Comments on the foregoing paper by M. A. Whitaker

Damian Canals-Frau

Maglegårds Allé 48, 2860 Søborg, Danemark.

ABSTRACT. The paper is an answer to Whitaker's treatment of some controversial quantum mechanical physical points, but seen in a different light. The subjects dealt with are : EPR's problem ; Bohr's struggle to impose a new physics ; the so-called reduction of the wave function ; the Bell affair ; the disturbance interpretation of quantum mechanics ; the problem of interpretation in quantum mechanics. The conclusions reached here are different from Whitaker's.

RÉSUMÉ. Cette étude est une réponse à celle de Whitaker, qui traite de thèmes controversés de physique quantique, mais ici ils sont vus sous un autre angle. Les points traités sont : la problématique d'EPR ; la lutte de Bohr pour imposer une nouvelle vision de la physique ; le problème de la réduction du paquet d'ondes ; l'affaire Bell ; la controverse sur l'altération provoquée par la mesure ; la difficulté de l'interprétation de la mécanique quantique. Les conclusions différent de ceux de Whitaker.

Introduction

I have read with great interest M.A.B. Whitaker's "Analysis of Some Arguments on Quantum Interpretation" [1]. He makes repeated references to my paper [2], which comments on some points of his book "Einstein, Bohr and the Quantum Dilemma" [3]. It is possible that my paper would have been different in some details if, prior to publishing it, I had had an exchange of ideas with Whitaker. Dare one hope that a second edition of his book will contain some modifications concerning some of his former arguments ?

There certainly are misunderstandings, e.g., concerning Bohr, Einstein and Bell. Moreover, I believe that our disagreement concerning physics is based on our different backgrounds. When I write "physics", I mean modern physics, quantum physics included. There are disagreements that I would call minor, as for example, when Whitaker writes "It is scarcely ever possible to maintain that even the most accomplished and accepted theoretical development rests *unambiguously* (emphasis mine) on experimental evidence." He is probably right, but the statement can give non-specialists the impression that physics is not an experimental science but a kind of metaphysical description. (Concerning Whitaker's ironical definition of the concept "metaphysical" (see [3] p. 162), let me say that I never drink.) I agree that "experimentally based" is a good concept for physical statements and physical theories, but I believe that the concept "physical fact" is also necessary. For example, in physics, when we postulate something, the postulate must, more or less, be physically based, but it is not a physical fact. (To postulate = "to assume without proof to be true, real, or necessary, esp. as a basis of argument" (Webster's)).

If I had had a conversation with Whitaker, then, instead of writing "...if pictures are constructed on the results of experiments, i.e., on physical facts, they will be accepted by all physicists without discussion", I would have written "...if pictures are physically based they will be accepted easily by physicists". Whitaker writes "Physical data which obeys classical theory, for instance, may be analysed in three completely different but equally valid ways, in Newtonian theory, relativity theory and quantum theory ; the statements in each analysis cannot be described as "physical" since they will differ in the three cases." I believe that this statement needs a more thorough analysis, because a description that is not physical, certainly does not belong to physics. I thing that, as in mathematics, we must specify for each theory its "domain of validity" or its "domain of applicability". But in physics, this domain is the region (of variables) where each theory is experimentally based. So, there are no faulty physical theories. And, if there are regions where two domains of validity overlap, then there are two legitimate physical theories in those regions, because it is the result of measurements that are the physicist's "touchstone", even if "common sense" will not admit it. Remember that the extrapolation of an experimentally based theory is *not* part of a physical theory until an experiment settles the question.

Let me say a word about Whitaker's "analysis of situations" and "analysis of theories". The former are certainly nearer to working experimental physicists, the latter, to theorists.

Whitaker writes, (p. 6) "It will be noted that, as set out above, the EPR analysis made no assumptions" ; (p.7) "We do note, though, that EPR refer to reality as a "criterion", in fact an assumption."

Now let us see the important points of disagreement : EPR, reduction of the wave function, Bell, Einstein, Bohr, disturbance interpretation and interpretation of quantum theory

Concerning EPR [4]

Since the forties, my teachers and I have always –perhaps naively- considered the EPR paper to be metaphysical speculation, because it is based on the hypothesis of a physical reality (something that the man in the street considers to be a physical reality). In this context, it is easily understandable that later, when I heard that Bell's work could question quantum mechanics and authenticate EPR's speculations as being physically based, I immediately thought that there must be something wrong with Bell's analysis.

Whitaker considers that the EPR paper enters into the category of "analysis of theories", particularly of quantum theory. Why not ? It was certainly "brilliantly successful" because 65 years later it continuous to be a bone of contention. What if, in fact, this longevity was just a proof of its ambiguity ? I disagree with EPR's analysis because their analysis of the quantum theory is based, for example, on the "reduction of the wave function", a quantum mechanical *postulate*, frequently misinterpreted as a physical fact. Let us take a naively but somewhat clearer look at the problem. I say naïve, because, like the young Einstein (please excuse this pretentious comparison). I prefer go back to simple facts and procedures instead of basing my reasoning on (sometimes extravagant) interpretation of quantum theory.

EPR consider a property of two systems a long time after they have interacted. They have no data to write state functions for each system after the interaction, but only data for the global system and so, the description of the global system evolves with the characteristic of not having separated wave functions for each of the two partial systems. They want to measure a physical quantity A where they know the possible values a_1, a_2 ... (eigenvalues), pertaining to system I and the corresponding eigenfunctions $u_1(x_1)$, $u_2(x_1)$, ... as solutions of the Schrödinger equation. If x_2 are the variables of system II, they say that the global wave function can be written $\Psi(x_1, x_2) = \Sigma_n \psi_n(x_2) u_n(x_1)$, [7]

(EPR formula 7). Then EPR write : "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 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, ... and the eigenfunction $v_1(x_1)$, $v_2(x_1)$, ... we should have obtained, instead of eq. [7], the expansion $\Psi(x_1, x_2) = \Sigma_s \varphi_s(x_2) v_s(x_1)$, [8], where the φ_s's are the new coefficients. If now the quantity B is measured and it 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 case ψ_k and φ_r) *to the same reality* (the second system after the interaction with the first)." (End of quotation).

EPR's statement that the quantum mechanical description of physics is not complete is based on this reasoning. I do not know if our quantum mechanical description of our quantum mechanical world is complete or not complete, but I know that it gives unambiguous and accurate predictions for all known phenomena and also for sophisticated experiments devised by humans. It is easy to see that, from the purely physical point of view, EPR's reasoning – a hybrid of classical and quantum arguments – is not correct in terms of physics, and it is strange that Einstein accepted this hybrid description, in which mathematics serves to obscure rather then enlighten. At present we know that Einstein regretted Podolsky's editing of the EPR paper [1].

[1] Deltete and Guy (5) state that is was Podolsky who wrote the EPR

EPR's misapprehension derives from using both classical and quantum reasoning to deal with a quantum mechanical problem. The fact is that the modern physicist, the quantum physicist, cannot merely say, "Suppose that we measure the quantity A". This statement has physical meaning for the quantum physicist, only if he sets up the necessary apparatus to measure A, e.g., a Stern-Gerlach oriented in a given direction, an oriented linear polarizer, (remember, that the a_1, etc. are also characteristics of the measuring apparatus). This means that to avoid misinterpretations and to describe the situation correctly, the left hand member of [7] must be written, for example, as $\Psi(x_1, x_2; A)$; and for [8], $\Psi(x_1, x_2; B)$; (the eigenvalues b_r are not eigenvalues of A). Therefore, as the measurement of A or B need completely different set-ups (let me say that the universe described by $\Psi(x_1, x_2 : A)$ is different from the one described by $\Psi(x_1, x_2; B)$, it turns out that EPR's reasoning cannot show that quantum mechanics is not a complete theory.

We must also recall that, as EPR have no data to write wave functions for each of the partial systems that have interacted, the mere mathematical development [7] cannot make appear individual wave functions and so each term of [7] refers to the two systems : the sum of products of functions corresponding to I and II, and this is true even if systems I and II are widely separated. A measurement of the x_1 system gives, e.g., the eigenvalue a_k and the corresponding wave function $u_k(x_1)$, and in the development [7] we find that the k-term gives, simultaneously with u_k, also ψ_k, a function corresponding to II. (See chapter "Bohr" for a detailed analysis). There are many confusing statements in EPR's paper. One of many examples, at the bottom of p. 778 : "In quantum mechanics 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." A correct statement is : in quantum mechanics it is assumed that the wave function *does* contain all that the physicist knows about the physical system in the state to which it corresponds. (No human can properly claim to describe more than what he knows.)

These are some of the reasons why I have always considered the EPR paper to be ambiguous and speculative, and not underline proper physics.

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paper, after many discussions. Einstein thought that his own arguments for incompleteness were poorly present in the paper and that the main point had been "buried by erudition". Deltete and Guy evaluate Einsteins own incompleteness argument, based on his correspondence with Schrödinger.

Concerning the reduction or collapse of the wave function

The wave function is a mathematical expression based on the physicist's knowledge of the physical situation he works with. It contains all possible information concerning his problem. It is a human construct, generally obtained with the help of Schrödinger's equation. We call it also probability amplitude, because we have learnt that its modulus squared can be interpreted as a probability function. As a measure is a physical intervention in the natural course of Nature, it is a modification of the former situation and the knowledge of the result –say a_k – is a new bit of information. This information renders the former wave function "null and void". Quantum mechanists postulate that, after the measurement, the primitive wave function "collapses" to the partial wave function associated with a_k . This is the postulate of the reduction of the wave function. This can be a misleading statement, because the collapse is not a physical, dynamic process, but simply the replacement of obsolete, with fresh, information. Thanks to the act of measurement we know something more about our system. When does the *reduction* of the mathematical expression take place ? In fact, never. The measurement itself is the modification of the course of natural events. Physicists are aware of the result (a consequence of the modification) only if it is registered (by a display or a pointer) and they look at the register. This can happen a long time after the universe has been modified. So, the knowledge of the new datum does not collapse but renders the physicist's primitive wave function "null and void". And what we call the postulate of the reduction of the wave functions is only a clever mathematical remark (based on our mathematical description of the measuring process), which makes it easier to write down the new wave function. It is not a physical fact. Another way to say this is to recall that any probability function loses its meaning with a new datum. Let me add that the Schrödinger equation describes the natural change in a given system. A measurement interrupts this natural change and compels the system to do something else before it begins a new natural change.

Bell [6]

Let me try, once more, to make comprehensible my reasoning concerning Bell's work. It is evident that Whitaker has not grasped the logic of my criticism. On the basis of several premises, Bell developed a formulation of a kind of EPR problem. His conclusion was a mathematical inequality, whose hypothetical physical meaning goes no further

than his premises. Experiments showed that the inequality is violated. Therefore, I conclude that Bell had not included among his premises, the one that would make his inequality agree with physics (I mean, with the experimental results). Only if his premises, taken together, gave the correct experimental results, could he legitimately, logically, analyse the consequences of the absence of one of them (in his case, locality). Since this is not the case, we cannot know (Bell could not know) whether the whole set of his premises is inadequate (Bell decided that locality is superfluous) **or** merely incomplete (lacking, at least, one premise) in the microphysical domain. By microphysical domain I mean the domain of physics where classical physics does not apply and classical reasoning are suspect, e.g., most of the problems treated in quantum optics. Aspect's test –and so the problem Bell tried to formulate- cannot be explained classically, it belongs to the domain of microphysics. We must remember that Bell's conclusion follows from the usual laws of logic, *combined with empirical facts*. It is this combination with empirical facts that makes Bell's conclusion physically unreliable and logically unsound. From Bell's work and the experiments performed we *cannot* conclude that locality is false. From Bell's work and the experiments performed we *cannot* conclude that physics is non-local. On the basis of Bell's work and the violation of his inequality by Nature, non-locality can be considered only as a hypothesis, an assumption, a speculative idea but not as a physical fact. At present, even Bell's most enthusiastic supporters recognise (indirectly) that Bell's 1964 paper is not physics at all. They express this by saying something such as "...they understand perfectly, and also Bell understood, the inadequacy of all experiments for his theorem". Physics being an experimental science, the result of experiments is the physicist's "touchstone". How much ink would have been spared if Bell, in his 1964 paper, had written openly that he was putting forward a mere metaphysical speculation!

These are some simple, and other more subtle reasoning that show why I consider Bell's work to be mere speculation and not proper physics: the experiments violate his inequality, and the conclusion he draws from this failure is not a legitimate inference, but a fallacy. Let me add that Aspect's [7] type of test have only confirmed the quantum mechanical prediction. And the claim that the measurement in the left "wing" determines, supraluminously, the result of the measurement in the right "wing", is refuted by the correct interpretation of the quantum mechanical formalism.

Let us look once more at the quantum mechanical "mechanism" and try to elicit its meaning [8]. We have a source that emits two specific photons in opposite directions and a polarizer in each "wing". From the study of the source (Ca, or also Hg, cascade) we know that we cannot write state functions for the polarization state of each photon, but only a state function that describes the polarization state of the two photons propagating in the $\pm z-$ direction.

$$|\Psi_{AB}\rangle = 2^{-(1/2)}(|X_A\rangle|X_B\rangle + |Y_A\rangle|Y_B\rangle)$$

where $|X_A\rangle$ and $|X_B\rangle$ are the polarizations states of photons A and B in the x-direction ; likewise for the y-direction. We see that this state function is not a usual state function because each term is a direct product of the individual ket vectors pertaining to different vector spaces. This state function embodies the quantum property Schrödinger named "entanglement" : it is the origin of what is called "EPR correlation". The A photon, after passing the polarizer A, is in the state described by

$$|\Phi^A\rangle = \cos\Phi^A|X_A\rangle + \sin\Phi^A|Y_A\rangle$$

likewise for photon B and polarizer B

$$|\Phi^B\rangle = \cos\Phi^B|X_B\rangle + \sin\Phi^B|Y_B\rangle$$

where Φ^A and Φ^B are the angles of polarizers A and B with the x-axis.

The probability amplitude that the polarization state of the two photons state $|\Psi_{AB}\rangle$ is also the polarization state of the B polarizer, is

$$\langle\Phi^B|\Psi_{AB}\rangle = 2^{-(1/2)}\left[\cos\Phi^B|X_A\rangle + \sin\Phi^B|Y_A\rangle\right] = |U^A\rangle$$

Here the parenthesis looks like a strange polarisation state. Yet the probability that photon B passes its polarizer is $|U^A\rangle|^2 = 1/2$, as it must be.

It is instructive to introduce the angle $\alpha = \Phi^B - \Phi^A$ between polarizers A and B.

$$|U^A\rangle = 2^{-(1/2)} = [\cos\alpha|\Phi^A\rangle + \sin\alpha|\Phi^A + (\pi/2)\rangle]$$

Now, the parenthesis of the state function can be interpreted as a hypothetical polarizer with axis fixed by Φ^A and $\Phi^A + (\pi/2)$ and forming

an angle α with polarizer B. In other words, the state of polarization of photon B is expressed as a function of the state of polarization of a photon having passed polarizer A. It is obvious that after passing polarizer B, photon B is in the corresponding state of polarization. But what is not so obvious is that this state can be expressed (not arbitrarily, but as a consequence of the particular two-photon state $|\Psi AB\rangle$) in the basis of polarizer A. Before measuring the second photon (photon A) we have enough data to write the state function corresponding to the passing of photon A through polarizer A, and this, because of the particular two-photon state function $|\Psi_{AB}\rangle$. To sum up : Because of the particular ("birth") link between photons A and B (not as Siamese twins but as ordinary identical twins : they are independent but are related by their appearance, character, etc) embodied in the two photon state function $|\Psi_{AB}\rangle$, they share a property (sum of their spins equal zero) such that the measurement of photon B allows to express the hypothetical polarization state of photon A (before its measurement) as a function of the angle that fixes polarizer A with respect to polarizer B. The probability that photon A passes through polarizer A is

$$|\langle\Phi^A|U^A\rangle|^2 = (1/2)\cos^2\alpha$$

as it must be.

The disturbance interpretation

Here there are many points of agreement. The principal disagreement concerns Whitaker's argument based on EPR's paper. I have already explained my viewpoint concerning EPR, so it is not necessary to repeat that here. The Stern-Gerlach disturbs the momentum and the angular momentum : if I tilt a Stern-Gerlach, the space quantification takes place in the new direction.

Some theoretical interaction-free measurements are very clever. Do they work experimentally ? Until now, I have had no time to make a detailed analysis of any of them in the quantum optical domain.

Bohr

I believe that a "once and for all illuminator" is someone who gives the light of fact and knowledge, and reveals truth conclusively. In my opinion, that definition does not apply to Niels Bohr. To my teacher,

Bohr was a leader in the battle at the frontiers of knowledge, someone who led a daily struggle to unveil nature's behaviour in the microphysical domain, who hesitated, and who noticed that epistemologically (in the sense of "meaning of knowledge) something must be said. Quantum mechanics was certainly born in discussions with Heisenberg, Dirac, Pauli, Schrödinger (using de Broglie's "matter waves") and many others. Then time passed. In the fifties Bohr was no more a leader but a spectator. In his last years had he given the impression of being a "once and for all illuminator of quantum mechanics" ? I am not sure, but it is difficult for me to believe it.

Einstein

My first contact with Einstein was his 1905 relativity paper [9]. I was struck by the astonishingly far-reaching consequences he could draw from simple real thought experiments with observer, metres, flashes, watches and moving reference systems. In all his early work I always encountered this simplicity that almost anyone can follow with relatively little physical background. His reasoning is not "buried in erudition". It was for me, a young graduate, a great encouragement to see that with next to nothing it was possible to obtain such decisive results. In another of his 1905 papers, "On a Heuristic Point of View concerning the Production and Transformation of Light" [10] he arrived at the heuristic viewpoint that radiation, like matter and electricity, is quantified, and this result is principally based on the physics of resonators and entropy of gases, and references to experimental knowledge : Stokes' rule, Lenard's work on what we would call now "photoemission", etc.

Concerning general relativity, I consider, first, Whitaker's use of the word "degenerate", rather inadequate ; secondly, I use the adjective " speculative" in the sense "1) of, characterised by, or having the nature of, speculation or meditation, conjecture, etc. ; 2) theoretical, non practical" (Webster's).

Everybody agrees that in the last years of his life, Einstein no longer had any influence on the work of physicists. That was due, I believe, not to a mere problem of ageing (as for anybody else) but to a change in his personal philosophical conception of the universe. I believe that this "mutation" is visible in his 1935 EPR paper, where the concept of "physical reality" (= a "thing" exists even if nobody has or can observe it) plays a main role.

I consider philosophy to be a great human achievement. I think that philosophy must be placed in one drawer, physics, in another. I noted that sometimes philosophers do not have sufficient physical knowledge to speak seriously about quantum physics, and have the impression that sometimes physicists do not have sufficient philosophical knowledge to speak seriously about philosophy. It is not easy to master these two quite different kinds of human knowledge. I am ill at ease when the two things are mixed up.

Interpretation of quantum mechanics

From my point of view, the difficulty in giving an interpretation of quantum mechanics lies in the fact that we lake the proper concepts and the corresponding words, to describe the microphysical phenomena, which seem to us to be strange and inconsistent. Our whole collection of concepts and words derives from our real-life experience. It is appropriate for describing classical physics, because classical physics is –let me say- more or less a part of everyday life (distance, motion, mass, gravitation, momentum, etc.). But the behaviour of photons, the wave and particle-like behaviour of electrons and neutrons, etc., cannot be explained unambiguously, because a currently classical description is inevitably made with concepts from this collection. When we try to adapt concepts from this collection to microphysical phenomena, they seem to have contradictory properties. It is evident to me that Nature herself is not –cannot be- contradictory. The contradiction is created in our minds because we have not –at least, not yet- the concepts adequate to microphysical phenomena. We need new concepts, and humans must integrate these new concepts into their whole collection of current concepts (in their "intellectual resources") so that humanity can become accustomed to these (now) strange properties and no longer consider them to be contradictory. Probably much more than one generation will pass before that happens. Remember that, barely a hundred years ago, many physicists (some very great) considered it contradictory to describe physical phenomena with continuous functions, given that matter is composed of atoms and molecules.

Conclusions

To sum up, Whitaker considers EPR and Bell's works as a far-reaching analysis of quantum theory whose consequences affect our

conception of the universe. I could agree if one added a) that this "far reaching analysis" is based on erroneous physical statements and deductions, and b) that it pertains only to a certain philosophical conception of the universe.

Références

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