Comments on A. Whitaker's "Einstein, Bohr and the Quantum Dilemma" [1]

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ABSTRACT. The book is intended for anyone with an interest in the fundamental questions of physics, and in its history and philosophy. The object of this paper is to comment on some of the physical points made in Whitaker's book. I maintain principally that the consequences drawn by Whitaker from EPR and Bell's works, which he considers to be physical facts, must be revised, because Einstein's "thought experiment" is a mere philosophical speculation and Bell's physically erroneous inequality and his hypothetical, fallacious, experimentally unproven "nonlocality" concept, cannot transform EPR's speculations into physical facts. This implies also, that all that is left from EPR's work is their "locality" assumption, which has, in fact, never been supplanted. We must remember that the experiments performed with regard to these questions (for example, Aspect's fine test) have only corroborated the quantum mechanical predictions. I comment also on the "disturbance interpretation" and the "collapse" of the wave function.

RÉSUMÉ. Le livre s'adresse à tous ceux qui s'intéressent aux questions fondamentales de la physique, à son histoire et à sa philosophie. J'affirme ici que les conséquences tirées par Whitaker, (qui considère les travaux d'EPR y de Bell comme étant des faits physiques), doivent être révisées. L'expérience de pensée d'Einstein n'est qu'une spéculation philosophique et la physiquement erronée inégalité de Bell et son hypothétique corolaire la (non expérimentalement confirmée) "nonlocalité", ne peuvent pas transformer les spéculations d'Einstein en faits physiques. Mais cela implique aussi, que du travail d'EPR ne reste que leur hypothèse de "localité" qui, en réalité, n'a jamais été supplantée. Je rappelle que les expériences réalisées (par exemple, les beaux tests d'Aspect) ont exclusivement confirmé les prédictions de la mécanique quantique. Je commente aussi l'interprétation du phénomène de la perturbation par la mesure et le collapsus de la fonction d'onde.

Preface

More than 30 years after Bell's paper [2], which renewed interest in Einstein's [3] so called "thought experiment", his ideas have been accepted by a great majority of physicists, despite the fact that neither EPR's [3] conjectures nor Bell's assumptions are proper physical statements. Philosophers, and physicists interested in philosophy, have recently written books into which they integrate the consequences of Einstein's speculations, renforced by Bell's hypothetical conclusions, in order to provide the layman with a comprehensive description of our physical universe.

Whitaker's book is one of these attempts. In 337 pages and 231 references, it sketches the vast panorama. This is not an easy task because there are some rather extravagant interpretations and/or extrapolations of the mathematically correct formulation of microphysical facts in quantum theory. The greatest names in physics have tried, unsuccessfully, to explain in everyday words and current concepts the microphysical behavior of Nature. Finally, most of them have given up and restricted themselves to the correct use of the mathematical formulation of quantum mechanics and its now most widely accepted, though perhaps not really satisfactory, Copenhagenish interpretation.

This intellectually unsatisfying state of affairs coexists with a pragmatically satisfactory one: quantum mechanics has developed into the physicist's most effective tool and is at the heart of all modern technological progress. Quantum mechanics includes in its record of successes the determination of many fundamental physical properties with unmatched precision, and the correct prediction of the results of all possible experiments, even very sophisticated ones.

I do not believe that all physicists approve Whitaker's *parti pris*, because each of us has his own preconceived **philosophical** views and also his preferences. In particular, quantum mechanists will probably consider it to be mere verbiage. Unfortunately for Whitaker, those of his reasonings and statements that are based on the currently accepted exposition of EPR's and Bell's views, must be reexamined, because in fact, Einstein's speculative conclusions are based on a hypothetical microphysical world and Bell's physically erroneous inequality and his subsequent faulty deduction (nonlocality) cannot help in giving them a physical status. An important consequence of Bell's failure is that EPR's statement ([3], page 779, where they explain what is meant by the absence of an interaction between the two systems under consideration): *"...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"*, is, and always has been, a true physical statement, confirmed by the **correct** interpretation [4] of the experiments performed [5].

The present unsatisfactory state of affairs with respect to the interpretation of quantum physics, allowed Bell's subtle fallacy, concealed in an (apparently) clever line of reasoning, to be accepted by a majority of physicists.

In his extensive analysis of the pros and cons of the various interpretations of the correct quantum mechanical, mathematical formulation of physical facts, Whitaker occasionally reproduces arguments that I believe do not help to understand the underlying physics, but rather increase the confusion. Some colleagues, speaking about these problems, would put it as Whitaker did. This implies that by criticizing some aspects of the book, I actually disagree with a fraction of the physics community.

1. Introduction

I have no particular philosophical competences but I consider that any physicist must have something like a basic philosophical education, i. e., he must know what such words as, 'knowledge', 'comprehension', 'language', really mean. This is very important in quantum physics where the difficulties stem principally from the interpretation of the formulas. The correct and precise wording and understanding of the meaning of concepts plays a paramount role. And here it is important to recall that our microphysical language is "polluted" by centuries of classical, macroscopic physics. Let me quote J. R. Oppenheimer: *"Often the very fact that the words of science are the same as those of common life and tongue can be more misleading than enlightening"*. For the modern physicist, the tentative approach to microphysical facts with the reasoning of classical physics gives rise to misunderstandings and masks the real, microphysical description of Nature's behavior and creates paradoxes. We must not forget that our universe is not a "classical" universe but a quantal one, and that classical physics gives only a coarsely approximate description; and classical formulae, give only rough estimates of the basically atomic constitution of all matter and their discontinous interactions by means of elementary particles.

I recommend a critical reading of this book, mainly because there are two important points that I believe require correction: on the one hand, Whitaker (like many others) considers that EPR's paper and Bell's work contain physical facts, and so, he believes that they are solid ground on which physicists can construct and philosophers can found their reasonings. But in fact, this is not the case: EPR and Bell's works are mere theoretical assumptions not confirmed by any experiment (as we will see later); on the other hand, I consider that the important and subtle problem of the "disturbance interpretation" is worth seeing in a different light. Moreover, the "collapse" of the wave function also deserves a commentary. In the following, I will discuss these points and base my remarks directly on some of Whitaker's statements. Let me begin with the Bell affair.

2. Bell's fallacious reasoning and EPR's "thought experiment"

There are many ways to show that Bell's theoretical speculations (about the problem raised by EPR) do not constitute a problem based on physical facts (as has been demonstrated by various authors with various arguments [6]), and that they are nothing more than working hypotheses and/or assumptions not confirmed by experiments. I think that the simplest of these ways is the following:

1) It is evident, that Bell's inequality is not an expression of physical facts because it is not satisfied by Nature, but is indeed violated by the experiments performed [5] (provocatively, I could say that this inequality has nothing to do with physics proper).

2) Bell conjectured that from this failure to describe Nature's behavior, he could draw a physical conclusion by the following (apparently) very clever reasoning: if the inequality is not satisfied by Nature, this implies that one of the assumptions (premises) I have made to establish my formula, is not in accordance with Nature. He hypothesized that the locality assumption is a good candidate for rejection and so he concluded that microphysics must be nonlocal. Therefore, **according to Bell** (after the violation of his inequality) the nonlocality (and/or nonseparability) concept was born: for him and his followers, the experimental falsehood of Bell's inequality implied the falsehood of locality.

3) Actually, Bell's reasoning is fallacious (in fact, it is a paralogism). He probably thought: if a set of premises (the set of my assumptions) leads to a false conclusion (my inequality transgresses Nature), then one at least of the premises must be false. This reasoning is absolutely correct as a matter of **pure** logic. Yet in Bell's case it has been (and must be) examined in the light of the empirical facts: as the inequality **does not** satisfy Nature, we cannot know if merely **one** of his premises is false (Bell's supposition) **or** if his entire set of premises is inadequate to describe the real **microphysical** situation. Owing to this duality (only one false assumption **or** an inadequate set of assumptions), not noticed by Bell and his followers, Bell's view that only the locality asumption is false, cannot be taken as an established physical fact, as being a logical **compulsory** consequence of his reasoning, and therefore, Bell's conclusion is nothing more than a mere supposition without physical confirmation. In other words, nonlocality has not been confirmed as a physical fact by Aspect's fine measurements (which showed clearly that Nature does not agree with Bell's inequality.) Accordingly, Bell's work, being a theoretical speculation, cannot be invoked in support of EPR: neither of these works is properly based on physical facts.

Unfortunately, the fact that physicists could not agree on any picture to represent the (for our macroscopic mind) mysterious behavior of Nature in the submicroscopic domain, created a long lasting uneasiness in the physics community and so Bell's improperly inferred physically incorrect inequality, and its fallacious corollary, nonlocality, found a favorable ground and have been accepted, rather easily, by many colleagues. Moreover, the relation of Bell's work to the EPR problem has induced some colleagues, seeking for (what they call) a "realistic solution", to think that they can settle the philosophical questions raised by EPR. This bit of wishful thinking blunted the physicists' generally sharp critical analysis of new propositions and so they have not realized that there is a flaw in Bell's argument: Bell's nonlocality -and/or nonseparability- is not physically established. It is a mere assumption.

It follows that from Bell's work we cannot draw physical conclusions. The very important experiments performed [5] have only confirmed the quantum mechanical predictions: this is "Bell's" only (indirect) contribution to physics in this affair. In brief, neither EPR's (1935) nor Bell's (1964) papers brought about any **physical** innovation, but merely stirred up old speculations about a philosophical "reality" and added new (physically) fantastic ideas, such as "nonlocality", to a hypothetical, imaginary universe. This has made much noise, much has been written about it, but, let me say, it has nothing to do with physics proper.

Right from the start, some colleagues [6] sound the alarm, but they were roughly snubbed by Bell's first enthusiastic followers, who despised them because they had not "seen" Bell's "inspired" work. Unfortunately, apart from these early defenders of physics proper, the majority of physicists has been enticed by Bell's apparently clever reasoning and, retrospectively, by Einstein's fame. Wishful thinking about a philosophical reality contributed also to this persisting error. Bell's failure to describe Nature's behavior with his inequality, and his pseudo logical reasoning rejecting one of his assumptions (locality), could not rescue EPR's speculations which are physically baseless, except to the extent that their "locality statement" ([3], page 779) is confirmed by the **correct** interpretation [4] of the experiments performed. We must always keep in mind that there are two mistakes in Bell's work: his inequality does not conform to Nature's demand, and his reasoning, based on the violation of the inequality, is fallacious.

I think that the EPR paper is a typical example of a clever, purely intellectual work not based on physical facts (that is, not grounded on results confirmed by measurements), but only on theoretical speculations inspired by conjectures founded on a hypothetical behavior of Nature. After Bohr's [7] answer to EPR and before Bell's paper, there had been broad agreement that the EPR paper is a purely speculative (metaphysical) work. Bohr has correctly (but perhaps not sufficiently clearly) pointed out that EPR's working hypotheses are not in accordance with the physical ground on which quantum mechanics is constructed. And today (1997), we can add 1) that the ground on which quantum mechanics is constructed is physics pure and simple, since the quantum mechanics mathematical formulation correctly describes all physically possible situations and so, it accounts for Nature's micro- and macrophysical behavior; and 2) that Bell's inequality (more or less based on EPR's assumptions) does not describe physical facts (the experiments performed [5] have "violated" the inequality and Bell's inference based on this violation is wrong). It is clear (and unfortunate for the nostaligic followers of EPR), that Bell's physically baseless paper could not transform EPR's hypothetical conjectures into a physically correct description. This is why the famous EPR speculations about 'reality' and the 'incompleteness' of quantum mechanics, were and remain mere speculations. The experiments performed after 1964 [5] have only confirmed the quantum mechanical predictions, and the existence of correlations does not imply that the result of one of the measurements (e. g., the left) **determines** the result of the other far away (the right). The results are correlated because the photons stem from one and the same cascade and they maintain their zero global angular momentum, (and hence, their polarization states) from the source until the polarizers [1] [8]. Therefore, the correct interpretation of the experiments performed restores locality. In fact, locality has never been refuted by Bell's fallacious work.

3. About the disturbance interpretation

Much has been written about this subject, and I want to contribute some arguments that are distinct from Whitaker's.

Whitaker writes (page 168): "*Unfortunately for Bohr, in 1935 Einstein, in collaboration with Boris Podolsky and Nathan Rosen, produced the famous EPR paper, which effectively demolished the disturbance interpretation."* I disagree with this statement because (as we have seen) EPR's paper, with its so called "thought experiment", concerns only theoretical speculations based on a hypothetical behavior of Nature: their conclusions are mere philosophical conjectures and we must remember (with L. de Broglie) that physics is an experimental science. So, the EPR paper by itself could not demolish the disturbance interpretation which is (and must be) based on physical facts.

Page 174, Whitaker writes : "*Bohr himself would not have allowed all the possibilities I have included*" (to try to fix the separation of an electron to be measured from the measuring system). *" He would certainly have refused to consider any macroscopic object as part of the observed system, because, if he did so, he would have to assign it a wave-function. Nevertheless there is still sufficient ambiguity to justify his remark [10] that 'this crucial point ...implies the impossibility of any sharp separation between the behaviuor of atomic objects and the interaction with the measuring instruments which serve to define the conditions under which the phenomena appears.' "*

"This ambiguity implies the collapse of the disturbance interpretation."

Here Whitaker puts forward another argument to refute the disturbance interpretation. I say that we must consider the disturbance caused (or influence exerted) by a measurement, to be a physical fact. We have seen that EPR have not demolished it, and what is still worse for Whitaker's arguments, they could not demolish it with their merely theoretical speculations.

Now, let us look nearer at the 'disturbance' problem. Whitaker writes (page 172): *"What I shall call the 'disturbance interpretation' implies that an atomic particle which is having one of its properties measured exists in a perfectly well-defined state ; this state cannot , however, be determined exactly because our apparatus is so much larger than the atomic system that it* **disturbs** *it, changing its state unpredictably to another well-defined one."* Whitaker's statement is at variance with the extremely cautious view of the quantum physicist, who does not say that the particle is in a perfectly well-defined state (because he has no data about this state). In general, it is not possible to use the same words to define the state before and after a measurement: I say that after the measurement the particle is in a perfectly defined state, a state that I know, but before the measurement, generally, I cannot speak of a perfectly defined state. The particle is possibly in a state but I cannot say a 'perfectly defined' one because I have no data concerning this state.

Let us (naively) take a closer look at the subtle measuring procedure. To measure something we must establish a "link" between this something and the measuring device. It is clear that we want the link to be as small as possible. But, on the one hand, the link cannot be nil because it would then be possible to measure a thing in the total absence of the thing; and on the other hand, our world being a quantum world, we cannot reduce the link (the interaction) to an arbitrarely low level: we know that there is a minimum action -given by Planck's h- for all interactions with physical consequences. This means that the interaction must have at least the value h to modify something physically in the measuring device (this modification is then amplified with the help of an external energy source and displayed on the viewing screen or something analogous). It follows that it is not possible to "act" on something to obtain a physical result (the measurement), without disturbing that thing, and a measurement is an interaction. This implies that the object to be measured and the measuring device must interchange at least one quantum of action if a macroscopic display is finally (after amplification) to be obtained.

The "disturbance interpretation" must express in words the behavior of Nature. Evidently, our measuring device is a part of Nature and it behaves according to Nature's principles.

In a Stern-Gerlach, the link is the action of the

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[1] The photons are circularly left, and circularly right polarized and we know [9] that a circularly polarized photon does not look the same in all set of axes. Moreover, by their "birth" in the same cascade they are "consistent" in polarization, i. e.,they have both one and the same "initial phase", a feature they maintain until something acts on them. This characteristic determines their identical behavior when they encounter parallel linear polarizers. If one of the polarizers is turned by an angle θ with respect to the other, this is equivalent to an advance (or retardation) of the value of the property I called "initial phase" of one of the cicularly polarized photons with respect to the other, and the number of correlations reduces by $cos^2\theta$. Bell's nonseparability concept, and /or his instananeous causal influence at distance, are consequences of his erroneous physical assumptions and wrong reasonings.

inhomogenous magnetic field on the magnetic moment of the electron, and the final result is a latent image in a photographic emulsion. If an electron enters a Stern-Gerlach and comes out as 'spin up' in the direction defined by the apparatus, in general I do not know in which state the electron was before entering the apparatus. But we can imagine situations where we know the state before measuring it. For example, if after selecting the spin-up electrons from a beam emerging from a vertical Stern-Gerlach we measure these spins with a second slightly tilted Stern-Gerlach, we find that all emerging electrons have their spin states tilted (many of them "pointing" up in the new direction, and only a few, down in the new direction). This shows clearly that the second measurement has disturbed the electrons being measured. In this case the electrons were in a well-defined state before the measurement and are in one of the two possible states after measurement, yet for each individual electron, I cannot say in which state it will be, but I can give probabilities. (I recall that in many situations we must have recourse to the (macroscopically not enlightening, but mathematically very effective) quantum mechanical concept of "probability amplitude"). A measurement being an ordinary interaction, an interaction like any other in Nature, this behavior authorizes us to speak of a disturbance by measurement and also of a "disturbance interpretation".

The difficulty with the 'disturbance interpretation' has a historical origin. Classical physics claimed that measuring something does not affect what is measured. Yet, measuring is an interaction. This interaction produces -after amplification- a macroscopic change in the measuring apparatus: a pointer is shifted. If after the thing is removed the pointer comes back to its primitive position, we say that we have made a measurement.

The classical physicist says that the object we have measured has not changed. In fact it has not changed macroscopically. We must recall that, e. g., what we call classically the weight of a macroscopic object is only an approximate measure to within millions and millions of atoms and electrons. The object we want to weigh must be placed on the scales with our fingers or with pliers. This manipulation pulls away, and/or sticks on, some atoms or electrons. In general, the object is surrounded by the atmosphere, that means, bombarded by thousands and thousands of "air" molecules that interact with the object and can pull away some of its molecules and/or electrons, etc. In addition, we must see what we are doing, this implies also a bombardement with photons, which are partially absorbed (and warm up the object, increasing the chaotic agitation we call heat) and partially sent back to our eyes. This also alters the object discontinuosly. So, it is totally impossible to weigh an object "exactly" within one atom or electron. And the object's apparently arbitrary "variation" in weight by adsorption or loss of atoms and/or electrons is neither continuous nor "smooth", for the change is quantal: each time, a full atom or electron more, or less. We see that even macroscopic objects are disturbed when we handle them. I repeat: classical physics is an approximate description that masks Nature's microphysical behavior.

This implies also that we cannot rely on the reasonings of classical physics when we try to understand microphysics.

4. On Einstein's "going into effective scientific exile".

I believe that it is easy to "justify" this statement, but first let me comment on Whitaker's remark, page 163. I read:" *...if Bohr was forced to change his views from time to time in response to the arguments of others, it becomes much more difficult to present him as the once-and-for-all illuminator of quantum theory."* I do not believe that any serious epistemologist who follows Bohr's struggle to unveil Nature's behavior would consider him a "once-and-for-all illuminator of quantum theory".

Along the same lines, I would add that if we want to understand Einstein, we must also say something about his historical change of convictions. Between 1905 and 1935 he radically modified his way of looking at the world . After being an empiricist (positivist) he became a metaphysician of the Platonic type [11]. His early works (until, say 1925-30) are characterized by an intimate contact with simple physical facts. Yet in 1935, his EPR paper is based on speculative ideas concerning a hypothetical behavior of Nature, where *a priori* concepts concerning "physical reality" have replaced empirical facts. This has nothing to do with aging:in 1925 he was only forty-six. It has to do rather with a radical change in his

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[2] It is not easy to determine exactly the date of his "mutation". It is correct to say that his General Relativity (1915) is an essentially speculative achievement. But nonetheless, after this exploit he contributed still fundamental papers based on simple physical principles, as those concerning his spontaneous and induced emission coefficient (1916-1917) and a new derivation of his energy fluctuation formula for radiation. He recognized also that light quanta carry momentum $h\nu/c$ (Nadelstrahlung). The fact that the time of an emission and the direction of the radiation could not be derived from physical principles but were due to chance, shocked his belief in classical causality. His 1924-1925 (Bose-)Einstein statistics treats radiation simply as a photon gas. He reacted favorably to de Broglie's proposition $p = h/\lambda$. With his important thought experiments and his discussions he participated indirectly in the new orientation in physics championed by Heisenberg, Born,Bohr, Dirac, this notwithstanding his distrust of the way things developed. The rather difficult birth and subsequent slow experimental confirmation of quantum mechanics, compelled him to admit that the new theory is probably a correct description of physical facts without inner contradictions, but he remained

conception of the universe[2]. To understand Einstein, we must always remember that there are two completely different Einstein: roughly pre-1925/30 and post-1925/30. I believe that this is why Whitaker writes page 324: *"The fact that his* (Eintein's) *own work in the years after 1935 was not fruitful, was scarcely likely to encourage others to take much interest in his challenge to Bohr";* and on the back cover we can read "*The debate between Bohr and Einstein, which raged in the 1920s and 1930s, but which is still highly relevant today, involved the two greatest physicists of the twentieth century, and played a large part in Einstein, perhaps the most famous physicist ever, going into scientific exile.*".

5. On the physicist's need of an interpretation

After explaining Bohr's position and before introducing Einstein and Bell's work in detail, Whitaker writes (p. 165):

"*I close this section by mentioning that I suspect many people have been put off Bohr's views because their motivation is so different from his. Today most physicists come to accept readily the mathematical approach to quantum theory, and are unaware or even dismissive of any conceptual worries about working with it. What they require from an interpretation is some sort of* **picture** *of what is going on, to give at least a partial explanation of the very perplexing aspects of the theory, or, it might be said, to change mathematics in physics. To these people, the Copenhagen interpretation seems little more than unenlightening, pedantic, and frankly uninteresting logic-chopping. In contrast, some of the later interpretations discussed in the next few chapters meet such people's demands more closely.*"

If the Copenhagenish interpretation actually does not fulfill some people's need for a clear description of the mathematical formulation, unfortunately, insofar as Whitaker's "next chapters" are based on EPR and Bell's physically baseless work, "such people's demands" cannot be met in his book.

Let me add that an interpretation is a sort of picture of what is going on. As a down-to-earth physicist I need pictures, but in microphysics it is not always possible to provide such pictures.For example, if we do not enter into our "picture" the purely quantum mechanical concept "probability amplitude", we cannot describe all experiments, although this concept is a macroscopically incomprehensible one. If we consider Young's double-slit experiment, we cannot "picture" the photon's behavior without the probability amplitude concept. This implies that when we name a new (macroscopically unknown and incomprehensible) thing such as the photon, we must always keep in mind their pragmatic definition. Pragmatic definition means (in our case) a synthesis of all physical (even macroscopically contradictory) results concerning the photon. The photon is nearly one hundred years old but we cannot draw it. Since it has no macroscopic equivalent, we cannot imagine a thing with its (macroscopically contradictory) properties, and can only give a precise mathematical procedure for obtaining the probability of the correct experimental result.

In general, if the pictures are constructed on the results of measurements, i.e., on physical facts, they will be accepted by all physicists without discussion. (Physics being an experimental science, experiments are the physicists' "touchstone").This general agreement incorporates the picture (i.e., the interpretation) into the body of knowledge we call Physics. Yet if a picture is based on conjectures not confirmed by measurements, i.e., on prejudices or philosophical speculations, no consensus is reached because the picture goes beyond the line which separates physics from theoretical speculations called metaphysics.

Let me add some words about the expression "thought experiment". We use this expression to indicate that we have not performed the experiment but we think that we can perform it, and that it is not a mere philosophical speculation. In this sense, EPR do not propose a "thought experiment", but a hypothetical experiment or better a philosophical speculation. But the history of humanity has taught us that, sometimes, philosophical speculations about the physical world are later recognized as physical facts. An example of this is the atomic hypothesis: this assumption had been a philosophical speculation for centuries, until Rutherford's experiments showed that ordinary matter is composed of atoms and/or molecules. So, philosophical speculations can be of great interest for physicists: they can give them ideas for future experiments. But I consider that philosophical speculations must not be treated by physicists as if they were already physical facts, at least not until new experiments have shown that they are henceforth to be recognized as physical facts. Unlike B. d'Espagnat [12], I think that it is essential for physics that these two quite different ways of looking at the world are not confused.

6. Concerning the "collapse of the wave function"

Whitaker gives an exhaustive exposition of the different interpretations concerning the wave function and

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convinced that "behind" the correct prediction of experimental results there must be an (experimentally inaccessible ?) "objective reality" What for the architects of the new quantum mechanics were the expression of "laws of nature" was for him mere incompleteness of the theory and simple provisional accounts. This situation took a definite shape with the EPR paper and thereafter, one of the leading personalities of modern physics, stayed on the sideline.

its "collapse", also called the "reduction postulate".

Using various arguments, some physicists (including myself) state that there is no "collapse" in Nature [13]. One of these arguments is that the wave function is constructed on the physicist's knowledge of the physical system, and each new item of information renders the primitive wave function obsolete. This is also required by the probability interpretation of the squared modulus of the wave function: each new datum renders the primitive probability function "null and void" [14]. The creators of the designation "projection postulate" knew that, because, to postulate means, to assume without proof to be true, real or necessary (Webster's). Nevertheless, because of a non-rational historical development, the great majority of physicists treats the "collapse" as something close to a physical fact. That gives rise to innumerable gratuitous difficulties and paradoxes.

The representation of the wave function as containing the physicists' knowledge of the physical system, is also advocated, e. g., by R. Peierls (see p. 302, and [15]). Here I want to complete Whitaker's presentation of the subject (p. 301). He writes: "*The first is that the problem of quantum theory may be explained by the idea of* **knowledge**... *It is a very natural idea for someone getting to grips with the difficulties of quantum theory to decide that the solution is as follows. I may not* **know** *the value of, say,the z-component of spin for an electron, just as I may toss a coin and not* **know** *whether it has landed heads or tails, just because I have not looked! I may then look at the coin, and discover that it is, in fact, heads, and was so, of course, before I looked. Similarly I may measure the z-component for the electron, and obtain the value of +h/2 (rather than -h/2), and I may again deduce that it must have been so before I measured it! That is all, from this point of view, that the collapse of the wave function is about. I gain* **knowledge** *of something that existed all time, and I do not need to disturb the system to do so. Thus the wave function denotes* **knowledge** *of the system.*"

I do not agree with this description: the problem must be better specified. First, I repeat that macroscopic or classical reasoning is not a good guide for microphysics. Secondly, in general, measurements modify the physical universe: the physical universe is not the same before and after the measurement. Therefore, after the coin has been tossed, the universe is not the same as before it was tossed, even if I have not looked at the result. This implies that there are two different aspects in all measurements: the physical operations which modify the physical universe, and the fact that a human notes it, reads the display and so can introduce the new result into the body of knowledge we call Physics, and, should the occasion arrise, to use it to write a new wave function.

For the physicist, it is evident that the measurement is truly finished only when he has read the "display", though the fact of reading the result does not alter the physical universe (except for the content of his brain): first, because he must be sure that his measuring device has functioned correctly (for example, having tossed the coin he must be sure that it has not fallen on its edge), and secondly, because he works to increase physical knowledge.

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