Reception and Impact of the EPR Paper

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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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* 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

[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).

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.