these quotes differs remarkably from the first part of the EPR paper. Whereas in that paper, reality is an objective quantity and the terms and definitions one applies serve the only purpose of corresponding to the elements of this reality, Einstein now promotes the freedom of choice when selecting the terms and definition in an attempt to approach the reality that is somewhere beyond its being observed. He wrote [59, p. 12]:

> Although the conceptual systems are logically entirely arbitrary, they are bound by the aim to permit the most nearly possible certain (intuitive) and complete co-ordination with the totality of sense-experiences; [. . .][27]

A whole lot of these remarks resemble the *Philosophical Investigations* ('Philosophische Untersuchungen'), published years later, in 1953, by the philosopher Ludwig Wittgenstein (1889–1951, cf. Wittgenstein [171]) who to some extent was influenced by

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[25] "Die Physik ist eine Bemühung, das Seiende als etwas begrifflich zu erfassen, was unabhängig vom Wahrgenommen-Werden gedacht wird. In diesem Sinne spricht man vom 'Physikalisch-Realen'." (English translation by Schilpp [141, p. 81])

[26] "Das 'Sein' ist immer etwas von uns gedanklich Konstruiertes, also von uns (im logischen Sinne) frei Gesetztes." (English translation by Schilpp [141, p. 669])

[27] "Die Begriffssysteme sind zwar an sich logisch gänzlich willkürlich, aber gebunden durch das Ziel, eine möglichst sichere (intuitive) und vollständige Zuordnung zu der Gesamtheit der Sinneserlebnisse zuzulassen; [. . .]" (English translation by Schilpp [141, p. 13])

the Vienna Circle chaired by Moritz Schlick. In some ways, Einstein's notion of a free posit of terms and definitions corresponds to Wittgenstein's notion of language-games. The main difference between the two is of course that the terms and definitions one chooses in physics are subject to empirical testing.

Since Einstein was convinced to have discovered the incompleteness of the quantum theory, he tried to complete it. But he did not attempt a completion from within: "I believe, however, that this theory offers no useful point of departure for future development."[28]

Instead, Einstein saw remedy in his search for a unified field theory. Since the 1920s, Einstein had tried to unify gravitation and electromagnetism following the example of his general relativity. He expected particles – that is, the behaviour of atoms and electrons, usually the realm of quantum theory – to turn out to be singularity-free solutions of his fundamental field equations. The above mentioned paper he wrote with Rosen was supposed to be the first brick of this beautiful building: "Look at the small paper on a possible relativistic interpretation of matter that I recently published with Mr. Rosen in the Physical Review. If the mathematical difficulties can be overcome, this might lead to something."[29]

In his contribution to the de Broglie Festschrift, Einstein once again made the point:

> We partly owe my efforts to complete the theory of general relativity by generalising the gravitational equations to the surmise that a reasonable general relativistic field theory might offer the key to complete the quantum theory. This is but a modest hope, not a conviction.[30]

We will discuss further below why ultimately Einstein's hope was in vain.

Quite early, Einstein felt appalled by the statistical character of quantum theory. There is a passage in a letter he wrote to Born on December 4, 1926 (i.e., ten years before the EPR paper), that has gained fame:

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[28] "Ich glaube aber, daß diese Theorie keinen brauchbaren Ausgangspunkt für die künftige Entwicklung bietet." ([59, English translation by S. Linden and A. K. Hudert])

[29] "Schau Dir die kleine Arbeit an, die ich mit Herrn Rosen in der Physikalischen Review jüngst über eine denkbare relativistische Deutung der Materie publiziert habe. Dies könnte zu etwas führen, wenn sich die mathematischen Schwierigkeiten überwinden lassen." Einstein in a letter to Schrödinger on August 8, 1935, see von Meyenn [158, vol. 2, p. 562].

[30] "Meine Bemühungen, die allgemeine Relativitätstheorie durch Verallgemeinerung der Gravitationsgleichungen zu vervollständigen, verdanken ihre Entstehung zum Teil der Vermutung, daß eine vernünftige allgemein relativistische Feldtheorie vielleicht den Schlüssel zu einer vollkommeneren Quantentheorie liefern könne. Dies ist eine bescheidene Hoffnung, aber durchaus keine Überzeugung." ([62, p. 17]), English translation by S. Linden and A. K. Hudert

> Quantum mechanics is certainly imposing. But an inner voice tells me that it is not yet the real thing. The theory says a lot, but does not really bring us any closer to the secret of the 'old one'. I, at any rate, am convinced that *He* is not playing at dice.[31]

As early as in May 1927, i.e., one month after having studied Heisenberg's paper on the uncertainty relations, Einstein gave a talk at the Prussian Academy of Sciences in Berlin where he discussed the question whether Schrödinger's wave mechanics describes the dynamics of a system completely or only in a statistical sense. Einstein withdrew the submitted manuscript before publication, but today it is available online [56].[32] In it, Einstein introduces an interpretation that shows some similarities to de Broglie's theory of pilot waves. But what is important for our purpose here is the fact that, already in 1927, Einstein was bothered by the question of the completeness of the quantum theory; also cf. Brown [32]. The EPR paper and his later writings clearly show that Einstein had accepted the statistical nature of quantum theory. But to him, it was only an expression of the theory's incompleteness. Determinism is not a presupposition of EPR's analysis (see the quote by John Bell towards the end of Sect. 5.2).

In his contribution to the Born Festschrift, Einstein later highlighted the importance of the classical limit [61]. He takes as an example a wave function that in the macroscopic realm corresponds to a superposition of macroscopically different momenta. So it does not describe a classical behaviour. (Schrödinger got to the heart of this situation in his famous cat argument, see further below.) As a consequence, according to Einstein, quantum mechanics can only tell the probability of finding a certain macroscopic state when measuring. This is what he wrote:

> The result of our consideration is this: The only acceptable interpretation of the Schrödinger equation is the statistical interpretation given by Born. However, this does not provide a real description of an individual system, but only statistical statements about ensembles of systems.[33]

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[31] "Die Quantenmechanik ist sehr achtung-gebietend. Aber eine innere Stimme sagt mir, daß das doch nicht der wahre Jakob ist. Die Theorie liefert viel, aber dem Geheimnis des Alten bringt sie uns kaum näher. Jedenfalls bin ich überzeugt, daß *der* nicht würfelt." ([69, English translation taken from Born, M. (ed.), Born, I. (trans.) (1971) *The Born-Einstein Letters.* New York: Walker and Company, p. 90])

[32] This is what Schrödinger referred to when he wrote to Einstein on July 13, 1935, i.e., a few days after the publication of the EPR paper: "True, we discussed those things much and heatedly in the seminars after you brought them up in Berlin years ago." ("Wir haben ja die Dinge, nachdem Du schon vor Jahren in Berlin darauf hingewiesen hattest, in den Seminaren viel und mit heißen Köpfen diskutiert.", [158], p. 551, English translation by S. Linden and A. K. Hudert)

[33] "Das Ergebnis unserer Betrachtung ist dieses. Die einzige bisherige annehmbare Interpretation der Schrödinger-Gleichung ist die von Born gegebene statistische Interpretation. Diese liefert jedoch keine Realbeschreibung für das Einzelsystem, sondern nur statistische Aussagen über System-Gesamtheiten." ([61, p. 40]), English translation by S. Linden and A. K. Hudert

It still took a long time to truly understand how to obtain the classical limit from quantum theory (see Sect. 5.4). Macroscopic wave functions necessarily become entangled with their environment's degrees of freedom, yielding a combined state that emulates classical behaviour for the variables of the macroscopic system. So Einstein's argument from 1953 is ineffective, because it assumes a strictly isolated state.

For the rest of his life, Einstein considered the quantum theory to be a statistical description of nature, similar to the statistical mechanics of the nineteenth century, that was to be replaced by a microscopic theory without such a fundamentally probabilistic character. Amongst other things, it was this belief that motivated his search for a unified field theory based on classical principles – an endeavour that was ultimately in vain.

of lanthanum is 7/2, hence the nuclear magnetic moment as determined by this analysis is 2.5 nuclear magnetons. This is in fair agreement with the value 2.8 nuclear magnetons determined from La III hyperfine structures by the writer and N. S. Grace.[9]

This investigation was carried out under the supervision of Professor G. Breit, and I wish to thank him for the invaluable advice and assistance so freely given. I also take this opportunity to acknowledge the award of a Fellowship by the Royal Society of Canada, and to thank the University of Wisconsin and the Department of Physics for the privilege of working here.

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[9] M. F. Crawford and N. S. Grace, Phys. Rev. 47, 536 (1935).

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Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?

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In a complete theory there is an element corresponding to each element of reality. A sufficient condition for the reality of a physical quantity is the possibility of predicting it with certainty, without disturbing the system. In quantum mechanics in the case of two physical quantities described by non-commuting operators, the knowledge of one precludes the knowledge of the other. Then either (1) the description of reality given by the wave function in quantum mechanics is not complete or (2) these two quantities cannot have simultaneous reality. Consideration of the problem of making predictions concerning a system on the basis of measurements made on another system that had previously interacted with it leads to the result that if (1) is false then (2) is also false. One is thus led to conclude that the description of reality as given by a wave function is not complete.

1.

ANY serious consideration of a physical theory must take into account the distinction between the objective reality, which is independent of any theory, and the physical concepts with which the theory operates. These concepts are intended to correspond with the objective reality, and by means of these concepts we picture this reality to ourselves.

In attempting to judge the success of a physical theory, we may ask ourselves two questions: (1) "Is the theory correct?" and (2) "Is the description given by the theory complete?" It is only in the case in which positive answers may be given to both of these questions, that the concepts of the theory may be said to be satisfactory. The correctness of the theory is judged by the degree of agreement between the conclusions of the theory and human experience. This experience, which alone enables us to make inferences about reality, in physics takes the form of experiment and measurement. It is the second question that we wish to consider here, as applied to quantum mechanics.

Whatever the meaning assigned to the term *complete*, the following requirement for a complete theory seems to be a necessary one: *every element of the physical reality must have a counterpart in the physical theory.* We shall call this the condition of completeness. The second question is thus easily answered, as soon as we are able to decide what are the elements of the physical reality.

The elements of the physical reality cannot be determined by *a priori* philosophical considerations, but must be found by an appeal to results of experiments and measurements. A comprehensive definition of reality is, however, unnecessary for our purpose. We shall be satisfied with the following criterion, which we regard as reasonable. *If, without in any way disturbing a system, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity.* It seems to us that this criterion, while far from exhausting all possible ways of recognizing a physical reality, at least provides us with one such way, whenever the conditions set down in it occur. Regarded not as a necessary, but merely as a sufficient, condition of reality, this criterion is in agreement with classical as well as quantum-mechanical ideas of reality.

To illustrate the ideas involved let us consider the quantum-mechanical description of the behavior of a particle having a single degree of freedom. The fundamental concept of the theory is the concept of *state*, which is supposed to be completely characterized by the wave function ψ, which is a function of the variables chosen to describe the particle's behavior. Corresponding to each physically observable quantity A there is an operator, which may be designated by the same letter.

If ψ is an eigenfunction of the operator A, that is, if

$$\psi' \equiv A\psi = a\psi, \tag{1}$$

where a is a number, then the physical quantity A has with certainty the value a whenever the particle is in the state given by ψ. In accordance with our criterion of reality, for a particle in the state given by ψ for which Eq. (1) holds, there is an element of physical reality corresponding to the physical quantity A. Let, for example,

$$\psi = e^{(2\pi i/h)p_0 x}, \tag{2}$$

where h is Planck's constant, p_0 is some constant number, and x the independent variable. Since the operator corresponding to the momentum of the particle is

$$p = (h/2\pi i)\partial/\partial x, \tag{3}$$

we obtain

$$\psi' = p\psi = (h/2\pi i)\partial\psi/\partial x = p_0\psi. \tag{4}$$

Thus, in the state given by Eq. (2), the momentum has certainly the value p_0. It thus has meaning to say that the momentum of the particle in the state given by Eq. (2) is real.

On the other hand if Eq. (1) does not hold, we can no longer speak of the physical quantity A having a particular value. This is the case, for example, with the coordinate of the particle. The operator corresponding to it, say q, is the operator of multiplication by the independent variable. Thus,

$$q\psi = x\psi \neq a\psi. \tag{5}$$

In accordance with quantum mechanics we can only say that the relative probability that a measurement of the coordinate will give a result lying between a and b is

$$P(a, b) = \int_a^b \bar{\psi}\psi dx = \int_a^b dx = b - a. \tag{6}$$

Since this probability is independent of a, but depends only upon the difference $b-a$, we see that all values of the coordinate are equally probable.

A definite value of the coordinate, for a particle in the state given by Eq. (2), is thus not predictable, but may be obtained only by a direct measurement. Such a measurement however disturbs the particle and thus alters its state. After the coordinate is determined, the particle will no longer be in the state given by Eq. (2). The usual conclusion from this in quantum mechanics is that *when the momentum of a particle is known, its coordinate has no physical reality.*

More generally, it is shown in quantum mechanics that, if the operators corresponding to two physical quantities, say A and B, do not commute, that is, if $AB \neq BA$, then the precise knowledge of one of them precludes such a knowledge of the other. Furthermore, any attempt to determine the latter experimentally will alter the state of the system in such a way as to destroy the knowledge of the first.

From this follows that either (1) *the quantum-mechanical description of reality given by the wave function is not complete* or (2) *when the operators corresponding to two physical quantities do not commute the two quantities cannot have simultaneous reality.* For if both of them had simultaneous reality—and thus definite values—these values would enter into the complete description, according to the condition of completeness. If then the wave function provided such a complete description of reality, it would contain these values; these would then be predictable. This not being the case, we are left with the alternatives stated.

In quantum mechanics it is usually assumed that the wave function *does* contain a complete description of the physical reality of the system in the state to which it corresponds. At first sight this assumption is entirely reasonable, for the information obtainable from a wave function seems to correspond exactly to what can be measured without altering the state of the system. We shall show, however, that this assumption, together with the criterion of reality given above, leads to a contradiction.

2.

For this purpose let us suppose that we have two systems, I and II, which we permit to interact from the time $t=0$ to $t=T$, after which time we suppose that there is no longer any interaction between the two parts. We suppose further that the states of the two systems before $t=0$ were known. We can then calculate with the help of Schrödinger's equation the state of the combined system I+II at any subsequent time; in particular, for any $t>T$. Let us designate the corresponding wave function by Ψ. We cannot, however, calculate the state in which either one of the two systems is left after the interaction. This, according to quantum mechanics, can be done only with the help of further measurements, by a process known as the *reduction of the wave packet*. Let us consider the essentials of this process.

Let $a_1, a_2, a_3, \cdots$ be the eigenvalues of some physical quantity A pertaining to system I and $u_1(x_1)$, $u_2(x_1)$, $u_3(x_1)$, $\cdots$ the corresponding eigenfunctions, where x_1 stands for the variables used to describe the first system. Then Ψ, considered as a function of x_1, can be expressed as

$$\Psi(x_1, x_2)=\sum_{n=1}^{\infty} \psi_n(x_2)u_n(x_1), \tag{7}$$

where x_2 stands for the variables used to describe the second system. Here $\psi_n(x_2)$ are to be regarded merely as the coefficients of the expansion of Ψ into a series of orthogonal functions $u_n(x_1)$. Suppose now that the quantity A is measured and it is found that it has the value a_k. It is then concluded that after the measurement the first system is left in the state given by the wave function $u_k(x_1)$, and that the second system is left in the state given by the wave function $\psi_k(x_2)$. This is the process of reduction of the wave packet; the wave packet given by the infinite series (7) is reduced to a single term $\psi_k(x_2)u_k(x_1)$.

The set of functions $u_n(x_1)$ is determined by the choice of the physical quantity A. If, instead of this, we had chosen another quantity, say B, having the eigenvalues b_1, b_2, b_3, $\cdots$ and eigenfunctions $v_1(x_1)$, $v_2(x_1)$, $v_3(x_1)$, $\cdots$ we should have obtained, instead of Eq. (7), the expansion

$$\Psi(x_1, x_2)=\sum_{s=1}^{\infty} \varphi_s(x_2)v_s(x_1), \tag{8}$$

where φ_s's are the new coefficients. If now the quantity B is measured and is found to have the value b_r, we conclude that after the measurement the first system is left in the state given by $v_r(x_1)$ and the second system is left in the state given by $\varphi_r(x_2)$.

We see therefore that, as a consequence of two different measurements performed upon the first system, the second system may be left in states with two different wave functions. On the other hand, since at the time of measurement the two systems no longer interact, no real change can take place in the second system in consequence of anything that may be done to the first system. This is, of course, merely a statement of what is meant by the absence of an interaction between the two systems. Thus, *it is possible to assign two different wave functions* (in our example ψ_k and φ_r) *to the same reality* (the second system after the interaction with the first).

Now, it may happen that the two wave functions, ψ_k and φ_r, are eigenfunctions of two noncommuting operators corresponding to some physical quantities P and Q, respectively. That this may actually be the case can best be shown by an example. Let us suppose that the two systems are two particles, and that

$$\Psi(x_1, x_2)=\int_{-\infty}^{\infty} e^{(2\pi i/h)(x_1-x_2+x_0)p}dp, \tag{9}$$

where x_0 is some constant. Let A be the momentum of the first particle; then, as we have seen in Eq. (4), its eigenfunctions will be

$$u_p(x_1)=e^{(2\pi i/h)px_1} \tag{10}$$

corresponding to the eigenvalue p. Since we have here the case of a continuous spectrum, Eq. (7) will now be written

$$\Psi(x_1, x_2)=\int_{-\infty}^{\infty}\psi_p(x_2)u_p(x_1)dp, \tag{11}$$

where

$$\psi_p(x_2)=e^{-(2\pi i/h)(x_2-x_0)p}. \tag{12}$$

This ψ_p however is the eigenfunction of the operator

$$P=(h/2\pi i)\partial/\partial x_2, \tag{13}$$

corresponding to the eigenvalue $-p$ of the momentum of the second particle. On the other hand, if B is the coordinate of the first particle, it has for eigenfunctions

$$v_x(x_1)=\delta(x_1-x), \tag{14}$$

corresponding to the eigenvalue x, where $\delta(x_1-x)$ is the well-known Dirac delta-function. Eq. (8) in this case becomes

$$\Psi(x_1, x_2)=\int_{-\infty}^{\infty}\varphi_x(x_2)v_x(x_1)dx, \tag{15}$$

where

$$\varphi_x(x_2)=\int_{-\infty}^{\infty}e^{(2\pi i/h)(x-x_2+x_0)p}dp$$
$$=h\delta(x-x_2+x_0). \tag{16}$$

This φ_x, however, is the eigenfunction of the operator

$$Q=x_2 \tag{17}$$

corresponding to the eigenvalue $x+x_0$ of the coordinate of the second particle. Since

$$PQ-QP=h/2\pi i, \tag{18}$$

we have shown that it is in general possible for ψ_k and φ_r to be eigenfunctions of two noncommuting operators, corresponding to physical quantities.

Returning now to the general case contemplated in Eqs. (7) and (8), we assume that ψ_k and φ_r are indeed eigenfunctions of some noncommuting operators P and Q, corresponding to the eigenvalues p_k and q_r, respectively. Thus, by measuring either A or B we are in a position to predict with certainty, and without in any way disturbing the second system, either the value of the quantity P (that is p_k) or the value of the quantity Q (that is q_r). In accordance with our criterion of reality, in the first case we must consider the quantity P as being an element of reality, in the second case the quantity Q is an element of reality. But, as we have seen, both wave functions ψ_k and φ_r belong to the same reality.

Previously we proved that either (1) the quantum-mechanical description of reality given by the wave function is not complete or (2) when the operators corresponding to two physical quantities do not commute the two quantities cannot have simultaneous reality. Starting then with the assumption that the wave function does give a complete description of the physical reality, we arrived at the conclusion that two physical quantities, with noncommuting operators, can have simultaneous reality. Thus the negation of (1) leads to the negation of the only other alternative (2). We are thus forced to conclude that the quantum-mechanical description of physical reality given by wave functions is not complete.

One could object to this conclusion on the grounds that our criterion of reality is not sufficiently restrictive. Indeed, one would not arrive at our conclusion if one insisted that two or more physical quantities can be regarded as simultaneous elements of reality *only when they can be simultaneously measured or predicted.* On this point of view, since either one or the other, but not both simultaneously, of the quantities P and Q can be predicted, they are not simultaneously real. This makes the reality of P and Q depend upon the process of measurement carried out on the first system, which does not disturb the second system in any way. No reasonable definition of reality could be expected to permit this.

While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question of whether or not such a description exists. We believe, however, that such a theory is possible.

Translation of Einstein's 1948 Paper

3

A. Einstein (1948), Quanten-Mechanik und Wirklichkeit, *Dialectica, 2*, 320–324. For the original publication please see: https://doi.org/10.1111/j.1746-8361.1948.tb00704.x

English translation by S. Linden and A. K. Hudert. Translation of Part II and the last three paragraphs are taken from Howard, D. (1985), Einstein on Locality and Separability. *Studies in History and Philosophy of Science*, **16**, 171–201. The English summary is part of Einstein's original paper.

Einstein's handwritten manuscript of this paper is available online: Einstein Archives Online, Archival Call Number 2–100.

3.1 Quantum Mechanics and Reality

In the following, I want to lay out briefly and in an elementary way, why I do not consider the method of quantum mechanics to be satisfying in principle. However, I want to point out straight away that I do not deny that this theory constitutes significant, in a certain sense even final, progress in physical knowledge. I imagine that this theory will be contained in a later theory just as ray optics is in wave optics: the relations will hold, but the foundation will be deeper or rather be replaced by a more comprehensive one.

I.

I picture a free particle at a certain moment in time to be (completely, in the sense of quantum mechanics) described by a spatially bounded ψ-function. According to that picture, neither the particle's momentum nor its position are determined with certainty.

C. Kiefer (ed.), *Albert Einstein, Boris Podolsky, Nathan Rosen*, Classic Texts in the Sciences, https://doi.org/10.1007/978-3-030-47037-1_3

Now in what sense shall I imagine that this picture represents a real, individual matter of fact? Two notions seem possible and obvious to me and we shall weigh them against each other:

(a) The (free) particle really has a definite position and a definite momentum, even though they cannot be both measured simultaneously in the same individual case. According to this notion, the ψ-function gives an *incomplete* description of a real physical situation.

This point of view is not the one physicists accept. Accepting it would call for a complete description of the physical situation in addition to the incomplete one and physical laws for such a description. This would be outside the scope of the theory of quantum mechanics.

(b) The particle really has neither a definite momentum nor a definite location; the description with a ψ-function is in principle a complete description. The definite position of the particle that I obtain measuring its position cannot be interpreted as the position of the particle *before* the measurement. The definite localisation that appears in the measurement is brought about only by the inevitable (not inessential) measurement intervention. The measurement result does not depend solely on the real particle situation but also on the nature of the measuring mechanism, which is (in principle) partially unknown. The same applies to the measurement of the momentum or any other observable related to the particle. This seems to be the interpretation physicists currently prefer; and, admittedly, that it is the only one that naturally satisfies the empirical situation expressed by Heisenberg's uncertainty relations within the framework of quantum mechanics.

According to this notion, two (essentially) different ψ-functions always describe two different real situations (e.g., the particle with definite position and the one with definite momentum).

The above also applies, *mutatis mutandis,* to the description of systems consisting of several mass points. Once again, we shall (in the sense of interpretation I*b*) assume that the ψ-function completely describes a real physical situation, and that two (essentially) different ψ-functions describe two different real physical situations, even though when submitted to a complete measurement, they may render identical measurement results; the identity of the measurement results is, to some extent, attributed to the partially unknown influence of the measurement system.

II.

If one asks what is characteristic of the realm of physical ideas independently of the quantum theory, then above all the following attracts our attention: the concepts of physics refer to a real external world, i.e., ideas are posited of things that claim a 'real existence' independent of the perceiving subject (bodies, fields, etc.), and these ideas are, on the other hand, brought into as secure a relationship as possible with sense impressions. Moreover, it is characteristic of these physical things that they are conceived of as being arranged in a space-time continuum. Further, it appears to be essential for this arrangement of the things introduced in physics that, at a specific time, these things claim an existence independent of one another, insofar as these things 'lie in different parts of space'. Without such an assumption of the mutually independent existence (the 'being-thus') of spatially distant things, an assumption which originates in everyday thought, physical thought in the sense familiar to us would not be possible. Nor does one see how physical laws could be formulated and tested without such a clean separation. Field theory has carried out this principle to the extreme, in that it localises within infinitely small (four-dimensional) space-elements the elementary things existing independently of one another that it takes as basic, as well as the elementary laws it postulates for them.

For the relative independence of spatially distant things (A and B), this idea is characteristic: an external influence on A has no *immediate* effect on B; this is known as the 'principle of local action', which is applied consistently only in field theory. The complete suspension of this basic principle would make impossible the idea of the existence of (quasi-) closed systems and, thereby, the establishment of empirically testable laws in the sense familiar to us.

III.

I now assert that the interpretation of quantum mechanics (according to I*b*) is incompatible with principle II.

We consider a physical system S_{12} that consists of two subsystems S_1 and S_2. The two subsystems may have physically interacted in the past. But we consider them at a time t when this interaction is over. The whole system shall be completely described in the sense of quantum mechanics by a ψ-function ψ_{12} of the coordinates $q_1..$ and $q_2..$ of the two subsystems (ψ_{12} cannot be represented as a product of the form $\psi(q_1..)\,\psi(q_2..)$ but only as a sum of such products). At a time t, the two subsystems shall be spatially separated so that ψ_{12} differs from *0* only when the $q_1..$ belong to a restricted part R_1 of space *and* the $q_2..$ belong to a different part R_2 of space.

The ψ-functions of the subsystems S_1 and S_2 then are initially unknown or they do not exist at all. It is true that the methods of quantum mechanics allow us to determine ψ_2 of S_2 from ψ_{12} if a (in the sense of quantum mechanics) complete measurement of the subsystem S_1 has been performed. One thus obtains the ψ-function ψ_2 of the subsystem S_2 instead of the initial ψ_{12} of S_{12}.

But for this determination it is essential which kind of (in the sense of quantum mechanics) complete measurement has been performed on subsystem S_1, i.e., which observables we measure. If S_1 is, for example, a single particle, then we decide whether we measure its position *or* its momentum components. Depending on that choice, we obtain different representations of ψ_{12}, which leads to, depending on the choice of measurement performed on S_1, different (statistical) predictions about the measurements to be performed later on S_2. From the point of view of interpretation I*b,* this means that, depending on the choice of the complete measurement on S_1, different real situations with respect to S_2 are created that themselves are described by different ψ_2, $\underline{\psi_2}$, $\underline{\underline{\psi_2}}$, etc.

From the point of view of quantum mechanics *alone,* this poses no difficulty. Depending on the choice of measurement on S_1, different real situations are created, and there is never a need for two or more different ψ-functions ψ_2, $\underline{\psi}_2$... to be attributed to the same system S_2.

But it is a different matter if one aspires to simultaneously adhere to the principles of quantum mechanics and to principle II concerning the mutually independent existence of real physical situations in two separated regions R_1 and R_2 of space. In our example, the complete measurement on S_1 represents a physical intervention that only concerns region R_1. Such an intervention can have no immediate effect on the spatially distant region R_2. Because from this would follow that every statement about S_2 that we can make due to complete measurements we performed on S_1, would still necessary hold for S_2 even if no measurement was performed on S_1. This would mean, that all statements about S_2 that can be deduced from positing ψ_2 or $\underline{\psi}_2$, etc. would be simultaneously true. This, of course, is impossible, when ψ_2, $\underline{\psi}_2$, etc. are supposed to represent different real physical situations of S_2, i.e., one comes into conflict with interpretation I*b* of the ψ-function.

It appears to me, there can be no doubt that the physicists who hold the quantum-mechanical manner of description to be, in principle, definitive, will react to these considerations as follows: They will drop requirement II of the independent existence of the physical realities which are present in different portions of space; they can rightly appeal to the fact that the quantum theory nowhere makes explicit use of this requirement.

I grant this, but note if I consider the physical phenomena with which I am acquainted, and especially those which are so successfully comprehended by means of quantum mechanics, then, nevertheless, I nowhere find a fact which makes it appear to me provable that one has to give up requirement II. For that reason I am inclined to believe that the description afforded by quantum mechanics is to be viewed, in the sense of I*a*, as an incomplete description of reality, that will again be replaced later by a complete and direct description.

In any case, one should be on guard, in my opinion, against committing oneself dogmatically to the schema of current theory in the search for a unified basis for the whole of physics.

A. Einstein

3.2 Summary

If, in quantum mechanics, we consider the Ψ-function as (in principle) a complete description of a real physical situation we thereby imply the hypothesis of action-at-distance, a hypothesis which is hardly acceptable. If, on the other hand, we consider the Ψ-function as an incomplete description of a real physical situation, then it is hardly to be believed that, for this incomplete description, strict laws of temporal dependence hold.

Reception and Impact of the EPR Paper

4

Probably even the authors did not foresee the impact their 1935 paper would have on the debate about the meaning of quantum theory. Astonishingly, it still influences the debate today. In this chapter we therefore give a historic account of its impact and reception. For two reasons, special attention will be given to Bohr's reply to EPR's work. On the one hand, it exemplarily shows the conceptual difficulties associated with the debate. On the other hand, it was historically the most important reaction to EPR's work, because many physicists considered Bohr to be *the* authority in the field of quantum theory that was not to be challenged. Many, if not most physicists followed Bohr without criticism and did not really bother to read EPR's and Bohr's original works. But as Mara Beller, in particular, has pointed out, Bohr's 'victory' over Einstein is but a legend and not based on facts [17, p. 151*f*]. According to Arthur Fine, the 'EPR paradox' is a paradox first and foremost if one adopts the Copenhagen interpretation of quantum theory [73, p. 4*f*]. This interpretation, we recall, had been formulated by Heisenberg and especially by Bohr, who thought of himself as this interpretation's creator, in the years after 1925, and had been dismissed by Einstein as "Heisenberg-Bohr tranquillising philosophy" as early as in 1928 in a letter to Schrödinger.[1] So let us first have a look at Bohr's paper.

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[1] "The Heisenberg-Bohr tranquillising philosophy – or religion? – is so delicately concocted that it provides a gentle pillow for the believer from which he cannot be aroused that easily. So let him lie there." ("Die Heisenberg-Bohrsche Beruhigungsphilosophie – oder Religion? – ist so fein ausgeheckt, daß sie dem Gläubigen einstweilen ein sanftes Ruhekissen liefert, von dem er nicht so leicht sich aufscheuchen läßt. Also lasse man ihn liegen.", von Meyenn [158, p. 459], English translation taken from Capellmann, H. (2017), *The Development of Elementary Quantum Theory*, Springer International Publishing, Cham, p. 63).

C. Kiefer (ed.), *Albert Einstein, Boris Podolsky, Nathan Rosen*, Classic Texts in the Sciences, https://doi.org/10.1007/978-3-030-47037-1_4

4.1 Reprint of Bohr's Paper

Can Quantum-Mechanical Description of Physical Reality Be Considered Complete? N. Bohr, Physical Review, Volume 48, Page 696–702, published in 1935 by the American Physical Society. Reprinted with permission https://doi.org/10.1103/PhysRev.48.696

4.2 Bohr's Reply

Bohr was very much alarmed by the EPR paper, which becomes apparent in this short account by his student Léon Rosenfeld [138]:

> This onslaught came down upon us as a bolt from the blue. Its effect on Bohr was remarkable. [...] as soon as Bohr had heard my report of Einstein's argument, everything else was abandoned: we had to clear up such a misunderstanding at once. We should reply by taking up the same example and showing the right way to speak about it.

Apparently, Bohr and his devoted student Rosenfeld were not interested in an open discussion, but rather in clearing up what according to them was a misunderstanding in the EPR paper.

At first, Bohr published a short response, only one page long, in *Nature* [27]. Even on the day of its publication, Schrödinger wrote to Einstein: "I was furious about N. Bohr's letter to Nature from July 13. He *only* makes you curious, does not reveal at all what he is talking about, and refers to a paper that is to appear in Physical Review."[2] The paper Bohr had announced in *Nature* was indeed submitted to *Physical Review* on the same day and was published on October 15, 1935. Comprising six pages in its original version, Bohr's paper is not long but two pages longer than the paper he criticised.

Bohr's paper is not a prime example of clarity.[3] Mara Beller brought up the following quite amusing fact [16]. Most commentators refer to the reprint of Bohr's article in the collective volume dealing with the foundations of quantum theory edited by Wheeler and Zurek [164]. In this reprint, however, pages 700 and 699 were interchanged.[4] Nobody ever noticed. Indeed, when reading the paper in the wrong order of pages, one does not get a significantly different impression than when reading the original. The author seems

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[2] "Wutgeschnaubt habe ich über N. Bohrs Naturebrief vom 13. Juli. Er macht einen *nur* neugierig, verrät nicht mit einem Wort, was er meint, und verweist auf einen Artikel, der im Physical Review kommen wird." (von Meyenn [158, p. 552], English translation by S. Linden and A. K. Hudert)

[3] Cf. Schrödinger's remark in a letter to Born: "The eminent physicist Niels Bohr is being eminently overrated as 'philosopher scientist' by his fellow-physicists." ("Der eminente Physiker Niels Bohr wird als 'Philosopher-Scientist' von Seiten seiner Physikerkollegen eminent überschätzt.", von Meyenn [158, vol. 2, p. 665], English translation by S. Linden and A. K. Hudert)

[4] Page numbers referring to the original article [28].

to have known that his paper was incomprehensible. He later wrote about it: "Rereading these passages, I am deeply aware of the inefficiency of expression which must have made it very difficult to appreciate the trend of the argumentation [. . .]."[5] Is it still possible to extract the core messages from Bohr's paper?

Already the introductory lines include two points that were essential to Bohr: EPR's 'criterion of physical reality' and the concept of complementarity, introduced by Bohr. According to the author, the application of complementarity will entail the completeness of the quantum mechanical description. In his paper, Bohr especially attacks EPR's criterion of reality, although it did not play a central role in the EPR paper as we have seen above. Naturally, Bohr felt especially provoked by the passage "without in any way disturbing a system". After all, when formulating the earlier version of the Copenhagen interpretation, it was indispensable to assume a necessary disturbance of the measured system by the measurement apparatus. This unavoidable disturbance had followed from Heisenberg's thought experiments concerning the uncertainty relations.

Now in the first part of his paper, Bohr covers the example of the double slit, as he did in the discussions during the Solvay Conference of 1927, which has little to do with the EPR paper. Although Bohr accepted their thought experiment, he did not agree with their interpretation, which he then replaced with his own. This happens in the second part of his paper, which is also where his notion of complementarity comes fully into play. While in Como in 1927 Bohr had talked about complementarity of causality and space-time description, he now applied the complementarity to the measurement apparatus. Since the measurements of position and momentum exclude each other, and thus are 'complementary' to each other, neither position and momentum of the measured particle, nor the position and momentum of the distant particle as calculated from the information on the first particle, can have simultaneous reality. Bohr wrote (p. 700 of the paper reprinted in this book, italics by Bohr):

> Of course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation [Bohr means the second, distant particle, C.K.] during the last critical stage of the measuring procedure. But even at this stage there is essentially the question of *an influence on the very conditions which define the possible types of predictions regarding the future behaviour of the system.* Since these conditions constitute an inherent element of the description of any phenomenon to which the term "physical reality" can be properly attached, we see that the argument of the mentioned authors does not justify their conclusion that quantum-mechanical description is essentially incomplete. [. . .] It is just this entirely new situation as regards the description of physical phenomena, that the notion of *complementarity* aims at characterising.

Evidently, EPR did not directly claim that position and momentum have simultaneous reality, although this seems to follow implicitly from their argumentation. EPR agreed with Bohr that position and momentum of the first particle cannot be measured simultaneously

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[5] Bohr [29, p. 234].

and that therefore position and momentum of the second particle cannot be calculated simultaneously. EPR only concluded that different wave functions can be ascribed to the same reality, and therefore the description of reality with wave functions is not unique and quantum theory not complete. But Bohr did not mention wave functions at all! So in his reply, Bohr missed the essential message of the EPR paper. Instead, he assigned a word to the situation in question – complementarity.

In her book, Mara Beller attentively analyses Bohr's paper and sees two contradicting voices coming to light [17, chap. 7]. One voice expresses Bohr's point of view from before the EPR paper. According to it, a measurement always corresponds to a direct physical disturbance of the measured system by the measurement apparatus. After the publication of the EPR paper, such a point of view could not be maintained, because the second particle, by assumption, cannot be disturbed – at least not mechanically, as Bohr specified in the above quoted section. The second voice expresses a positivist attitude. Only what can be measured simultaneously has simultaneous reality; there is no objective reality independent of observations. It is this second point of view that Bohr would maintain the rest of his life. Beller accurately describes it as the transition of a physical disturbance of the system into a semantic disturbance of a system – the semantic disturbance being the above quoted "influence on the very conditions which define the possible types of predictions regarding the future behaviour of the system."

What mattered to Bohr in his discussions with Einstein from 1927 to 1930 was to also apply the uncertainty relations to the measurement apparatus. So the measurement apparatus also became a quantum mechanical system. After 1935, Bohr no longer held that opinion. From then on he emphasised the fundamental difference between the nature of atomic objects and the nature of measurement apparatus. The latter always need to be described classically. According to Beller, it was this doctrine of the necessity of classical notions in the macroscopic realm that underlie Bohr's philosophy of complementarity. For Beller, complementarity is but a metaphor [17, p. 243*f*]:

> Complementarity is not a rigorous guide to the heart of the quantum mystery. Nor do Bohr's numerous analogies between quantum physics and other domains, such as psychology or biology, withstand close scrutiny. Complementarity does not reveal preexisting similarities; it generates them. Complementarity builds new worlds by making new sets of associations. These worlds are spiritual and poetic, not physical. Complementarity did not result in any new physical discovery – "it is merely a way to talk about the discoveries that have already been made" (interview with Dirac, Archive for the History of Quantum Physics).

Beller was right to point out that the asserted necessity of classical concepts is vague, historically as well as philosophically. According to Beller, this view ignores the huge gap between Aristotle's direct intuition and the abstract framework of Newton's (and Einstein's) physics. Following Fine, Bohr was the more conservative one in the Einstein and Bohr debate, because he absolutely wanted to keep the old (classical) notions, whereas Einstein subjected them to a critical examination; Bohr viewed the world through classical glasses [73, p. 19*f*]. As Whitaker pointed out, the assumptions underlying the idea of complementarity prohibit the kind of argument that EPR used, because alternative measurements may not be taken into account [165, p. 1335*f*].

The notion of complementarity in its positivist formulation and the necessity of the classical concepts when describing the measurement apparatus constitute the core of what is known today as the Copenhagen interpretation.[6] This is why EPR's argumentation constitutes such a problem for the followers of this interpretation. But other authors had their problems with EPR as well, as we shall see in the next section.

4.3 Schrödinger and Entanglement

Erwin Schrödinger, the father of wave mechanics, was especially interested in the conceptual questions raised by EPR. In reaction to the EPR paper, he published a number of articles in 1935 and 1936, detailing his point of view on quantum mechanics [145–147]; in a footnote in one of these articles he openly admitted: "The appearance of this work [the EPR paper, C. K.] motivated the present – shall I say lecture or general confession?"[7]

In his general confession, Schrödinger introduced a notion that today is considered to be *the* central element of quantum theory – *entanglement.* Modern research areas like quantum information are inconceivable without an extensive discussion of properties of entangled systems. De facto, entangled states had already been discussed before 1935, for example in the above quoted works by Hylleraas [93, 94].

An entanglement between quantum mechanical systems (like the two particles in the EPR paper) generally occurs when these systems interact. The wave function of the combined system cannot be expressed as a product of two wave functions that correspond to one of the subsystems each; this does not change even when the subsystems are being separated by so far a distance that an exchange of information is no longer possible. Schrödinger wrote:

> Maximal knowledge of a combined system does not necessarily include maximal knowledge of all its parts, not even when these are fully separated from each other and at the moment are not influencing each other at all.[8]

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[6] "Bohr's reply to EPR has come down to us as the so-called *Copenhagen interpretation* of quantum mechanics." [149, p. 539]; "The Copenhagen interpretation, and its rhetoric of inevitability, rests on two central pillars – positivism and the doctrine of the necessity of classical concepts." [17, p. 205]

[7] "Das Erscheinen dieser Arbeit [EPR, C. K.] gab den Anstoß zu dem vorliegenden – soll ich sagen Referat oder Generalbeichte?" (Schrödinger [146, p. 845], English translation by J. D. Trimmer)

[8] "*Maximale Kenntnis von einem Gesamtsystem schließt nicht notwendig maximale Kenntnis aller seiner Teile ein, auch dann nicht, wenn dieselben völlig voneinander abgetrennt sind und einander zur Zeit gar nicht beeinflussen.*" (Schrödinger [146, p. 826], English translation following J. D. Trimmer, italics by Schrödinger)

Following Schrödinger, maximal knowledge of a quantum mechanical system is obtained by knowledge of its wave function ψ, which in the case of an entangled system is known only for the combined system, but nor for the constituting subsystems. Entanglement occurs naturally when two systems interact:

> If two separated bodies, each by itself known maximally, enter a situation in which they influence each other, and separate again, then there occurs regularly that which I have just called *entanglement* of our knowledge of the two bodies.[9]

Different to our usage of the term entanglement today, Schrödinger here spoke about an entanglement of knowledge. This is due to his interpretation of the wave function as an 'expectation-catalog' and not as the dynamically relevant state that can be understood in a specific realistic sense. For Schrödinger, the entanglement of the subsystems was mainly a correlation of probabilities, as is already highlighted in the titles of his papers [145, 147].

Shortly after the publication of the EPR paper, an intense exchange of letters set in between Schrödinger an Einstein. We already talked about this above (Sect. 2.5). In these letters, some of the topics of Schrödinger's 1935 papers are anticipated. Most notably they already contained the notorious cat-example, known today as Schrödinger's cat, that was later printed in Schrödinger [146, p. 812]. In his letter to Einstein on August 19, 1935, Schrödinger wrote:

> I am long past the stage where I thought that one can consider the ψ-function as somehow a direct description of reality. [...] Confined in a steel chamber is a Geigercounter prepared with a tiny amount of uranium, so small that in the next hour it is just as probable to expect *one* atomic decay as none. An amplifying relay provides that the first decay shatters a small bottle of prussic acid. This and – cruelly – a cat is also trapped in the steel chamber. According to the ψ-function for the total system, after an hour, *sit venia verbo,*[10] the living and dead cat are smeared out in equal measure.[11]

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[9] "Wenn zwei getrennte Körper, die einzeln maximal bekannt sind, in eine Situation kommen, in der sie aufeinander einwirken, und sich wieder trennen, dann kommt regelmäßig das zustande, was ich eben *Verschränkung* unseres Wissens um die beiden Körper nannte." (Schrödinger [146, p. 827], English translation by J. D. Trimmer)

[10] 'Pardon the expression!', following Plinius, *Epistulae* **5**, 6, 46.

[11] "Ich bin längst über das Stadium hinaus, wo ich mir dachte, daß man die ψ-Funktion irgendwie direkt als Beschreibung der Wirklichkeit ansehen kann. [...] In einer Stahlkammer ist ein Geigerzähler eingeschlossen, der mit einer winzigen Menge Uran beschickt ist, so wenig, daß in der nächsten Stunde ebenso wahrscheinlich *ein* Atomzerfall zu erwarten ist wie keiner. Ein verstärkendes Relais sorgt dafür, daß der erste Atomzerfall ein Kölbchen mit Blausäure zertrümmert. Dieses und – grausamer Weise – eine Katze befinden sich auch in der Stahlkammer. Nach einer Stunde sind dann in der ψ-Funktion des Gesamtsystems, sit venia verbo, ['Man verzeihe den Ausdruck!'] die lebende und die tote Katze zu gleichen Teilen verschmiert." (von Meyenn [158, p.566], English translation taken from Fine, A. [73], *The Shaky Game,* Second edition, University of Chicago Press, Chicago, p. 82–83)

The situation of Schrödinger's cat is a macroscopic superposition of quantum states that exhibits non-classical properties. The example with the coupling to a radioactive is meant to illustrate how such states occur naturally when one extends the quantum mechanical formalism to macroscopic areas. To Schrödinger, this thought experiment proves the interpretation of ψ as a mere expectation-catalogue. Only the understanding of the classical limit through decoherence (Sect. 5.4), that was reached much later, shows why the state of Schrödinger's cat can correspond to reality. Einstein, in a letter to Schrödinger on September 4, 1935, noted in reference to the cat-example:

> As for the rest, your cat shows that we are in complete agreement concerning our assessment of the character of the current theory. A ψ-function that contains the living as well as the dead cat just cannot be taken as a description of a real state of affairs. To the contrary, this example shows exactly that it is reasonable to let the ψ-function correspond to a statistical ensemble that contains both systems with live cats and those with dead cats.[12]

Einstein would highlight this point in later letters, too.

In quantum optics one today speaks of 'cat-states' when coherent states of ions or atoms are superposed. Serge Haroche (Ecole Normale Supérieure, Paris, France) and David Wineland (National Institute of Standards and Technology, Boulder, USA) are pioneers in this research area and report on it in their Nobel lectures [84, 170].[13] Preparing this kind of states is an important prerequisite for experiments concerning the behaviour in the classical limit, see Sect. 5.4 further below.

Einstein and Schrödinger would go on to discuss these fundamental questions until Einsteins's death, without ever finding a consensus.[14] For Einstein it was unthinkable that the ψ-function directly describes the physical reality, beyond a purely statistical description. In his last letters to Schrödinger and Born, he emphasised the role of the superposition principle and the resulting 'fuzziness' of macroscopic states, herein differing from what he said directly after the EPR paper, also cf. Einstein [61]. On March 22, 1953, Einstein wrote to Schrödinger:

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[12] "Übrigens zeigt Dein Katzenbeispiel, daß wir bezüglich der Beurteilung des Charakters der gegenwärtigen Theorie völlig übereinstimmen. Eine ψ-Funktion, in welche sowohl die lebende wie die tote Katze eingeht, kann eben nicht als Beschreibung eines wirklichen Zustandes aufgefaßt werden. Dagegen weist gerade dies Beispiel darauf hin, daß es vernünftig ist, die ψ-Funktion einer statistischen Gesamtheit zuzuordnen, welche sowohl Systeme mit lebendiger Katze wie solche mit toter Katze in sich begreift." (von Meyenn [158, p. 569], English translation taken from Fine, A. [73], *The Shaky Game,* Second edition, University of Chicago Press, Chicago, p. 84)

[13] Other experiments are concerned with 'quantum-cheshire-cat'-states (Denkmayr et al. 2015). These are interference experiments with neutrons, where the system acts as if the neutron follows a trajectory different from the trajectory its magnetic moment follows. However, the interpretation of the results is still under discussion (Corrêa et al. 2014).

[14] See the correspondence in von Meyenn [158].

> I do not understand at all the *analogy* between the uncertainty of the general ψ function and the difficulty this creates to consider it a description of physical reality on the one hand, and a thermodynamical description on the other hand.[15] The essence of quantum theory, after all, is that the ψ function obeys a *linear* equation. This has been explicitly arranged so that the sum of two ψ functions is again a ψ function (a solution). All the solutions obtained by such summations are *per se* coequal and thus represent, according to your interpretation, possible real cases that are to be treated as coequal in the theory. It therefore seems to me that in such a theory the quasi-sharpness of positions and momenta of a system as a whole cannot exist. Because the superposition of quasi-sharp states creates arbitrarily fuzzy macroscopic systems (ψ functions), in whose real existence, in the sense of your interpretation, no man can believe. I am convinced that only the statistical interpretation can overcome this difficulty.[16]

At around the same time, Einstein voiced the same line of reasoning in letters to Max Born [69], who, just like Schrödinger, missed the root of the matter. Applying the superposition principle, according to which the sum of two physically reasonable ψ functions again constitutes a physically reasonable ψ function, necessarily yields 'fuzzy' macroscopic states like Schrödinger's cat that have never been observed. Einstein's proposition of interpreting the wave function merely statistically offers a way out of this paradox. But we will see further below this way out is not necessary, because the application of quantum theory to *realistic systems* makes it possible to understand the non-appearance of macroscopic superpositions within the framework of a realistic interpretation of the wave function.

The problem of the macroscopic superpositions also weighed heavy on Wigner's mind. In his famous paper 'Remarks on the Mind-Body Question' he speculated that only the human consciousness is responsible for the wave function collapse and the fact that 'fuzzy' states have never been observed. He wrote [168, p. 176]: "It follows that the quantum description of objects is influenced by impressions entering my consciousness."

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[15] Schrödinger had drawn this analogy in an earlier letter by comparing the non-appearance of 'fuzzy' solutions of the wave equation with the observation that most systems are not in thermodynamical equilibrium although this should be expected from entropic considerations (cf. von Meyenn [158, vol. 2, p. 677]).

[16] "Die *Analogie* zwischen der Unschärfe der allgemeinen ψ-Funktion und der durch sie geschaffenen Schwierigkeit, die ψ-Funktion als Beschreibung der physikalischen Realität aufzufassen einerseits und der thermodynamischen Beschreibung andererseits, verstehe ich gar nicht. Der Witz der Quantentheorie liegt doch darin, daß die ψ-Funktion einer *linearen* Gleichung unterliegt. Dies hat man doch eigens so eingerichtet, damit die Summe zweier ψ-Lösungen wieder eine ψ-Funktion (Lösung) ist. Alle durch solche Summenbildung einheitlichen Lösungen sind an sich gleichberechtigt und stellen also im Sinne Deiner Interpretation theoretisch gleichberechtigte mögliche reale Sonderfälle dar. Deshalb erscheint es mir, daß in einer solchen Theorie die Quasi-Schärfe der Lagen und Impulse des Systems als Ganzes nicht existieren kann. Denn durch Superposition von quasi-scharfen Zuständen entstehen makroskopisch beliebig unscharfe Systeme (ψ-Funktionen), an deren physikalische Existenz im Sinne Deiner Interpretation doch kein Mensch glauben kann. Ich bin davon überzeugt, daß nur die statistische Interpretation diese Schwierigkeit überwinden kann." (von Meyenn [158, vol. 2, p. 679], English translation by S. Linden and A. K. Hudert)

He later abandoned this thought under the impression of Zeh's work [173] that showed that macroscopic objects act classically due to unavoidable interactions with their environment, see Wigner [169, p. 240]. This phenomenon called *decoherence* will play a central role in the debate on the interpretation of quantum theory, see Sect. 5.4.

4.4 Pauli and Heisenberg

Wolfgang Pauli reacted to the EPR paper in his habitual way, i.e., harshly. Already on June 15, 1935, he wrote to Heisenberg:

> *Einstein* once again commented publicly on quantum mechanics, this time in Physical Review on May 15 (together with Podolsky and Rosen – no good company, by the way). As is well known this is a catastrophe every time it happens. "For – he keenly concludes – that which must not, cannot be." (Morgenstern).
> At least I want to concede to him that if an undergraduate student came to me with such objections, I would think him quite intelligent and promising. – Since this publication risks confusing the public opinion – namely in America – , I would suggest to send a reply to Physical Review, something I wish to encourage *you* to do.[17]

As far as Pauli was concerned, the interpretation of quantum mechanics was just about pedagogical questions. In his letter, he fundamentally attacked EPR's assumption of separability. Because, according to Pauli, you can only assume this if you are dealing with a very special state, namely a state that is a product with respect to the subsystems. He therefore is not surprised that you run into contradictions when neglecting this and instead conceive 'hidden properties' of an un-measured system. In the above-quoted excerpt of his letter, Pauli encourages Heisenberg to publish a riposte to the EPR paper in order to clarify those issues.

Heisenberg was willing to write such a riposte. In his response to Pauli on July 2, 1935, he mentioned that Bohr planned an answer to EPR, but that this answer would differ very much from his own points of view [128, p. 407*f*]. In his summer vacation 1935, Heisenberg wrote a manuscript and sent it to some of his colleagues (among them, Bohr). However, he never published it, maybe due to the fact that in the meantime a whole

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[17] "*Einstein* hat sich wieder einmal zur Quantenmechanik öffentlich geäußert und zwar im Heft des Physical Review vom 15. Mai (gemeinsam mit Podolsky und Rosen – keine gute Kompanie übrigens). Bekanntlich ist das jedes Mal eine Katastrophe, wenn es geschieht. 'Weil, so schließt er messerscharf – nicht sein kann, was nicht sein darf.' (Morgenstern). Immerhin möchte ich ihm zugestehen, daß ich, wenn mir ein Student in jüngeren Semestern solche Einwände machen würde, diesen für ganz intelligent und hoffnungsvoll halten würde. – Da durch die Publikation eine gewisse Gefahr einer Verwirrung der öffentlichen Meinung – namentlich in Amerika – besteht, so wäre es vielleicht angezeigt, eine Erwiderung darauf ans Physical Review zu schicken, wozu ich *Dir* gerne zureden möchte." (Pauli [128, p. 402], English translation by S. Linden and A. K. Hudert)

number of ripostes to the EPR paper had been published. The manuscript's title reads "Is a deterministic completion of quantum mechanics possible?". It was published only posthumously in Pauli [128, p. 409–418].[18]

Already the manuscript's title highlights Heisenberg's intention to focus on the incompleteness of quantum theory that played such a central role in the EPR paper. He goes on to show that such a deterministic completion is impossible, i.e., in contradiction to the experimental successes of quantum mechanics. Heisenberg emphasises that the wave function is defined in a configuration space of higher dimension whereas observations take place in space and time. He therefore asks: "At what place should one draw the cut between the description by wave functions and the classical-anschaulich description?"[19] His answer being: "*The quantum mechanical predictions about the outcome of an arbitrary experiment are independent of the location of the cut just discussed.*"[20] So the place of the Heisenberg cut (later so named) is, to a certain degree, arbitrary; except, it must remain far enough away from the system to be measured in order to avoid coming into conflict with the system's observed quantum properties, e.g., interference.

Heisenberg then concludes as follows. Let us assume there exist hidden variables that describe the time evolution beyond the cut. At the place of the cut, and only there, they should contain the transition from a description by wave functions to a statistical interpretation. The place of the cut being arbitrary, this cannot be the case, Heisenberg says. Bacciagaluppi and Crull [5] mention that Heisenberg had turned against the concept of hidden variables even earlier than in this manuscript, because their existence would contradict the observed quantum mechanical phenomenon of interference.

In his letter to Pauli, Heisenberg mentioned an essay by philosopher Grete Hermann (1901–1984) on the subject of incompleteness of quantum mechanics, wherein Hermann exposed a circularity in von Neumann's proof of impossibility of hidden variables [90].[21] This will be discussed further below.

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[18] An extensive study of this manuscript can be found in Bacciagaluppi and Crull [5].

[19] "An welcher Stelle soll der Schnitt zwischen der Beschreibung durch Wellenfunktionen und der klassisch-anschaulichen Beschreibung gezogen werden?" (Pauli [128, p. 411], English translation taken from Crull, E. and Bacciagaluppi, G. (2011), Translation of: W. Heisenberg, 'Ist eine deterministische Ergänzung der Quantenmechanik möglich?' <halshs-00996315>, p. 9) Crull and Bacciagaluppi did not translate the German word 'anschaulich'. 'Classical-anschaulich' means something like 'behaving in a classical manner and being depictable to the mind by means of accustomed concepts.'

[20] "*Die quantenmechanischen Voraussagen über den Ausgang irgendeines Experimentes sind unabhängig von der Lage des besprochenen Schnitts.*"

[21] Hermann [89] is an extract from this essay. For more on Grete Hermann's work, cf. Soler [151].

4.5 Some More Early Responses

Maybe the earliest printed response to the EPR paper is the one by American physicist Edwin C. Kemble (1889–1984), cf. Kemble [102]. Schrödinger noted [158, p. 551*f*]: "What I understand least is the paper by E. C. Kemble in Physical Review from June 15 – he doesn't even mention the case that causes us a headache. It's as if one is saying: It's bitterly cold in Chicago, and someone's answering: That's a false conclusion, it's very hot in Florida." Indeed, Kemble's criticism misses the point of the EPR paper. He simply claims that a merely statistical interpretation of the wave function suffices to avoid paradoxes. Obviously, Einstein himself had concluded this, but was not ready to accept a merely statistical interpretation (i.e., without an explaining ensemble of fundamental physical objects) and therefore concluded the incompleteness of the theory.

In contrast, American physicist Arthur E. Ruark (1899–1979) used another criterion of reality in his response [139]. According to his criterion, a physical property of a physical system only has reality if and when it is measured. In this respect, his position is close to Bohr's, whose work, however, had not been published yet at the time. Ruark drew the somewhat evasive conclusion, that given current knowledge a decision was impossible, because one could not know which criterion made more sense.

Wendell H. Furry (1907–1984), another American physicist, took Bohr's side in his response, but made use of wave functions in his argumentation [75]. He formulated an 'assumption A', according to which a system, when interacting with another system, non-causally evolves into a state with a definite wave function. Following the interaction, the total system is then represented by a product of two wave functions (one for the first, one for the second system). This separation occurs without measurement and thus has nothing to do with an alleged collapse of the wave function during the measurement, according to which a measurement should, with some given probability, result in a certain state. Furry then showed explicitly that his assumption A contradicts Schrödinger's equation. In a short supplement to his paper [76], Furry commented on the articles by Schrödinger that had come out in the meantime [145, 146]. Furry underlined, that while Schrödinger's mathematical approach resembled his own, he had come to opposing conclusions. Schrödinger rejected assumption A and joined in on EPR's criterion of reality. Furry commented [76]:

> Thus there can be no doubt that quantum mechanics requires us to regard the realistic attitude as in principle inadequate.

By this, he meant EPR's criterion of local reality. Because:

> No matter how far apart the particles are when we try to collect one of them, the relative probabilities of finding it in different places are strongly affected by the 'interference term' in the cross section; it is not really 'free'.

In contrast to this, Schrödinger concludes the incompleteness of quantum theory, but in another way than EPR; he sees more of a problem in the fact that the theory only allows predictions for a "sharply-defined time."[22] But Furry had spotted the crucial point: The reality described by quantum theory is non-local. Bohm and Aharonov [25] referred to an actual experiment contradicting Furry's assumption A (also cf. Whitaker [166, p. 155*f*]). Assumption A thus holds no solution to the problem EPR had raised; the entanglement between two subsystems after their interaction is real.

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[22] "scharf bestimmte Zeitpunkte" [146, p. 848].

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Can Quantum-Mechanical Description of Physical Reality be Considered Complete?

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It is shown that a certain "criterion of physical reality" formulated in a recent article with the above title by A. Einstein, B. Podolsky and N. Rosen contains an essential ambiguity when it is applied to quantum phenomena. In this connection a viewpoint termed "complementarity" is explained from which quantum-mechanical description of physical phenomena would seem to fulfill, within its scope, all rational demands of completeness.

IN a recent article[1] under the above title A. Einstein, B. Podolsky and N. Rosen have presented arguments which lead them to answer the question at issue in the negative. The trend of their argumentation, however, does not seem to me adequately to meet the actual situation with which we are faced in atomic physics. I shall therefore be glad to use this opportunity to explain in somewhat greater detail a general viewpoint, conveniently termed "complementarity," which I have indicated on various previous occasions,[2] and from which quantum mechanics within its scope would appear as a completely rational description of physical phenomena, such as we meet in atomic processes.

The extent to which an unambiguous meaning can be attributed to such an expression as "physical reality" cannot of course be deduced from *a priori* philosophical conceptions, but—as the authors of the article cited themselves emphasize—must be founded on a direct appeal to experiments and measurements. For this purpose they propose a "criterion of reality" formulated as follows: "If, without in any way disturbing a system, we can predict with certainty the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity." By means of an interesting example, to which we shall return below, they next proceed to show that in quantum mechanics, just as in classical mechanics, it is possible under suitable conditions to predict the value of any given variable pertaining to the description of a mechanical system from measurements performed entirely on other systems which previously have been in interaction with the system under investigation. According to their criterion the authors therefore want to ascribe an element of reality to each of the quantities represented by such variables. Since, moreover, it is a well-known feature of the present formalism of quantum mechanics that it is never possible, in the description of the state of a mechanical system, to attach definite values to both of two canonically conjugate variables, they consequently deem this formalism to be incomplete, and express the belief that a more satisfactory theory can be developed.

Such an argumentation, however, would hardly seem suited to affect the soundness of quantum-mechanical description, which is based on a coherent mathematical formalism covering automatically any procedure of measurement like that indicated.* The apparent contradiction in

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[1] A. Einstein, B. Podolsky and N. Rosen, Phys. Rev. **47**, 777 (1935).

[2] Cf. N. Bohr, *Atomic Theory and Description of Nature*, I (Cambridge, 1934).

* The deductions contained in the article cited may in this respect be considered as an immediate consequence of the transformation theorems of quantum mechanics, which perhaps more than any other feature of the formalism contribute to secure its mathematical completeness and its rational correspondence with classical mechanics. In fact, it is always possible in the description of a mechanical system, consisting of two partial systems (1) and (2), interacting or not, to replace any two pairs of canonically conjugate variables (q_1p_1), (q_2p_2) pertaining to systems (1) and (2), respectively, and satisfying the usual commutation rules

$$[q_1p_1]=[q_2p_2]=ih/2\pi,$$
$$[q_1q_2]=[p_1p_2]=[q_1p_2]=[q_2p_1]=0,$$

by two pairs of new conjugate variables (Q_1P_1), (Q_2P_2) related to the first variables by a simple orthogonal transformation, corresponding to a rotation of angle θ in the planes (q_1q_2), (p_1p_2)

$$q_1=Q_1\cos\theta-Q_2\sin\theta \qquad p_1=P_1\cos\theta-P_2\sin\theta$$
$$q_2=Q_1\sin\theta+Q_2\cos\theta \qquad p_2=P_1\sin\theta+P_2\cos\theta.$$

Since these variables will satisfy analogous commutation rules, in particular

$$[Q_1P_1]=ih/2\pi, \qquad [Q_1P_2]=0,$$

it follows that in the description of the state of the combined system definite numerical values may not be assigned to both Q_1 and P_1, but that we may clearly assign fact discloses only an essential inadequacy of the customary viewpoint of natural philosophy for a rational account of physical phenomena of the type with which we are concerned in quantum mechanics. Indeed the *finite interaction between object and measuring agencies* conditioned by the very existence of the quantum of action entails—because of the impossibility of controlling the reaction of the object on the measuring instruments if these are to serve their purpose—the necessity of a final renunciation of the classical ideal of causality and a radical revision of our attitude towards the problem of physical reality. In fact, as we shall see, a criterion of reality like that proposed by the named authors contains—however cautious its formulation may appear—an essential ambiguity when it is applied to the actual problems with which we are here concerned. In order to make the argument to this end as clear as possible, I shall first consider in some detail a few simple examples of measuring arrangements.

Let us begin with the simple case of a particle passing through a slit in a diaphragm, which may form part of some more or less complicated experimental arrangement. Even if the momentum of this particle is completely known before it impinges on the diaphragm, the diffraction by the slit of the plane wave giving the symbolic representation of its state will imply an uncertainty in the momentum of the particle, after it has passed the diaphragm, which is the greater the narrower the slit. Now the width of the slit, at any rate if it is still large compared with the wave-length, may be taken as the uncertainty Δq of the position of the particle relative to the diaphragm, in a direction perpendicular to the slit. Moreover, it is simply seen from de Broglie's relation between momentum and wave-length that the uncertainty Δp of the momentum of the particle in this direction is correlated to Δq by means of Heisenberg's general principle

$$\Delta p \Delta q \sim h,$$

which in the quantum-mechanical formalism is a direct consequence of the commutation relation for any pair of conjugate variables. Obviously the uncertainty Δp is inseparably connected with the possibility of an exchange of momentum between the particle and the diaphragm; and the question of principal interest for our discussion is now to what extent the momentum thus exchanged can be taken into account in the description of the phenomenon to be studied by the experimental arrangement concerned, of which the passing of the particle through the slit may be considered as the initial stage.

Let us first assume that, corresponding to usual experiments on the remarkable phenomena of electron diffraction, the diaphragm, like the other parts of the apparatus,—say a second diaphragm with several slits parallel to the first and a photographic plate,—is rigidly fixed to a support which defines the space frame of reference. Then the momentum exchanged between the particle and the diaphragm will, together with the reaction of the particle on the other bodies, pass into this common support, and we have thus voluntarily cut ourselves off from any possibility of taking these reactions separately into account in predictions regarding the final result of the experiment,—say the position of the spot produced by the particle on the photographic plate. The impossibility of a closer analysis of the reactions between the particle and the measuring instrument is indeed no peculiarity of the experimental procedure described, but is rather an essential property of any arrangement suited to the study of the phenomena of the type concerned, where we have to do with a feature of *individuality* completely foreign to classical physics. In fact, any possibility of taking into account the momentum exchanged between the particle and the separate parts of the apparatus would at once permit us to draw conclusions regarding the "course" of such phenomena,—say through what particular slit of the second diaphragm the particle passes on its way to the photographic plate—which would be quite incompatible with the fact that the probability of the particle reaching a given element of area on this plate is determined not by the presence of any particular slit, but by the positions of all the slits of the second diaphragm within reach

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such values to both Q_1 and P_2. In that case it further results from the expressions of these variables in terms of (q_1p_1) and (q_2p_2), namely

$$Q_1 = q_1 \cos\theta + q_2 \sin\theta, \qquad P_2 = -p_1 \sin\theta + p_2 \cos\theta,$$

that a subsequent measurement of either q_2 or p_2 will allow us to predict the value of q_1 or p_1 respectively.

of the associated wave diffracted from the slit of the first diaphragm.

By another experimental arrangement, where the first diaphragm is not rigidly connected with the other parts of the apparatus, it would at least in principle* be possible to measure its momentum with any desired accuracy before and after the passage of the particle, and thus to predict the momentum of the latter after it has passed through the slit. In fact, such measurements of momentum require only an unambiguous application of the classical law of conservation of momentum, applied for instance to a collision process between the diaphragm and some test body, the momentum of which is suitably controlled before and after the collision. It is true that such a control will essentially depend on an examination of the space-time course of some process to which the ideas of classical mechanics can be applied; if, however, all spatial dimensions and time intervals are taken sufficiently large, this involves clearly no limitation as regards the accurate control of the momentum of the test bodies, but only a renunciation as regards the accuracy of the control of their space-time coordination. This last circumstance is in fact quite analogous to the renunciation of the control of the momentum of the fixed diaphragm in the experimental arrangement discussed above, and depends in the last resort on the claim of a purely classical account of the measuring apparatus, which implies the necessity of allowing a latitude corresponding to the quantum-mechanical uncertainty relations in our description of their behavior.

The principal difference between the two experimental arrangements under consideration is, however, that in the arrangement suited for the control of the momentum of the first diaphragm, this body can no longer be used as a measuring instrument for the same purpose as in the previous case, but must, as regards its position relative to the rest of the apparatus, be treated, like the particle traversing the slit, as an object of investigation, in the sense that the quantum-mechanical uncertainty relations regarding its position and momentum must be taken explicitly into account. In fact, even if we knew the position of the diaphragm relative to the space frame before the first measurement of its momentum, and even though its position after the last measurement can be accurately fixed, we lose, on account of the uncontrollable displacement of the diaphragm during each collision process with the test bodies, the knowledge of its position when the particle passed through the slit. The whole arrangement is therefore obviously unsuited to study the same kind of phenomena as in the previous case. In particular it may be shown that, if the momentum of the diaphragm is measured with an accuracy sufficient for allowing definite conclusions regarding the passage of the particle through some selected slit of the second diaphragm, then even the minimum uncertainty of the position of the first diaphragm compatible with such a knowledge will imply the total wiping out of any interference effect—regarding the zones of permitted impact of the particle on the photographic plate—to which the presence of more than one slit in the second diaphragm would give rise in case the positions of all apparatus are fixed relative to each other.

In an arrangement suited for measurements of the momentum of the first diaphragm, it is further clear that even if we have measured this momentum before the passage of the particle through the slit, we are after this passage still left with a *free choice* whether we wish to know the momentum of the particle or its initial position relative to the rest of the apparatus. In the first eventuality we need only to make a second determination of the momentum of the diaphragm, leaving unknown forever its exact position when the particle passed. In the second eventuality we need only to determine its position relative to the space frame with the inevitable loss of the knowledge of the momentum exchanged between the diaphragm and the particle. If the diaphragm is sufficiently massive in comparison with the particle, we may even arrange the procedure of measurements in such a way that the diaphragm after the first determination of its momentum will remain at rest in some unknown position relative to the

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* The obvious impossibility of actually carrying out, with the experimental technique at our disposal, such measuring procedures as are discussed here and in the following does clearly not affect the theoretical argument, since the procedures in question are essentially equivalent with atomic processes, like the Compton effect, where a corresponding application of the conservation theorem of momentum is well established.

other parts of the apparatus, and the subsequent fixation of this position may therefore simply consist in establishing a rigid connection between the diaphragm and the common support.

My main purpose in repeating these simple, and in substance well-known considerations, is to emphasize that in the phenomena concerned we are not dealing with an incomplete description characterized by the arbitrary picking out of different elements of physical reality at the cost of sacrifying other such elements, but with a rational discrimination between essentially different experimental arrangements and procedures which are suited either for an unambiguous use of the idea of space location, or for a legitimate application of the conservation theorem of momentum. Any remaining appearance of arbitrariness concerns merely our freedom of handling the measuring instruments, characteristic of the very idea of experiment. In fact, the renunciation in each experimental arrangement of the one or the other of two aspects of the description of physical phenomena,—the combination of which characterizes the method of classical physics, and which therefore in this sense may be considered as *complementary* to one another,—depends essentially on the impossibility, in the field of quantum theory, of accurately controlling the reaction of the object on the measuring instruments, i.e., the transfer of momentum in case of position measurements, and the displacement in case of momentum measurements. Just in this last respect any comparison between quantum mechanics and ordinary statistical mechanics,—however useful it may be for the formal presentation of the theory,—is essentially irrelevant. Indeed we have in each experimental arrangement suited for the study of proper quantum phenomena not merely to do with an ignorance of the value of certain physical quantities, but with the impossibility of defining these quantities in an unambiguous way.

The last remarks apply equally well to the special problem treated by Einstein, Podolsky and Rosen, which has been referred to above, and which does not actually involve any greater intricacies than the simple examples discussed above. The particular quantum-mechanical state of two free particles, for which they give an explicit mathematical expression, may be reproduced, at least in principle, by a simple experimental arrangement, comprising a rigid diaphragm with two parallel slits, which are very narrow compared with their separation, and through each of which one particle with given initial momentum passes independently of the other. If the momentum of this diaphragm is measured accurately before as well as after the passing of the particles, we shall in fact know the sum of the components perpendicular to the slits of the momenta of the two escaping particles, as well as the difference of their initial positional coordinates in the same direction; while of course the conjugate quantities, i.e., the difference of the components of their momenta, and the sum of their positional coordinates, are entirely unknown.* In this arrangement, it is therefore clear that a subsequent single measurement either of the position or of the momentum of one of the particles will automatically determine the position or momentum, respectively, of the other particle with any desired accuracy; at least if the wave-length corresponding to the free motion of each particle is sufficiently short compared with the width of the slits. As pointed out by the named authors, we are therefore faced at this stage with a completely free choice whether we want to determine the one or the other of the latter quantities by a process which does not directly interfere with the particle concerned.

Like the above simple case of the choice between the experimental procedures suited for the prediction of the position or the momentum of a single particle which has passed through a slit in a diaphragm, we are, in the "freedom of choice" offered by the last arrangement, just concerned with a *discrimination between different experimental procedures which allow of the unambiguous use of complementary classical concepts.* In fact to measure the position of one of the particles can mean nothing else than to establish a correlation between its behavior and some

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* As will be seen, this description, apart from a trivial normalizing factor, corresponds exactly to the transformation of variables described in the preceding footnote if (q_1p_1), (q_2p_2) represent the positional coordinates and components of momenta of the two particles and if $\theta=-\pi/4$. It may also be remarked that the wave function given by formula (9) of the article cited corresponds to the special choice of $P_2=0$ and the limiting case of two infinitely narrow slits.

instrument rigidly fixed to the support which defines the space frame of reference. Under the experimental conditions described such a measurement will therefore also provide us with the knowledge of the location, otherwise completely unknown, of the diaphragm with respect to this space frame when the particles passed through the slits. Indeed, only in this way we obtain a basis for conclusions about the initial position of the other particle relative to the rest of the apparatus. By allowing an essentially uncontrollable momentum to pass from the first particle into the mentioned support, however, we have by this procedure cut ourselves off from any future possibility of applying the law of conservation of momentum to the system consisting of the diaphragm and the two particles and therefore have lost our only basis for an unambiguous application of the idea of momentum in predictions regarding the behavior of the second particle. Conversely, if we choose to measure the momentum of one of the particles, we lose through the uncontrollable displacement inevitable in such a measurement any possibility of deducing from the behavior of this particle the position of the diaphragm relative to the rest of the apparatus, and have thus no basis whatever for predictions regarding the location of the other particle.

From our point of view we now see that the wording of the above-mentioned criterion of physical reality proposed by Einstein, Podolsky and Rosen contains an ambiguity as regards the meaning of the expression "without in any way disturbing a system." Of course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure. But even at this stage there is essentially the question of *an influence on the very conditions which define the possible types of predictions regarding the future behavior of the system.* Since these conditions constitute an inherent element of the description of any phenomenon to which the term "physical reality" can be properly attached, we see that the argumentation of the mentioned authors does not justify their conclusion that quantum-mechanical description is essentially incomplete. On the contrary this description, as appears from the preceding discussion, may be characterized as a rational utilization of all possibilities of unambiguous interpretation of measurements, compatible with the finite and uncontrollable interaction between the objects and the measuring instruments in the field of quantum theory. In fact, it is only the mutual exclusion of any two experimental procedures, permitting the unambiguous definition of complementary physical quantities, which provides room for new physical laws, the coexistence of which might at first sight appear irreconcilable with the basic principles of science. It is just this entirely new situation as regards the description of physical phenomena, that the notion of *complementarity* aims at characterizing.

The experimental arrangements hitherto discussed present a special simplicity on account of the secondary role which the idea of time plays in the description of the phenomena in question. It is true that we have freely made use of such words as "before" and "after" implying time-relationships; but in each case allowance must be made for a certain inaccuracy, which is of no importance, however, so long as the time intervals concerned are sufficiently large compared with the proper periods entering in the closer analysis of the phenomenon under investigation. As soon as we attempt a more accurate time description of quantum phenomena, we meet with well-known new paradoxes, for the elucidation of which further features of the interaction between the objects and the measuring instruments must be taken into account. In fact, in such phenomena we have no longer to do with experimental arrangements consisting of apparatus essentially at rest relative to one another, but with arrangements containing moving parts,—like shutters before the slits of the diaphragms,—controlled by mechanisms serving as clocks. Besides the transfer of momentum, discussed above, between the object and the bodies defining the space frame, we shall therefore, in such arrangements, have to consider an eventual exchange of energy between the object and these clock-like mechanisms.

The decisive point as regards time measurements in quantum theory is now completely analogous to the argument concerning measurements of positions outlined above. Just as the transfer of momentum to the separate parts of the apparatus,—the knowledge of the relative positions of which is required for the description of the phenomenon,—has been seen to be entirely uncontrollable, so the exchange of energy between the object and the various bodies, whose relative motion must be known for the intended use of the apparatus, will defy any closer analysis. Indeed, it is *excluded in principle to control the energy which goes into the clocks without interfering essentially with their use as time indicators.* This use in fact entirely relies on the assumed possibility of accounting for the functioning of each clock as well as for its eventual comparison with other clocks on the basis of the methods of classical physics. In this account we must therefore obviously allow for a latitude in the energy balance, corresponding to the quantum-mechanical uncertainty relation for the conjugate time and energy variables. Just as in the question discussed above of the mutually exclusive character of any unambiguous use in quantum theory of the concepts of position and momentum, it is in the last resort this circumstance which entails the complementary relationship between any detailed time account of atomic phenomena on the one hand and the unclassical features of intrinsic stability of atoms, disclosed by the study of energy transfers in atomic reactions on the other hand.

This necessity of discriminating in each experimental arrangement between those parts of the physical system considered which are to be treated as measuring instruments and those which constitute the objects under investigation may indeed be said to form a *principal distinction between classical and quantum-mechanical description of physical phenomena.* It is true that the place within each measuring procedure where this discrimination is made is in both cases largely a matter of convenience. While, however, in classical physics the distinction between object and measuring agencies does not entail any difference in the character of the description of the phenomena concerned, its fundamental importance in quantum theory, as we have seen, has its root in the indispensable use of classical concepts in the interpretation of all proper measurements, even though the classical theories do not suffice in accounting for the new types of regularities with which we are concerned in atomic physics. In accordance with this situation there can be no question of any unambiguous interpretation of the symbols of quantum mechanics other than that embodied in the well-known rules which allow to predict the results to be obtained by a given experimental arrangement described in a totally classical way, and which have found their general expression through the transformation theorems, already referred to. By securing its proper correspondence with the classical theory, these theorems exclude in particular any imaginable inconsistency in the quantum-mechanical description, connected with a change of the place where the discrimination is made between object and measuring agencies. In fact it is an obvious consequence of the above argumentation that in each experimental arrangement and measuring procedure we have only a free choice of this place within a region where the quantum-mechanical description of the process concerned is effectively equivalent with the classical description.

Before concluding I should still like to emphasize the bearing of the great lesson derived from general relativity theory upon the question of physical reality in the field of quantum theory. In fact, notwithstanding all characteristic differences, the situations we are concerned with in these generalizations of classical theory present striking analogies which have often been noted. Especially, the singular position of measuring instruments in the account of quantum phenomena, just discussed, appears closely analogous to the well-known necessity in relativity theory of upholding an ordinary description of all measuring processes, including a sharp distinction between space and time coordinates, although the very essence of this theory is the establishment of new physical laws, in the comprehension of which we must renounce the customary separation of space and time ideas.*

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* Just this circumstance, together with the relativistic invariance of the uncertainty relations of quantum mechanics, ensures the compatibility between the argumentation outlined in the present article and all exigencies of relativity theory. This question will be treated in greater detail in a paper under preparation, where the writer will in particular discuss a very interesting paradox suggested by Einstein concerning the application of gravitation theory to energy measurements, and the solution of which offers an especially instructive illustration of the generality of the argument of complementarity. On the same occasion a more thorough discussion of space-time measurements in quantum theory will be given with all necessary mathematical developments and diagrams of experimental

The dependence on the reference system, in relativity theory, of all readings of scales and clocks may even be compared with the essentially uncontrollable exchange of momentum or energy between the objects of measurements and all instruments defining the space-time system of reference, which in quantum theory confronts us with the situation characterized by the notion of complementarity. In fact this new feature of natural philosophy means a radical revision of our attitude as regards physical reality, which may be paralleled with the fundamental modification of all ideas regarding the absolute character of physical phenomena, brought about by the general theory of relativity.

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arrangements, which had to be left out of this article, where the main stress is laid on the dialectic aspect of the question at issue.

5 Further Developments

Einstein, Podolsky, and Rosen concluded that quantum mechanics has to be incomplete. This obviously leads to the question of how to complete it. It especially leads to the question about ‘hidden variables’ that would, for example, allow to simultaneously determine position and momentum of a particle.

John von Neumann had already raised the question of hidden variables in his famous textbook [159, p. 109], three years before the EPR paper, but without being mentioned by EPR. In chapter V of his book, von Neumann presented a formal mathematical proof of the impossibility of such hidden variables. It is unknown whether EPR knew of this proof and whether they had refrained from writing their paper, had they known of the proof.[1]

Later, several physicists would find an essential gap in von Neumann’s proof, the contribution by John Bell being the most consequential one (see Sect. 5.2). However, already in 1935 Grete Hermann, whom we mentioned in connection with Heisenberg’s response to the EPR paper, had pointed out the existence of this gap: von Neumann assumed the linearity of the expectation values. This assumption is valid in quantum mechanics, but in a theory containing hidden variables this linearity is not necessarily given and in general the assumption will not be valid. For this reason, Hermann is not surprised at all that von Neumann achieves to prove the impossibility of such variables, she even spoke of a circular argument (see Hermann [90, pp. 99–102]). Towards the end of the corresponding section, Hermann writes:

> But with this consideration, the decisive *physical* question, whether a progressing physical research may achieve calculating more precise predictions than is possible today, cannot be transformed into the *mathematical* question – being not at all equivalent to the physical

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[1] Anyhow, in 1938 Einstein seems to have known about von Neumann’s proof, cf. Maudlin [117, p. 20].

C. Kiefer (ed.), *Albert Einstein, Boris Podolsky, Nathan Rosen*, Classic Texts
in the Sciences, https://doi.org/10.1007/978-3-030-47037-1_5

> question – whether such a development could be realised by exclusively using the quantum mechanical operator calculus.[2]

Grete Hermann was a philosopher and as such saw herself in the tradition of Immanuel Kant. She could not accept that it should be impossible in quantum mechanics to give the cause for a single radioactive decay beyond the purely statistical interpretation. This is why she was interested in von Neumann's proof and was quite gratified to find that gap in it. She discussed this issue a lot with Heisenberg and Carl Friedrich von Weizsäcker in Leipzig. In his autobiography *Der Teil und das Ganze,* Heisenberg gave an eloquent account of these discussions [88, pp. 163–173].

A theory containing hidden variables that circumvents von Neumann's proof is Bohm's theory. This theory is what the next section is about.

5.1 Bohm's Theory

In his textbook on quantum mechanics, Bohm had presented a simplified version of the EPR experiment that operates with two particles of spin 1/2 [21]. He, however, rejected EPR's conclusion of quantum mechanics' incompleteness. For, in his opinion, the assumption of a local reality contradicts quantum theory.[3]

Nevertheless, Bohm was unsatisfied with the then common (Copenhagen) point of view on quantum theory. In an interview he once said (cf. Pauli [130, p. 341]):

> I wrote my book Quantum Theory in an attempt to understand quantum theory from Bohr's point of view. After I'd written it I wasn't satisfied that I really understood it, and I began to look again.

'Looking again' led Bohm to develop a new interpretation [22, 23].[4] Strictly speaking, he not only developed a new interpretation, but a new theory. Whereas this theory leaves the wave function untouched, it introduces new, 'hidden' variables. These variables act non-locally and are therefore consistent with Bohm's assumption of a non-local reality.

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[2] "Aber mit dieser Überlegung kann die entscheidende *physikalische* Frage, ob die fortschreitende physikalische Forschung zu genaueren Vorausberechnungen gelangen kann, als sie heute möglich ist, nicht in die keineswegs gleichwertige *mathematische* Frage umgebogen werden, ob eine solche Entwicklung allein mit den Mitteln des quantenmechanischen Operatorenkalküls darstellbar wäre." (English translation by S. Linden and A. K. Hudert)

[3] For details on Bohm's scientific background see, e.g., Pauli [130, pp. 340–343].

[4] Maybe the discussions Bohm had with Einstein on this topic were the decisive trigger for Bohm's work, cf. Maudlin [117, p. 21].

In the introduction to the first paper, Bohm emphasises [22, p. 166]:

> Most physicists have felt that objections such as those raised by Einstein are not relevant, first, because the present form of the quantum theory with its usual probability interpretation is in excellent agreement with an extremely wide range of experiments, at least in the domain of distances larger than 10^{-13} cm, and secondly, because no consistent alternative interpretations have as yet been suggested. The purpose of this paper [...] is, however, to suggest just such an alternative interpretation.

The mentioned scale of 10^{-13} cm is the scale of nuclear physics. At the time, the common assumption was that smaller scales presented violations of quantum mechanics. Bohm himself assumed his theory to be equivalent to quantum mechanics only on scales larger than 10^{-13} cm, and to deviate from it on smaller scales.

If one discards fields and decides to only describe particles, Bohm's additional variables are the *positions* of the particles, e.g., of electrons.[5] The dynamics of these positions is determined by the autonomous wave function Ψ that is governed by Schrödinger's equation. In opposition to Newtonian mechanics, the velocities of these positions cannot be chosen freely but are determined by Ψ. The quantum mechanical probabilities, calculated from Ψ as usual, then become probabilities in the sense of classical statistics, i.e., they only express our ignorance of these particle positions.

Bohm presented his new interpretation in two papers [22,23], the second one containing a detailed analysis of the measurement process.[6] Bohm declares von Neumann's proof of the impossibility of hidden variables irrelevant for his theory. According to Bohm, the reason for this is that the hidden variables depend on the system as well as on the measurement apparatus; this is called a *contextual situation*, and such variables are called *contextual*, as opposed to a non-contextual situation where the variables depend only on the system, independently of the degrees of freedom interacting with it. Bell would later judge Bohm's critique of von Neumann's proof vague and imprecise, offering a trenchant critique of his own [12].

Bohm's ideas were not really new. In the 1920s, Louis de Broglie had proposed a theory of pilot waves that showed many similarities with Bohm's later theory and seemed appealing to Einstein at first (cf. Einstein [56]), see Sect. 1.3. De Broglie had dropped his idea following the harsh criticism by Pauli during the 1927 Solvay Conference, but came back to it after Bohm's papers were published. In his contribution to the Born Festschrift, he explains the return to his theory and also establishes an interesting connection to Einstein's ideas of particles being singularities of fields [44]. The same Festschrift contains contributions by Bohm and Einstein on this topic referencing each other [24,61]. In a letter to Born, Einstein commented on his contribution:

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[5] Because in experiments one rather observes the particles's positions than their wave functions, Bell proposed to call those variables *exposed* rather than hidden [15, p. 128].

[6] An extensive study of Bohm's theory is given by Dürr [50].

> For the presentation volume to be dedicated to you, I have written a little nursery song about physics, which has startled Bohm and de Broglie a little.[7]

At that time, Einstein was not partial to de Broglie's pilot wave theory anymore and certainly not to Bohm's variation of it. This should not come as a surprise, as both theories explicitly contradict the locality postulated by Einstein.

Bohm himself found that the greatest progress his idea brought, when compared to de Broglie, was the following [22, p. 167]:

> The essential new step in doing this is to apply our interpretation in the theory of the measurement process itself as well as in the description of the observed system.

His elaborations on this can be found in appendix B to his second paper [23]. It also includes comments on Rosen's interpretation [136], which exhibits similar traits.

The formal starting point of Bohm's (and de Broglie's) theory is this wave function ansatz:

$$\Psi = R \exp\left(\frac{\mathrm{i}S}{\hbar}\right). \tag{5.1}$$

Using this ansatz, Schrödinger's equation can be decomposed into an equation for the amplitude R and an equation for the phase S. The phase equation is similar to the Hamilton-Jacobi equation of classical mechanics but contains an additional term that Bohm called the *quantum potential* and that is determined by the wave function (5.1).[8] Its presence leads to the particles being 'guided' by the wave function in a non-local way on trajectories that cannot be understood intuitively. Two examples: As a particle's velocity is determined by the phase of the wave function, a ground state electron in a hydrogen atom is at rest, since its wave function is real. In a double slit experiment, the particle does not go through the slit it is 'heading to', but through the other one.

The exact expression of the quantum potential associated with (5.1) reads:

$$Q := -\frac{\hbar^2}{2m}\frac{\nabla^2 R}{R}, \tag{5.2}$$

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[7] "Ich habe für den Dir zugedachten Festband ein physikalisches Kinderliedchen geschrieben, das Bohm und de Broglie ein bißchen aufgescheucht hat." (Einstein et al. [69, p. 266], English translation taken from Born, M. (1971), *The Born-Einstein Letters,* Macmillan.)

[8] The quantum potential was already contained in the 'hydrodynamical interpretation' by Madelung [114]; he called it *Quantenglied*. But this name is somewhat misleading, since the equations of motion for Bohmian trajectories are of first order and there are therefore no forces associated with the quantum potential.

where m is the mass of the particle. Evidently, Q is invariant under rescaling the amplitude, $R \mapsto \lambda R$, which once again stresses that Ψ cannot be a classical field.

Bohm discusses the EPR situation in chapter 8 of his second paper, working with the original wave function by EPR and not with the simplified version he used in his textbook (which, of course, has to do with the fact that initially it was not clear how to apply Bohm's theory to spin states). Because the EPR wave function (2.1) is real, the particles are at rest. Their possible positions are described by an ensemble satisfying $x_1 - x_2 = a$. Bohm then proceeds to describing the situation in a way reminiscent of Bohr:

> Now, if we measure the position of the first particle, we introduce uncontrollable fluctuations in the wave function for the entire system, which, through the 'quantum-mechanical' forces, bring about corresponding uncontrollable fluctuations in the momentum of each particle. Similarly, if we measure the momentum of the first particle, uncontrollable fluctuations in the wave function for the system bring about, through the 'quantum-mechanical' forces, corresponding uncontrollable changes in the position of each particle. Thus, the 'quantum-mechanical' forces may be said to transmit uncontrollable disturbances instantaneously from one particle to another through the medium of the Ψ-field.

Because Bohm accepts the non-locality as a fundamental aspect of the theory, he avoids the EPR criterion from the very beginning.

After the measurement is completed, the measured particle remains trapped in a wave packet; the other wave packets are empty and it is implicitly assumed that these empty packets do not interfere. Within usual quantum mechanics, this assumption can be justified by the process of decoherence, see Sect. 5.4. From this point of view, trajectories are superfluous and "entirely based on a classical prejudice" [174]. There is, in fact, no experiment that *requires* Bohmian trajectories for its explanation.

Bohm's proposition faced heavy pushback. Sharp-tongued Pauli criticised it in letters and published writing, the latter ironically in a contribution to a de Broglie Festschrift [125]. Schrödinger was critical of it, too. In a letter to Einstein he wrote:

> The fact that Bohm proposes to use the same function for probability distribution and force potential is unacceptable to me. Every trajectory occurring in reality may well be thought of as a member of different ensembles of trajectories. But the trajectories one adds in thought cannot act on the dynamics.[9]

Bohm himself had pointed out the asymmetries in his theory. Electrons are described with the help of particle trajectories, but not photons, even though they seem to also exhibit particle properties when blackening a photographic plate. In electromagnetism,

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[9] "Am Bohmschen Vorschlag ist mir unannehmbar, daß er dieselbe Funktion als Wahrscheinlichkeitsverteilung und als Kräftepotential benutzt. Nun kann aber jede wirklich auftretende Bahn doch wohl als Mitglied verschiedener Bahngesamtheiten gedacht werden. Die hinzugedachten, aber nicht verwirklichten Bahnen können doch nicht auf das Bewegliche einwirken. " (von Meyenn [158, vol. 2, p. 675], English translation by S. Linden and A. K. Hudert)

the quantity corresponding to a classical field that is ‘guided’ by the quantum field wave potential is not the photon but the electromagnetic four-potential. Favouring particle positions over their momenta also breaks the symmetry between the corresponding quantum mechanical representations. Additional problems concern the spin formulations and interactions in relativistic quantum field theories.[10]

Bohm and most of those after him assumed that the initial probability distribution is given by Born’s rule, i.e., by $|\Psi|^2$. More recently, attempts have been made to get by without this assumption [132,157]. Born’s probability distribution then becomes a process of relaxation, so to speak, that reduces a (mostly arbitrary) initial distribution to $|\psi|^2$. Needless to say, certain assumptions have to be made, similar to Boltzmann’s ansatz for the collision frequency when deriving the second law of thermodynamics. A possible cosmological origin of these assumptions is open to speculation.

5.2 The Bell Inequalities

We looked at how Einstein based his arguments on the assumption of a local reality. From it, he deduced the incompleteness of quantum theory. It was Irish physicist John Stewart Bell (1928–1990) who, from this assumption, derived very general inequalities that are violated by quantum theory. Thus, it can be *experimentally* tested whether quantum theory is right or the assumption of local reality. And the experiments were unambiguously in favour of quantum theory.[11]

Bell’s paper directly refers to EPR; its title being “On the Einstein-Podolsky-Rosen paradox” [11]. The first paragraph explains how the two papers are connected:

> The paradox of Einstein, Podolsky, and Rosen was advanced as an argument that quantum mechanics could not be a complete theory but should be supplemented by additional variables. These additional variables were to restore to the theory causality and locality. In this note that idea will be formulated mathematically and shown to be incompatible with the statistical predictions of quantum mechanics.

Note that Bell calls the EPR situation a paradox, probably referring to the conflict between the notion of a local reality and (non-local) quantum theory. The significance of Bell’s work lies precisely in the fact that he brought this conflict to a level of concrete, experimentally verifiable (in)equalities, surrendering it to a definitive decision.

If you are convinced of the universal validity of quantum theory, you will not be surprised to hear of the conflict between Bell’s inequalities and quantum theory. But if

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[10] Recently, aspects of Bohm’s theory have also been used as analogies in classical physics, see, e.g., Harris et al. [85].

[11] Bell wrote many highly readable essays on the foundations of quantum mechanics, collected in Bell [15]. The essay collection by Bertlmann and Zeilinger [19] is also worth reading.

you stand by the classical assumption based on locality, the conflict is astonishing and unsettling [181]. This explains the great interest in Bell's work, which according to Alain Aspect is "one of the most remarkable papers in the history of physics" [2]. Whatever your point of view, there is no question that this paper has stimulated the debate on the foundations of quantum mechanics like no other in the past fifty years.

As stated above, Bell had already been working on the foundations of quantum theory since 1952, mostly because he was impressed by Bohm's publications.[12] Bohm's theory explicitly contained 'hidden variables' that interact in a non-local way. Their existence seemed to contradict the above mentioned proof by von Neumann that had stated the impossibility of such variables as long as one holds on to quantum theory's predictions. Bell detected the gap in von Neumann's proof and authored a paper on it in 1964 (even before his work on the inequalities), but only published it two years later [12]. He apparently did so without knowledge of Grete Hermann's above mentioned work in 1935.

Bell began his paper[13] with the question whether quantum mechanical states could be represented as averages over a new kind of individual states, for which, e.g., the spin values would be determined with respect to any direction, or for which the position and the momentum of a particle would be determined simultaneously. Such states are called 'dispersion free' because in contrast to quantum mechanical states they show no dispersion. For these individual states one needs new 'hidden' variables in addition to the wave function. These variables shall be denoted by λ. They are introduced to allow the prediction of individual measurement results, where quantum mechanics only allows statistical assertions.

Bell then proceeded to a detailed discussion of von Neumann's proof and identified – as had done Grete Hermann before him – its sore spot: an overly strong assumption. Von Neumann had postulated the linearity of the expectation values: The expectation value of a sum of operators is the sum of their expectation values.[14] Whereas this rule applies in quantum mechanics, it does not necessarily apply to hidden variables. Thus, von Neumann's assumption is too strong.[15]

Bell then turned to other proofs proclaiming the impossibility of theories with hidden variables, especially to the work of Gleason [80] and Kochen and Specker [108].[16] These works show that there are no *non-contextual* models with hidden parameters that are

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[12] "The papers were for me a revelation", his wife Mary quoted him [19, p. 3].

[13] Bell included a detailed discussion of the same topic in his report of a conference held in Varenna in 1970, cf. Bell [15, pp. 29–39].

[14] See the appendix for the definition of expectation values.

[15] Bub [35] highlights that von Neumann's proof still rules out a certain class of theories with hidden variables.

[16] In the above mentioned Varenna report, Bell went into detail on Kochen and Specker [108] and subsequent works.

compatible with the predictions of quantum mechanics.[17] Though Bell did not use the term 'non-contextual',[18] his ideas develop exactly in the sense of the term. Non-contextual means that the full state, given by ψ and λ, can, for example, assign a well-defined spin component to every direction, no matter which other components or quantities are being measured. The exclusion of non-contextual models by the above mentioned proofs means: One cannot assume that the results of a quantum mechanical measurement exist before the measurement. An experimental validation of this impossibility is discussed in D'Ambrosio et al. [43].

These results in themselves are interesting enough, as they challenge the view schooled by classical physics. But Bell's most significant insight was that all these proofs are based on overly strong assumptions. The hidden variables may not only be associated with the measured system but also with the measurement apparatus, corresponding to a *contextual situation*. Generally, this means a situation in which a measurement result may depend on what other measurements are taken. Bell [12, p. 451]:

> The result of an observation may reasonably depend not only on the state of the system (including hidden variables) but also on the complete disposition of the apparatus.

Bell discussed contextual situations. Assuming a *local* theory with hidden variables, he derived very general inequalities that are violated by quantum mechanics [11]. If these inequalities are violated, Einstein's assumption of locality is wrong.

Fur the purpose of experimental testing, one usually works with a generalised version of Bell's original equations. This generalised version was first given by Clauser et al. [41]. Hence their name, CHSH inequalities or CHSH test, which stands for the initials of the authors. To derive these inequalities would go beyond the scope of this book,[19] but the main ideas shall be described.

Let us consider the experimental setup depicted in Fig. 5.1. The source in the centre sends out two particles with half-integral spin in opposite directions. Let us also assume that both particles are in the non-local state (2.9), this being the singlet state occurring in Bohm's version of EPR's thought experiment. To the right and to the left of the source, at equal distance to the source, there are two 'polarisers', P_1 and P_2, that let the particle through only if its spin points upwards in relation to a given direction; that way, the spin component with respect to that direction is measured. Let $\mathbf{a}$ and $\mathbf{a}'$ denote the two possible directions at P_1 and let $\mathbf{b}$ and $\mathbf{b}'$ denote the two possible directions at P_2. There is a detector behind each polariser responding to incoming particles.

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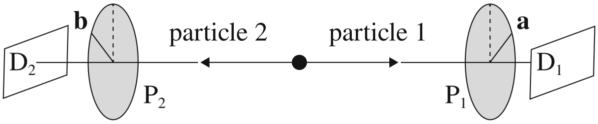
[17] Formally, these proofs only apply to Hilbert spaces with dimensions equal to or higher than three.

[18] An extensive discussion of the notions of 'contextuality' and 'non-contextuality' can be found in Peres [131] and Shimony [150].

[19] In addition to Bell [15] and Bertlmann and Zeilinger [19], Isham [95] and Peres [131] are recommendable.

Fig. 5.1 Experimental setup to test the Bell inequalities



When choosing directions **a** = **b**, the state (2.9) exhibits a perfect anticorrelation – if the spin points upwards at P_1, it points downwards at P_2, and vice versa. However, the Bell inequalities require at least two directions at each polariser. This corresponds to the contextuality of the situation. Now the assumption of locality states, that the measurement result at P_1 is independent of the chosen direction at P_2. In the experiments, this is assured by choosing a random direction at P_2 so quickly that no signal from P_1, traveling with less than or with light speed, can reach P_2 before the direction at P_2 is randomly chosen. (That is, the spacetime interval between the events 'measurement of the spin at P_1' and 'choice of direction at P_2' is spacelike.)

Let the correlation of the measurement results at P_1 and P_2 be described by a function $C(\mathbf{a}, \mathbf{b})$ that depends on the two chosen directions. (In case of a perfect anticorrelation, the value of this function shall be -1. In case of a perfect correlation, its value shall be $+1$.) From the assumption of locality alone, one can then deduce the following Bell inequality (or CHSH inequality):

$$|C(\mathbf{a}, \mathbf{b}) + C(\mathbf{a}, \mathbf{b}') + C(\mathbf{a}', \mathbf{b}) - C(\mathbf{a}', \mathbf{b}')| \leq 2. \tag{5.3}$$

Quantum mechanics yields the boundary $2\sqrt{2} > 2$ on the right-hand side, and there are indeed quantum mechanical states, that explicitly violate (5.3). Of course, these states are entangled like (2.9).[20]

In the experiments, one usually uses photons whose directions of polarisation take over the role of the spin in the above described examples. The first significant tests were performed by Alain Aspect and his group in Paris in the early 1980s. They found a violation of the CHSH inequalities with a 5σ-confidence. This group achieved the spacelike separation of P_1 and P_2.

So far, all relevant experiments have validated quantum mechanics and violated the Bell inequalities – and with them the assumption of locality. Nonetheless, possible loopholes that would utilise experimental imperfections to save the validity of the Bell inequalities keep being discussed [162]. One possible loophole would be the violation of the spacelike separation of the above mentioned events; but this has been essentially ruled out in all experiments. Another loophole would be biased statistics that could be rooted in the fact that not all of the photons are captured by the detectors; this is called the *detection loophole*. One can of course also question the concept of free will and thus whether it

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[20] Conversely, states that satisfy the Bell inequalities cannot necessarily be factorised cf. Bruß [34, p. 104]. Entangled states that satisfy the Bell inequalities are also referred to as 'Werner states'.

is even possible to randomly choose the direction of polarisation at P_2. However, this idea seems far-fetched to most physicists and shall not be discussed here.

The current experimental situation is mainly aimed at definitively closing these loopholes and has already been widely successful at it.[21] Even though some details are still being discussed – one can, with near certainty, conclude that the Bell inequalities are violated empirically, that the predictions of quantum mechanics hold true, and that the assumption of a local reality is wrong.

The tests of local reality proposed by Bell are essentially tests of inequalities. In addition to that, Greenberger et al. [81] were able to present a state whose test for local reality is a test of an equality. The Greenberger-Horne-Zeilinger state (GHZ state) is a state not of two (as is Bell's state), but of three (or more) entangled photons. While quantum mechanics predicts the value -1 for a certain observable (a specific product of spin components) of a system in this state, local reality predicts the value $+1$. In this case, too, did experimental tests result in the validity of quantum mechanics.[22]

Bell's work and the following development were set in motion by the EPR paper. For Einstein, the assumption of a local reality was pivotal. The above described development showed that this assumption contradicts empirically validated predictions of quantum mechanics. Would Einstein have adjusted his point of view if he had known of these results? There is no point in speculating, but it is hard to imagine that Einstein would have ignored empirical evidence.

Bell pointed out that the question of determinism was secondary to Einstein – his main concern being local reality (also cf. Maudlin [117]). In Bell [13], he wrote:

> It is important to note that to the limited degree to which *determinism* plays a role in the EPR argument, it is not assumed but *inferred*. What is held sacred is the principle of 'local causality' – or 'no action at a distance'. [...] It is remarkably difficult to get this point across, that determinism is not a *presupposition* of the analysis.[23]

Surely Einstein did not believe that God 'plays dice', but he was more willing to let go of determinism than of locality.

In the above mentioned article, Bell also addresses Bohr's reaction to the EPR paper (discussed in Sect. 4.2), also cf. Whitaker [165]. He essentially views Bohr's paper as unintelligible: "While imagining that I understand the position of Einstein, as regards the EPR correlations, I have very little understanding of the position of his principal opponent, Bohr." And after discussing some central aspects of Bohr's paper: "Indeed I have very little idea what this means." He concludes asking: "Is Bohr just rejecting the premise – 'no

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[21] Cf. the recent works by Christensen et al. [40], Giustina et al. [79] and Erven et al. [70].

[22] See Pan et al. [124]. Erven et al. [70], referenced in the preceding footnote, also worked with a GHZ state with three photons, two of which propagated over a distance of several hundred meters.

[23] Reprinted in Bell [15, p. 142].

action at a distance' – rather than refuting the argument?"[24] We have nothing to add to this.

5.3 The Many-Worlds Interpretation

Hugh Everett (1930–1982) published an article in 1957, based on the doctoral thesis he had written under the supervision of John Wheeler [72]. In it, he introduces a new interpretation of quantum theory, which he called the *relative state formulation*. Later, it became known as the 'many-worlds interpretation' or the 'Everett interpretation'.

Everett quoted attempts to quantise general relativity as a motivation, which in those days were of interest to Wheeler. One aspect of the problem is how to interpret a wave function that is applied to the whole universe, and therefore has no exterior observer. The title of his thesis was indeed *Theory of the universal wave function*. In it, however, quantising the theory of relativity plays no role; ten years later, Bryce DeWitt would pick up this thread in the context of Everett [48].

The key to Everett's interpretation is to take the formalism of quantum theory seriously and, in a sense, accept it as definitive. In particular, the Schrödinger equation (1.4) shall *always be exactly valid* for an isolated system. So in this interpretation, there is no collapse of the wave function, which has fundamental consequences for the role of the observer in quantum theory.

Let us consider a simple example of the quantum mechanical measurement process that was described by von Neumann in his book [159]. Consider a quantum mechanical system with half-integral spin. To measure that spin with respect to a freely chosen direction (defined, for example, by a magnetic field in z-direction), a measurement apparatus is connected to the system. According to the rules of quantum theory, the spin value can either be $+\hbar/2$ or $-\hbar/2$; in the first case we denote the state with the symbol $|\uparrow\rangle$ ('spin up'), in the latter case we denote it with the symbol $|\downarrow\rangle$ ('spin down'). We already encountered these states in Bohm's version of EPR's thought experiment, see Sect. 2.3.

In a consistent treatment of the measurement, the measurement apparatus will also be described by a quantum state. In order to measure the spin, the system and the apparatus must interact in such a manner that the state of the apparatus is correlated with the state of the system. Ideally, this goes on without the apparatus perturbing the system. For example, if the spin is measured in the z-direction, the interaction shall transform the uncorrelated initial states $|\uparrow\rangle|\phi_0\rangle$ ('spin up') and $|\downarrow\rangle|\phi_0\rangle$ ('spin down'), where $|\phi_0\rangle$ is the initial state of the apparatus, as follows:

$$|\uparrow\rangle|\phi_0\rangle \xrightarrow{t} |\uparrow\rangle|\phi_\uparrow\rangle, \quad |\downarrow\rangle|\phi_0\rangle \xrightarrow{t} |\downarrow\rangle|\phi_\downarrow\rangle. \tag{5.4}$$

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[24] Bell [13, appendix 1].

The state $|\phi_\uparrow\rangle$ ($|\phi_\downarrow\rangle$) is then interpreted as 'apparatus measured spin up' ('apparatus measured spin down'). Now, if quantum mechanics holds universally, the superposition principle holds universally. Then, according to Eq. (5.4), a superposition of spin up and spin down (resulting in a state with spin right or spin left) will develop as follows:

$$(|\uparrow\rangle \pm |\downarrow\rangle)|\phi_0\rangle \xrightarrow{t} |\uparrow\rangle|\phi_\uparrow\rangle \pm |\downarrow\rangle|\phi_\downarrow\rangle. \tag{5.5}$$

This, however, is nothing but the superposition of macroscopic states ('pointer states') of the measurement apparatus! Because one does not observe such a superposition (one always observes apparatus in definite classical states), von Neumann had postulated the collapse of the wave function, suspending the superposition principle during the measuring process and modifying the formalism of quantum mechanics, see Sect. 1.4.

Everett followed another path. He considered the superposition (5.5) *real.* But how do you explain that such states are never observed? The key to the answer is to explicitly involve the observer. Let $|O_0\rangle$ be the initial state of the observer before the measurement, $|O_\uparrow\rangle$ the state 'observer sees spin up ' and $|O_\downarrow\rangle$ the state 'observer sees spin down', then instead of (5.5) we have the following, larger superposition, that also includes the observer:

$$(|\uparrow\rangle \pm |\downarrow\rangle)|\phi_0\rangle|O_0\rangle \xrightarrow{t} |\uparrow\rangle|\phi_\uparrow\rangle|O_\uparrow\rangle \pm |\downarrow\rangle|\phi_\downarrow\rangle|O_\downarrow\rangle. \tag{5.6}$$

Does this not worsen the situation? No, says Everett. The expansion (5.6) means branching the wave function into independent components ('branches'), each corresponding to its own classical world. The whole quantum reality can thus be pictured as a world in which the same observer exists in two components of the wave function – one version of the observer sees spin up, the other version of the observer sees spin down. Thus, all possible outcomes of quantum measurements are physically realised in the full quantum world. Such a branching is robust due to decoherence which will be discussed in the next section.

The function $|\uparrow\rangle|\phi_\uparrow\rangle$ that is multiplied with the version $|O_\uparrow\rangle$ of the observer is called the 'relative state' with respect to $|O_\uparrow\rangle$ (and, accordingly, for the second component in (5.6)). This is why Everett called his interpretation the *relative state formulation.*

Of course, this picture not only holds for spin measurements, but for the measurements of all observables, be it the measurement of an electron's position or the hypothetical observation of Schrödinger's cat. According to the Everett interpretation, there is no superposition of a dead and a living cat in the classical world but rather a superposition of a world with a dead cat and a world with a living cat.

Everett's formulation does not separate system and observer. Von Neumann's psycho-physical parallelism (see Sect. 1.4) thus must be generalised. In his original formulation, von Neumann specifically states a connection between observer and observed system. In the Everett interpretation, there is merely a correspondence between the versions of the observer and the respective relative states of the system. In John Bell's words [15,

p. 133]: “The psycho-physical parallelism is supposed such that our representatives in a given ‘branch’ universe are aware only of what is going on in that branch.”

In the next sentence, Bell calls the Everett interpretation extravagant: “Now it seems to me that this multiplication of universes is extravagant, and serves no real purpose in the theory, and can simply be dropped without repercussions.” He therefore (at least in this respect)[25] prefers Bohm’s interpretation, which differs from the Everett interpretation only in that it adds classical particles (and field configurations) to the wave functions. After a measurement, these are trapped in a wave packet and describe the observed classical world.[26]

But is the Everett interpretation really extravagant? It follows quite naturally once you take the formalism of quantum theory seriously and do not manually introduce things, such as the collapse of the wave function. Looking at it that way, this interpretation is, in fact, minimalistic and corresponds directly to the formalism found in textbooks. Therefore, it does not seem quite right to consider it a standalone interpretation. From a fundamental point of view, there is only one quantum world – but with many classical, or better quasi-classical, components.

Bell’s discomfort is shared by many physicists. Bohm’s theory is an attempt at saving the idea of *one* macroscopic world. Other attempts go further and modify the Schrödinger equation by introducing additional, non-linear or stochastic terms. These terms are designed to cause the collapse of the wave function: superpositions such as (5.5) then develop according to these modified dynamics into one of their two components, with the probability given by Born’s rule; that is, the wave function ‘collapses’ into one of the two components.[27] Two of the most intensely discussed collapse models are the GRW model – named after its inventors Ghirardi, Rimini, and Weber – and the CSL model, which emerged from the GRW model.[28] Until today, there is no empirical evidence of a violation of Schrödinger’s equation and, thus, the validity of one of the collapse models. A detailed overview of collapse models and their experimental tests can be found in Bassi et al. [9].

Within the Everett interpretation, there is no EPR problem [173]. If we interpret (5.6) as a spin measurement in Bohm’s version of the EPR experiment, then the two possible outcomes with their corresponding versions of the observer physically exist in the combined state. Because of the non-locality of the quantum mechanical formalism,

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[25] He later sympathised with collapse models, especially the GRW model [15].

[26] Elsewhere, Bell described these classical variables as ‘beables’.

[27] Bell and Nauenberg commented on this collapse of the wave function (also referred to as ‘reduction of the wave packet’): “There are, ultimately, no mechanical arguments for this process, and the arguments that are actually used may well be called moral.” [15, p. 22] By ‘moral arguments’, the authors mean ideological or philosophical arguments.

[28] CSL stands for *continuous spontaneous localisation.*

Einstein's criterion of locality cannot be applied, and EPR's conclusion that quantum mechanics is incomplete cannot be drawn.[29]

Einstein did not get a chance to react to Everett's proposition, as he died in 1955, but Everett did meet Podolsky and Rosen at the Xavier conference in October 1962 (see Xavier University [172] for a transcript of the conference contributions). Peter Byrne describes their encounter in his biography on Everett [38, pp. 252–261]. The discussions were intense. Most conference participants considered Everett's interpretation valid and consistent, even if they were unwilling to accept its philosophical consequences. The same was true for Podolsky and Rosen. For Rosen, following the lines of EPR's arguments, the ongoing discussions of conceptual problems of quantum theory were further proof of the theory's incompleteness.

Reading the contributions to the discussion, one can feel the tension that builds up when you try to maintain the superposition principle as well as the linearity of the Schrödinger equation, but are unwilling to accept the consequences of the 'many worlds'. Everett commented [38, p. 255]:

> Yes, it's a consequence of the superposition principle that each separate element of the superposition will obey the same laws independent of the presence or absence of one another. Hence, why insist on having a certain selection of one of the elements as being real and all of the others somehow mysteriously vanishing?

Everett's original formulation brings up further important questions. For example, it is not clear which set of wave functions is supposed to be the basis of the branching. It is also unclear how Born's probability interpretation can result from a formalism that does not contain probabilities on a fundamental level. Everett was sure that his interpretation is consistent, but he could only give rudimentary answers to these questions. More precise answers could only be given after achieving a deeper understanding of how classical properties arise in a world that is fundamentally described by quantum theory. This is what the next section is about.

5.4 The Classical Limit

The concept of *measurement,* or rather the *measuring process,* plays a central role in discussions on the fundamentals of quantum theory. During the measurement, the Schrödinger equation is apparently suspended and the only state in a superposition that survives is the one that corresponds to the measurement result. John von Neumann has formalised this measurement process and introduced the collapse of the wave function as

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[29] For further reading on the many-worlds interpretation, beyond its relevance for EPR, the essays in Saunders et al. [140] and Zeh [179] can be recommended; also see Wallace [161]. Byrne [38] gives information on Everett's biography.

a new dynamical process, see Sect. 1.4. But why should the measurement of a system play such a crucial role?

Indeed, a measurement is nothing more than an interaction between two systems, where one system is the one to be measured, and the other one, the 'apparatus', is the one performing the measurement. So should we not simply call it an interaction with special properties? Perhaps more than any other person, John Bell spoke out against attributing a special role to measurements in the debate on the fundamentals of quantum mechanics. In his widely noticed paper "Against 'measurement' ", Bell wrote on the concept of measurement [14, p. 34]: "[...] the word has had such a damaging effect on the discussion, that I think it should now be banned altogether in quantum mechanics."

The problem of measurement in quantum theory is really part of a more general problem: How and when do classical properties form? So the actual issue is the problem of the classical limit – an aspect that goes unnoticed when you attribute a special role to measurement situations.

The problem of the classical limit had been discussed early on. During the Solvay Conference in 1927, Max Born asked how it could be understood that the trace of every alpha particle in a Wilson chamber appears as an (almost) straight line, although one needs a spherically symmetric wave function to describe its propagation. Two years later, Neville Mott presented his idea that the interaction of the alpha particles with atoms inside the Wilson chamber was responsible for the observed shape of the alpha particles traces [118]. This idea was not pursued in the time to come, probably due to Niels Bohr and the Copenhagen interpretation.

Decades later, Heinz-Dieter Zeh (Heidelberg, Germany) recognised how strongly quantum systems interact with the degrees of freedom in their environment and how important these interactions are for the classical limit. Macroscopic systems are always coupled with environmental degrees of freedom, (e.g., photons, scattering molecules, ...), so that they cannot be described as isolated systems. The Schrödinger equation can only be applied to the entire system, which is assumed to be closed; only from its solution for the entire system can one derive the behaviour of the subsystems. One finds that macroscopic subsystems generally show classical behaviour. The interactions with the degrees of freedom of the environment lead to a global entanglement of the system and its environment, making the system appear classical. This mechanism is referred to as *decoherence*.[30]

In the following, we shall briefly outline how decoherence results from the formalism of quantum theory.[31] A fundamental assumption is that this formalism indeed holds for all systems, without restrictions, and does not require modification by a dynamical collapse.

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[30] The term 'decoherence' was coined around 1989, probably by Murray Gell-Mann.

[31] See Joos [99], Joos et al. [101], Schlosshauer [142], and Zurek [182] for a detailed discussion. For the history of decoherence, see Camilleri [39] and Zeh [175].

In accordance with von Neumann, let us simply consider an interaction between a ‘system’ $\mathscr{S}$ and an ‘apparatus’ $\mathscr{A}$, correlating $\mathscr{S}$ and $\mathscr{A}$ without changing the state of the system; this being an ‘ideal measurement’ as described in the preceding chapter, cf. Eq. (5.4). Once again, we use the simple example of measuring the spin to demonstrate the process. If initially two states ‘spin up’ and ‘spin down’ exist in the system, the apparatus will be correlated with these states during the measurement according to Eq. (5.4).

Of course, due to the superposition principle, the state of the system may well be a superposition of different states with arbitrary complex coefficients α and β. The interaction then leads to

$$(\alpha|\uparrow\rangle + \beta|\downarrow\rangle)\,|\phi_0\rangle \xrightarrow{t} \alpha|\uparrow|\phi_\uparrow\rangle + \beta|\downarrow|\phi_\downarrow\rangle. \tag{5.7}$$

But this corresponds, as in (5.5), to a superposition of different states of the apparatus (‘pointer states’)! So far, we have only repeated von Neumann’s argument, in which he concluded the necessity for an additional dynamics (‘collapse’ or ‘reduction’ of the wave packet).

Considering Zeh’s idea, we now take into account the fact that the apparatus $\mathscr{A}$ is not an isolated system, but that it interacts with the degrees of freedom of the environment, which we shall denote by $\mathscr{E}$. If $|E_0\rangle$ denotes the initial state of the environment, then the state of the environment will be correlated with the states of the apparatus (and through this, indirectly with the states of the system) when apparatus and environment interact. Applying the superposition principle seems to worsen the situation, when compared to (5.7), because now the degrees of freedom of the environment also need to be taken into account:

$$\big(\alpha|\uparrow\rangle|\phi_\uparrow\rangle + \beta|\downarrow\rangle|\phi_\downarrow\rangle\big)\,|E_0\rangle \quad \xrightarrow{t} \quad \alpha|\uparrow\rangle|\phi_\uparrow\rangle|E_\uparrow\rangle + \beta|\downarrow\rangle|\phi_\downarrow\rangle|E_\downarrow\rangle. \tag{5.8}$$

This is an entangled state between, in general, many degrees of freedom that can be spatially quite far apart (as in the EPR situation). The crucial point of the matter, however, is that – in contrast to the apparatus – the degrees of freedom of $\mathscr{E}$ are not observable. For example, photons could scatter on the apparatus’ surface and then disappear irreversibly. What can really be observed locally (at the system or at the apparatus) ensues from the reduced density matrix (cf. Appendix). If one assumes (quite realistically) that the states of the environment for different n are nearly orthogonal, then the density matrix for (5.8) is:

$$\rho \approx |\alpha|^2|\uparrow\rangle\langle\uparrow| \otimes |\phi_\uparrow\rangle\langle\phi_\uparrow| + |\beta|^2|\downarrow\rangle\langle\downarrow| \otimes |\phi_\downarrow\rangle\langle\phi_\downarrow|. \tag{5.9}$$

But this is precisely the density matrix of a classical statistical ensemble of spin up and spin down. The information on possible interferences, expressed by the off-diagonal elements of the density matrix, passed into correlations between the apparatus and unavailable degrees of freedom of the environment: “The interference terms still exist, but they are not *there*!"[32] The discussion on spin measurement is, of course, valid for the interaction system-apparatus-environment in general.

The first quantitative calculations on decoherence in realistic situations were performed by Erich Joos and Zeh in 1985 [100]. Part of these applications concerned the important question of the localisation of objects. Because the superposition principle holds, one should not expect objects to be in a specific localised state; the general case in quantum theory is a superposition of localised states, i.e., extended states. Joos and Zeh showed that a very weak coupling to the degrees of freedom of the environment is sufficient for macroscopic objects to decohere, i.e., to localise. For example, a dust particle in interstellar space whose state is a superposition of various different locations will interact strongly enough with the 3 K cosmological background radiation, present everywhere in the universe, to appear as a classical (localised) particle if its radius is larger than only 10^{-3} cm. It is not the particle path that is being disturbed by scattering – it is the environment that is being changed. The interaction causes an entanglement with the environment; this entanglement causes the decoherence. Hence, entanglement not only is responsible for the pure quantum properties of the system, but also for the emergence of classical behaviour.

Thus, objects do not per se possess classical properties. To which degree they appear classical or not, depends on the details of their interaction with the environment. These details can be obtained from quantitative calculations. Because decoherence generally happens very fast, it looks like a spontaneous localisation, a 'quantum jump'. In contrast to a (never observed) dynamical collapse that would violate the Schrödinger equation, it is therefore referred to as an *apparent collapse* of the wave function. All observed phenomena, including all measurement processes, can (at least in principle) be consistently described by applying the Schrödinger equation to the entire system and restricting it to the subsystems in question. By consequence, a dynamical collapse has not yet been necessary to explain the outcome of any known experiment.

All experiments on decoherence, beginning with the first ones in 1996, have confirmed the theoretical predictions. The Vienna experiments on the slow disappearance of interference patterns through controlled interaction with the environment are worth highlighting. These interferences are created with, for example, fullerene molecules that are sent through a Talbot-Lau interferometer and interfere with themselves. Introducing a gas as a scattering environment [82] or heating for the emission of photons [83] make the interference pattern disappear, in accordance with the predictions by Joos and Zeh [100] and others.[33]

The Nobel lectures by Serge Haroche and David Wineland are impressive accounts of entanglement and decoherence experiments [84, 170]. The theoretical considerations on the classical limit have become quantum mechanical routine.

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[32] Joos and Zeh [100].

[33] Schlosshauer [142] provides a detailed discussion about the experimental situation in chapter 6.

Decoherence also plays a role in discussions on how relevant quantum mechanical superposition is for understanding the human consciousness. A possible relevance had been raised by Roger Penrose, among others, in the late 1980s. In detailed calculations, Max Tegmark was able to show that, due to decoherence, such superpositions in the brain – even if they were present – would disappear too rapidly to be of relevance for consciousness [155].[34] This example shows the wide scope of application of decoherence, i.e., of applications based on the quantum mechanical formalism.

The importance of decoherence is that it allows to explain the validity of classical concepts; at the same time, it can define the range of validity of these concepts. Objects *appear* classical, even if, on a fundamental level, they are described by quantum theory. The wave-particle 'complementarity', a historically relevant principle for quantum theory, follows naturally from applying quantum mechanics to realistic situations and from the process of decoherence. The fundamental concept of a state is a wave function in a generally high-dimensional configuration space, from which follow, according to the specific context, particle-like or wave-like properties in the three-dimensional space familiar to us.

Decoherence also solves a possible inconsistency in the Everett interpretation (see Sect. 5.3): With respect to which variables do the various branches of the wave function become mutually independent? The natural interaction with the environment selects a specific set of variables (for example, the position basis in the case of the localisation of objects discussed above). They define the robust, quasi-classical branches of the wave function. In Bohm's theory, it is the decoherent branches that carry the 'particles' as classical properties emerge, as opposed to the independent, empty wave packets.

With certain additional assumptions, the probability interpretation of usual quantum mechanics (the Born rule) can now be understood within the framework of the Everett interpretation. Many derivations, especially those making use of the density matrix concept, use circular arguments, because the desired outcome is already implicit in the ansatz, cf., e.g., Wallace [161, part II]. Some derivations, especially Zurek [183], work exclusively with the entangled state of the entire system and try to deduce the probabilities of the branches from their number of occurrences within the total wave function. Whether this constitutes an actual derivation of the probability interpretation or rather a consistency consideration, is a matter of debate. In any case, these analyses show that the Everett branches of the wave function can be interpreted consistently and realistically, at least as 'heuristic fiction' in the sense of Zeh [179, chapters 3 and 5].[35]

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[34] For example, a non-classical superposition could be a superposition of two states where one describes a firing neuron and the other describes a non-firing neuron. The firing of a neuron happens on a time scale of some milliseconds, whereas decoherence happens on a time scale of down to 10^{-20} s.

[35] For the term 'heuristic' cf. Footnote 6 in Chap. 1, page 4.

The probability interpretation can be applied as soon as there is decoherence. Then, the interferences between states that correspond to different ‘measurement results’ are no longer observable. This is also the moment when you are allowed to apply the ‘Heisenberg cut’ (see Sect. 4.4). The dynamical process of decoherence thus justifies the phenomenological interpretation of the theory. Without decoherence, the probability interpretation makes no sense.

The following (slightly modified) table from Joos [99, p. 194], sets the main properties of the Everett interpretation against the corresponding properties of the collapse models.

Collapse models	Everett
How and when does a collapse occur?	What is the exact structure of the Everett branches?
Traditional psycho-physical parallelism: perception is parallel to the *state* of the observer	New form of psycho-physical parallelism: perception is parallel to a *component* of the universal wave function
Probabilities are postulated	Probabilities may potentially be derived from the formalism (controversial)
Potential conflicts with relativity	No conflict with local interactions
Experimental test: Search collapse-like deviations from the Schrödinger equation ⇓ Seems impossible because of decoherence	Experimental test: Search for macroscopic superpositions ⇓ Seems impossible because of decoherence

The Everett interpretation (which makes use of the unaltered linear formalism of quantum theory) and collapse models (which explicitly modifies the Schrödinger equation) are in principle distinguishable in experiment. In macroscopic superpositions, this seems impossible because of decoherence, but it is possible and conceivable to test the predictions of *specific* collapse models in mesoscopic scenarios [9].

One objection against the Everett interpretation is that we do not perceive the other macroscopic components of the wave function; hence, they do not exist. But what would the world look like if the Everett interpretation was correct? Because of decoherence, it would look exactly the way we perceive it. This debate brings to mind the historic debate between the Ptolemaic and the Copernican system, which went on for centuries. Everett himself made this comparison in a *note added in proof* [72, p 460]:

> Arguments that the world picture presented by this theory is contradicted by experience, because we are unaware of any branching process, are like the criticism of the Copernican

> theory that the mobility of the earth as a real physical fact is incompatible with the common sense interpretation of nature because we feel no such motion. In both cases the argument fails when it is shown that the theory itself predicts that our experience will be what it in fact is. (In the Copernican case the addition of Newtonian physics was required to be able to show that the earth's inhabitants would be unaware of any motion of the earth.)

Only future developments in physics will allow for a final decision in this debate.

Future Relevance

6

Numerous experiments have confirmed quantum theory in all its aspects. The entanglement of systems as *the* main trait of the theory – and the nature portrayed by it – has been empirically validated. Paradoxes only arise when trying to explain the phenomena using classical assumptions. But classical properties turn out to be mere approximations and result from properties of the entanglement – the entanglement of the considered degrees of freedom of the system with the irrelevant degrees of freedom of the natural environment coupled to the system; this is the process of decoherence discussed in the preceding chapter.

Entangled quantum systems will continue to play an important role in the future, both in fundamental discussions and in practical applications. An in-depth explanation of these developments would be beyond the scope of this work, but a brief summary is warranted.

- Technology to create and alter entangled systems is developing at stunning speed. When discussing the test of (violation of) the Bell inequalities, we mentioned the three-photon entanglement described by Erven et al. [70]. Further examples are the entanglement of 10^5 photons [96] or the entanglement of diamonds [109]. The latter refers to creating entangled vibrational states of two millimetre-sized diamonds at room temperature with 15 cm between them; however, after 7 ps decoherence sets in. Palomaki et al. [123] describe how to create and prove the entanglement of a mesoscopic mechanical oscillator with an electromagnetic microwave field.[1] Gerlich et al. [77] present interference experiments with large organic molecules. These examples are only a few of numerous publications on the experimental realisations of entangled states.

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[1] This is one of many modern texts where the EPR paper is the first reference.

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- Entanglements play an important role in the relatively new area of *quantum information*. Growing interest in this area is probably the reason why fundamental questions of quantum mechanics have regained attention. In quantum information one tries to develop, among other things, a quantum computer; it could use entanglements by realising parallel calculations in all components of a superposition (but not in many macroscopic worlds). This would, for example, solve the problem of factorising large numbers. Decoherence makes building a quantum computer difficult, which is why it is questionable whether a large enough quantum computer will ever be operable.

  Other topics in quantum information are quantum teleportation and quantum cryptography. In these fields, researchers have created entangled states ('EPR correlations') over distances greater than a hundred kilometres. Situations such as the one faced by EPR have become routine (in physics). The abundant publications on the rapidly evolving topic of quantum information are difficult to keep track of. Nielsen and Chuang [119] give a comprehensive overview; Bruß [34] and various articles in Audretsch [3] are also recommendable.
- In Sect. 5.4, we stated that non-classical quantum states do not play a role in creating consciousness due to decoherence. Nevertheless, the importance of quantum effects is discussed in biology; see, for example, Huelga and Plenio [92] and O'Reilly and Olaya-Castro [120]. One research topic, for example, is quantum effects during photosynthesis and the sensitivity of birds in relation to magnetic fields. The field is referred to as quantum biology, a term that Pascual Jordan has coined in the 1940s. It is important to note that biological systems are *open* systems, as they are intensely coupled with their environment. It remains to be seen how this field will develop and what kind of insights it will bring forward.
- In particle physics, entanglements are usually not relevant. An exception are the quantum mechanical oscillations of neutrinos and neutral mesons. These phenomena are used, for example, to differentiate effects that can be predicted by collapse models and decoherence effects [7].
- EPR-correlations also play an interesting role in quantum field theory and cosmology. Flat space-time (i.e., in absence of gravitation) produces important effects for a uniformly accelerated observer [160]. Such an observer perceives the normal vacuum state as filled with thermally distributed particles. How can that be? The vacuum state is a non-local global state. However, the accelerated observer cannot access the entire space; there are horizons that hide a part of space-time. The observer cannot perceive the correlations of the vacuum state behind the horizon and must therefore use a reduced density matrix that describes the part of space-time inside the horizon. As it turns out, this density matrix describes a thermal distribution of particles; also known as the Unruh effect.

  Similar effects occur in relation to black holes and in cosmology; see, for example, Matín-Martínez and Menicucci [116] for an overview. In cosmology, EPR-correlations are important to understand primordial quantum fluctuations in an early (the inflationary) phase of the universe. These quantum fluctuations are the seeds of cosmic structure formation after decoherence has occurred (Kiefer et al. 1998). In all these cases, EPR-states are two-mode squeezed states in the form they are studied in quantum-optical laboratory experiments. The Hawking radiation of black holes, too, can be explained with these states.

- So far, everything points to the universal validity of quantum theory. The superposition principle has proven to be effective and the search for its limits has not been successful, see Arndt and Hornberger [1]. Since quantum systems interact with their environment and vice versa one should, for consistency reasons, also describe the universe as a whole – as it is the only truly closed system – within the framework of quantum theory. This leads us to quantum cosmology and the universal wave function [105]. On cosmic scales, gravitation is the dominating interaction, which brings us to the yet unsolved problem of quantum gravity (see, for example, Kiefer [105, 106]). Richard Feynman already used a simple thought experiment to show how the superposition principle applies to a gravitational field [178]. Entangled states in the sense of Einstein, Podolsky and Rosen play a particularly important role in quantum cosmology.

The EPR paper has sparked the interpretational debate on quantum theory like almost no other paper has. The debate is still ongoing, as shown in Schlosshauer et al. [143], Leifer [110] and numerous other papers. To quote just one example, here goes an excerpt of an interview related to Weinberg's textbook on quantum mechanics [163]. On the question of how to interpret quantum theory, Weinberg said:[2]

> Some very good theorists seem to be happy with an interpretation of quantum mechanics in which the wavefunction only serves to allow us to calculate the results of measurements. But the measuring apparatus and the physicist are presumably also governed by quantum mechanics, so ultimately we need interpretive postulates that do not distinguish apparatus or physicists from the rest of the world, and from which the usual postulates like the Born rule can be deduced. This effort seems to lead to something like a "many worlds" interpretation, which I find repellent. Alternatively, one can try to modify quantum mechanics so that the wavefunction does describe reality, and collapses stochastically and nonlinearly, but this seems to open up the possibility of instantaneous communication. I work on the interpretation of quantum mechanics from time to time, but have gotten nowhere.

Therein lies the predicament of most physicists. The quantum mechanical formalism has satisfied all experiments and observations so far. In particular, the apparent collapse explained by decoherence has been sufficient to interpret all experiments. If you do not change the formalism, you end up in the many-worlds interpretation detested by Weinberg. If you want to avoid this interpretation, you must change the formalism, which usually is tried using collapse models, bringing about their own problems.

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[2] See *Physics Today online*, July 2013.

The locality criterion pivotal for EPR has turned out to be false, as it contradicts both the established formalism of quantum theory and the experiments based solely on this criterion – namely the numerous experiments proving the violation of the Bell inequalities.

So, can the quantum mechanical description of physical reality be considered complete? The answer to the EPR question is definitely *yes*. This by no means implies that quantum theory is actually complete; it only means that we *can consider it* complete based on what we know today.

The work of Einstein, Podolsky and Rosen is still highly relevant in the twenty-first century. A fact that shows a sense of discomfort towards a structure of nature far from all classical perception, with consequences that still remain uncharted. Many questions have been answered, not least due to breathtaking experimental progress. Though whether the conflict about the interpretation of quantum theory will ever be solved, is yet to be determined. We will let Einstein have the final say:

> We are free to choose what we strive towards and we can all find solace in the fine words by Lessing that striving for the truth is far more precious than possessing it.[3]

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[3] “Die Richtung des Strebens steht jedem frei, und jeder darf Trost schöpfen aus Lessings schöner Bemerkung, nach welcher das Streben nach der Wahrheit köstlicher ist als deren gesicherter Besitz.” (Einstein [58, p. 121], English translation by S. Linden and A. K. Hudert)

The Formalism of Quantum Theory

A

This appendix shall provide a brief summary of the formalism of non-relativistic quantum theory. To this end, I will follow the chapter on the mathematical formalism from my book on quantum theory [103].[1]

The core of quantum theory is the superposition principle: when ψ_1 and ψ_2 are physical states, then $\alpha\psi_1 + \beta\psi_2$ with arbitrary complex numbers α and β also is a physical state. For this reason, the state space must be linear.

Another requirement is the existence of an inner product (a scalar product) to introduce the probability interpretation into the formalism ('Born's Rule'). This leads to the concept of a Hilbert space. (The completeness property of a Hilbert space is only needed for mathematical convenience.) Thus, quantum mechanical states are elements (state vectors) of Hilbert space. Paul Dirac introduced a widely-used notation for these vectors [49]. In it, a state in Hilbert space is written as $|\psi\rangle$ and referred to as *ket*. Mathematically, one can introduce a covector, called *bra* and written as $\langle\psi|$, as an element of the dual vector space.[2]

The scalar product between two states Ψ and Φ is denoted by $\langle\Psi|\Phi\rangle = \langle\Phi|\Psi\rangle^*$, where * indicates the conjugate complex.[3] The probability of measuring the state ψ_n on a system that is in state Ψ, then is: $p_n = |\langle\psi_n|\Psi\rangle|^2$. The Hilbert space usually has infinite

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[1] For more details, cf. textbooks and monographs on quantum mechanics, such as Auletta [4], Busch et al. [36], d'Espagnat [47], or Peres [131], which also include detailed discussions of conceptual questions.

[2] In linear algebra, the ket vector corresponds to a column vector, and the bra vector to a row vector.

[3] This explains Dirac's notation, since 'bra' and 'ket' together form a *bra(c)ket*.

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dimensions. For example, one describes N particles via wave functions $\psi(\mathbf{x}_1, \ldots, \mathbf{x}_N)$ that are square-integrable, i.e., they satisfy

$$\int_{-\infty}^{\infty} \mathrm{d}^3x_1 \cdot \ldots \cdot \mathrm{d}^3x_N \, |\psi(\mathbf{x}_1, \ldots, \mathbf{x}_N)|^2 < \infty \, . \tag{A.1}$$

The integral must not be infinite, because there is a probability that the 'particle' is somewhere in space. Usually one normalises the integral to 1. Condition (A.1) is a strong restriction of the admissible physical states. Most importantly, it yields the discrete energy values that are so characteristic for quantum theory. For example, the Hilbert space for spin $1/2$ has two dimensions, corresponding to the two possible orientations of the spin with respect to a given direction.

How does one describe the quantum mechanical analogies of quantities familiar to us from classical physics like location, momentum or energy? These 'observables' are represented by self-adjoint operators in Hilbert space. Possible measurement outcomes are the eigenvalues of these operators. When applied to a state Ψ that lies within the operator's domain D_Ψ in Hilbert space, the operator uniquely assigns to it another state in Hilbert space. For this, the operator's domain as well as the mapping rule are important. In quantum theory, only linear operators are relevant. Let $\hat{A}$ denote an operator, then $\Psi' = \hat{A}\Psi$ is the new state. The operator $\hat{A}$'s adjoint operator $\hat{A}^\dagger$ is defined via the scalar product:

$$\langle \Psi | \hat{A}^\dagger \Phi \rangle = \langle \hat{A}\Psi | \Phi \rangle \, . \tag{A.2}$$

This definition is valid for arbitrary states Ψ and Φ. For self-adjoint operators we have $\hat{A} = \hat{A}^\dagger$, including the identity of their domains. Note the validity of the important spectral theorem: The set of all eigenvectors of a self-adjoint operator constitutes an orthonormal basis of Hilbert space. Thus, every state can be expanded with respect to that basis. Because a self-adjoint operator can, with respect to that basis, be represented by a matrix (of generally infinite dimension), the term 'matrix mechanics' has been used. The eigenvectors of self-adjoint operators are real, so that possible measurement results are always described by real numbers. When many measurements are performed on a given state Ψ, the expectation value $\langle \hat{A} \rangle$ (weighted average of all the possible outcomes of a measurement) is given by $\langle \Psi | \hat{A}\Psi \rangle$. For the measurement outcomes will in general be dispersed around this value, one defines the dispersion $\Delta\hat{A}$:

$$\left(\Delta\hat{A}\right)^2 = \langle \hat{A}^2 \rangle - \langle \hat{A} \rangle^2 \, . \tag{A.3}$$

Of course, these notions have been taken from the theory of statistics and found their place in quantum theory due to the latter's probability interpretation. For self-adjoint operators $\hat{A}$ und $\hat{B}$ the following relation holds for any state Ψ:

$$\Delta\hat{A} \cdot \Delta\hat{B} \geq \frac{1}{2} |\langle\Psi| \left[\hat{A}, \hat{B}\right] \Psi\rangle| , \tag{A.4}$$

where $\left[\hat{A}, \hat{B}\right] = \hat{A}\hat{B} - \hat{B}\hat{A}$ is called the commutator. Equation (A.4) is called the generalised uncertainty relation. For the commutator of the position operator $\hat{x}$ and the momentum operator $\hat{p}$ in one dimension we have:

$$\left[\hat{x}, \hat{p}\right] = \mathrm{i}\hbar \ . \tag{A.5}$$

With (A.4), this yields the uncertainty relation for position and momentum (1.5).

An important class of self-adjoint operators are the projection operators $\hat{P}$. They project states in Hilbert space into linear subspaces and satisfy the relation $\hat{P}^2 = \hat{P}$. Their eigenvalues are 1 (for vectors $\hat{P}$ projects onto) and 0 (for vectors orthogonal to the subspace). The spectral decomposition of a self-adjoint operator $\hat{A}$ then reads

$$\hat{A} = \sum_n a_n \hat{P}_n , \tag{A.6}$$

where a_n are the eigenvalues of $\hat{A}$ (and where, for simplicity reasons, the possibility of degenerate eigenvectors has been excluded). Now probabilities can be described as expectation values of projection operators; e.g., for the above-mentioned probability p_n the following relation holds:

$$p_n = |\langle\psi_n|\Psi\rangle|^2 = \langle\Psi|\hat{P}_n\Psi\rangle \ , \tag{A.7}$$

where $\hat{P}_n$ projects onto the state ψ_n.[4]

Instead of representing states in position space, we can represent them in another basis; e.g., in momentum space or in energy space. The representation of a vector in momentum space is obtained by Fourier-transforming its representation in position space.

The time evolution of states is governed by the Schrödinger equation:[5]

$$\mathrm{i}\hbar \frac{\partial\Psi}{\partial t} = \hat{H}\Psi \ . \tag{A.8}$$

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[4] Instead of projection operators, a generalisation of this formalism uses more general operators that still satisfy (A.7). The corresponding *positive operator valued measures* (POVMs) allow for the treatment of imprecise measurements and of combined measurements of multiple quantities, cf., e.g., Wallace [161, p. 17*ff*].

[5] Cf. Eq. (1.4) on page 10.

$\hat{H}$ is called the *Hamiltonian*. It is the quantum theoretical representation of the observable 'energy' and is of course self-adjoint. The linear structure of the Schrödinger equation is an expression of the dynamical version of the superposition principle: The sum of two solutions to the equation is itself a solution to the equation. The overall probability (A.1) is constant throughout the time evolution described by the Schrödinger equation (which is the reason for the appearance of the imaginary unit i on the left-hand side of Eq. (A.8)).

Solutions of the Schrödinger equation that are of the form

$$\Psi(x,t) = \psi(x)\mathrm{e}^{-\mathrm{i}Et/\hbar} \tag{A.9}$$

are called stationary states. With (A.8) we obtain for $\psi(x)$ the time-independent Schrödinger equation

$$\hat{H}\psi(x) = E\psi(x)\,, \tag{A.10}$$

where E is the energy. Atomic spectra are calculated using this formula. The existence of discrete energy values E_n is a direct consequence of the normalisation (A.1).

Besides self-adjoint operators, unitary operators are especially important in quantum theory. They are operators that preserve scalar products, i.e., probabilities. Their importance arises from the fact that they are generally linked to symmetries of the physical system (invariance under rotation, translation, etc.). This is expressed mathematically by Wigner's theorem which states that a unitary (or antiunitary) operator exists for any transformation of states preserving the scalar product. There is an important connection between self-adjoint and unitary operators: when $\hat{A}$ is self-adjoint, then $\exp(\mathrm{i}\hat{A})$ is unitary and vice versa. From this follows the time invariance of the probability: Because $\hat{H}$ is self-adjoint, the time evolution operator $\exp(-\mathrm{i}\hat{H}t/\hbar)$ for the states is unitary.

The concept of entanglement that is so crucial for the EPR discussion can then be defined as follows (cf., e.g., Bruß [33]). Let the quantum system $\mathscr{S}$ be described by a state vector $|\psi\rangle$. The system shall consist of two subsystems ('bipartite quantum systems') $\mathscr{S}_1$ und $\mathscr{S}_2$. Then the state vector $|\psi\rangle$ shall be called *entangled* with respect to $\mathscr{S}_1$ and $\mathscr{S}_2$ if it cannot be expressed as a tensor product of state vectors $|\psi_1\rangle$ (from $\mathscr{S}_1$) and $|\psi_2\rangle$ (from $\mathscr{S}_2$), i.e.

$$|\psi\rangle \neq |\psi_1\rangle \otimes |\psi_2\rangle. \tag{A.11}$$

The states (2.1) and (2.9) used by EPR show exactly this behaviour; they cannot be written as a product of states that refer to particle I and II respectively – thus, they are entangled.

Because of quantum mechanical entanglement, subsystems that are coupled to other systems generally do not have a state (a wave function) of their own. Rather, subsystems are described by density operators $\hat{\rho}$ (also referred to as density matrices or statistical operators). They are obtained from the state of the whole system by taking a 'partial trace' of the density operator of the whole system, sorting out all degrees of freedom that are not part of the subsystem. By virtue of these density operators one can obtain probabilities and expectation values referring to that subsystem. Density operators play an important role in the study of decoherence (Sect. 5.4). Because of entanglement with other systems (the 'environment'), $\hat{\rho}$ in general does not obey a unitary time evolution, since information can escape to or migrate from the environment through correlations. For these 'open systems', the Schrödinger equation is not appropriate anymore, but instead a (generally very complex) equation governs the time evolution of ρ. For example, for the simple case of particles (air molecules, photons, ...) scattering off a massive object, this object is not described by the Schrödinger equation but by the following:

$$\mathrm{i}\hbar\frac{\partial\hat{\rho}}{\partial t} = [\hat{H}, \hat{\rho}] - \mathrm{i}\Lambda\hbar[\hat{x}, [\hat{x}, \hat{\rho}]]. \tag{A.12}$$

In this *master equation*, $\hat{x}$ is the position operator (scattering takes place in position space, not in momentum space) and Λ is the localisation rate. The value of Λ indicates how much the object is localised by interaction with its environment. Λ is also responsible for suppressing the dispersion of the wave function, as it should occur if the time evolution was described by the Schrödinger equation, by interacting with the environment.

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