On interpretations of quantum mechanics, no-hidden-variables "theorems", and physics

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ABSTRACT. Quantum mechanics is a supreme achievement of physical science but the attempts at understanding the physics underlying its mathematically efficient working postulates often led to large-scale irrationalism. For a long period of time, the dominant leader along such lines of thought was the Copenhagen (Conventional) Interpretation, which is systematically examined in Sec. 1. De Broglie's rational interpretation is considered in Sec. 2 and a number of its conceptual advantages are pointed out. The consistent-histories approach, considered in Sec. 3, represents a rational interpretation of quantum mechanics, resting on a minimal number of hypotheses, but it offers non answers to basic problems as the nature of wave-particle duality. No-hidden-variable arguments endeavoring to substantiate strictly the Copenhagen idea about the irreducibly statistical nature of micro-phenomena represent a curious feature of 20th century physics. The reasons of their ineffective character are noted in Sec. 4, where certain well known no-HVs arguments are examined in sufficient detail.

It would be a half-thuth to just state that Professor Georges Lochak is an eminent scientist whose long research activity ranges over various branches and topics of physics. Indeed, Georges is in a sense a missionary in science as well : being de Broglie's closest collaborator in the past, he does his best nowadays for the preservation and popularization of de Broglie's written heritage and ideas (an occupation that could only incompletely be illustrated, say, with references [1-7], dealing with - or making essential use of - de Broglie's ideology or simply acquainting the unprejudiced reader with facts of de Broglie's biography). At the same time, in his capacity of Director of Fondation Louis de Broglie he is striving, together with his colleagues of that foundation, to keep alive the de Brogliean spirit of open and fair discussion of physical problems and ideas, in that number ideas that are at complete variance with those shared by the patron of the foundation. The marked contrast between such a practice and that of other groups and schools in the expiring 20th century has no doubt been felt by many of those who have had something to do with the interpretation of quantum mechanics (QM).

The abovesaid makes me think that the best thing I could do for the present collection of papers in honour of Professor Lochak is writting a paper on certain well known interpretations of QM, de Broglie's one in that number, pointing to the conceptual advantages of the latter with regard to diverse questions, and to the futility of the efforts to contrive the ideal no-hidden-variables theorem which could convince the physics community that a more detailed description of micro-phenomena than the one given by QM is impossible. At first sight this task does not appear to be interesting enough. Indeed, there exist e.g. a number of books written by de Broglie himself on his interpretation of QM (say refs. 8, 9) or first-hand concise expositions of the same theme (say refs. 10, 1, 4). There also exists Jammer's remarkable book [11] on the development of QM and the origin and content of its Copenhagen (Conventional) Interpretation (CI) and critical papers (as the excellent review [12]) of a large number of interpretations of QM. And in the end, a number of "no-go-theorems" has been criticized from different viewpoints in the literature (see for example de Broglie's critique [9] of von Neumann's argument (ref. 13, ch. IV) and refs. 14, 15).

It is not my intention, however, to propound here what is already well known or write a review paper on interpretations of QM. What I wish to discuss is a personal viewpoint on questions as why the split between people as Einstein and (later on) de Broglie on the one side and Bohr and Heisenberg on the other side was a split between rationalism and irrationalism (in the apt words of Popper [16]), in what sense the influence of the CI is still felt (though strongly enfeebled) nowadays, and why one could expect that no-hidden-variables "theorems" - de facto inspired by the CI - will reap an eventual success of the same kind as e.g. that of the numerous proposals for perpetuum mobile in the past. The present consideration will be based on simple arguments. The particular interpretations and viewpoints on QM that will be examined are the CI, de Broglie's interpretation, and the consistent histories approach.

1. The Copenhagen Interpretation

For a long period of time this interpretation was identified with the actual content of QM. It is now clear enough that the theoretical, quantitative content of QM is given by its axiomatics whose standard present-day form is articulated e.g. in refs. [17] (Sec. 1.1.) and [18] (this axiomatics incorporates, in particular, Born's statistical viewpoint on the nature of the QM states of motion and its generalization by Pauli and Dirac), whereas the CI may be regarded as consisting mainly of Bohr's ideology about micro-physics, and in a lesser degree also of that of Heisenberg. (For that reason it was sometimes called the Copenhagen-Göttingen interpretation.) Bohr's viewpoint, as put forward in numerous writings (see e.g. [19], [20]) can be summarized as follows.

B1. The atomic postulate : Both the classically inexplicable stability of the atoms and the character of the effects on the atomic scale are determined by the indivisibility of Planck's quantum of action ($h \approx 6,6.10^{-27}$ erg.sec). Due to it the electromagnetic excitation or radiation of the atoms is an instantaneous act [19a]. The same applies to any atomic process [19b]. (Synonyms in [19b] and later papers : discrete, elementary, individual, atomic, discontinuous, indecomposable process).

B2. On the microscopic (atomic) scale the values of the physical magnitudes (and any physical information in general) are obtained via measurements in which the conduct of the micro-object cannot be separated from its interaction with the inevitably (and genuinely) classical measuring apparatus determining the conditions in which a given phenomenon takes place ; the absence of independent reality of both the phenomenon and the means of observation is a consequence of B1 which presupposes a nonneglibigble magnitude of the interaction of atomic phenomena with the means of observation.

B3. Space-time measurements exert an effect on energy-momentum variables that is uncontrollable in principle due to the uncontrollable exchange (because of B1) of energy and momentum in the process, so these measurements presuppose abandonment of strict applicability of $(\vec{p}, E)$-conservation laws ; on the other hand, the strict application of the $(\vec{p}, E)$-conservation laws presupposes an essential abandonment of space-time coordination (e.g. [19g]).

B4. The complementarity principle (ref. [19b], etc.). The description of quantum effects in (inevitably) classical terms has two complementary (and incompatible) aspects : it should be done either in terms of positions and moments of time or in terms of causal magnitudes : momenta and energies. The corresponding measuring apparata are incompatible [19f], which fact rules out any contradiction of logic between the complementary ways of description.

B5. The impossibility to apply simultaneously space-time characteristics $(\vec{r}, t)$ and energy-momentum (causal) characteristics $(\vec{p}, E)$ to the description of the quantum entity introduces essential indefiniteness in the description of this object with the aid of our macroscopic notions and this indefiniteness is given by Heisenberg's uncertainty relations.

B6. Quantum effects must have a statistical description.

Before discussing the statements (whose derivation from Bohr's writings is not an altogether trivial problem since he never propounded his ideology in the form of a clearly articulated set of postulates) I shall quote Heisenberg's viewpoint in a form admitting logical coherence and a possibility for comparison with that of Bohr.

H1. "The presumable objective reality of elementary particles turned out too crude a simplification of the actual state of things and it must be superseded by more abstract conceptions". [21a]. "Not the objective events but the probabilities of certain events to take place can be established by mathematical formulas. Already not the factual phenomena themselves but the possibilities for phenomena, 'Potentia', if we are to employ a conception of Aristotelian philosophy, are subjected to strict laws of nature". [21b].

H2. "... the laws of nature that we formulate mathematically in the quantum theory apply no longer to the elementary particles but to our knowledge about them ... As a result of this the conception about the objective reality of elementary particles vanishes in a strange fashion ; it does not vanish however in the haze of a certain new understanding of reality or of a conception about it that is still not understood but does so in the transparent clarity of the mathematics which describes our knowledge about the behaviour of the elementary particles and not their behaviour itself". [21a]. "The mathematical formulas no longer depict nature but our knowledge about nature". [21a].

(The knowledge in question is the one acquired by observations that essentially perturb particle behaviour and thus bring forward the notion of Potentia as well - my note).

The set B1-B6 evidently does not consist of wholly independent statements ; each one of them, though, has a part that does not overlap with the content of the remaining statements. An analogous assertion applies to H1 and H2.

B1 represents a slightly extended version of the one formulated by Bohr in 1927 and published in 1928 [19b]. We have added to the version in [19b] Bohr(s own viewpoint of the stability of the atom (in particular of its energy levels) and have lain an emphasis on the fact that the jump-like micro-processes were regarded by Bohr as actually *instantaneous*: a term employed in [19a,b] and evaded in later papers. This term was, however, unhesitatingly used again by von Neumann [13], one of whose basic goals in writing his well known monograph was to advance a strict form of Bohr's view on measurement (process 1. in [13], ch. V).

The formulation (though not the content) of points B2 to B5 does not appear to need special comments. The atomic postulate B1 obviously underlies all of them, as well as B6. Indeed, B6 becomes explicable on the basis of B1 by a comparison with classical mechanics in which motion is described via differential ("causal") equations that give account of continuous variations with time of both spatial coordinates and momentum-energy ("causal") magnitudes of individual objects. Therefore, the assumption that space-time description (registered by measurements) induces acausal jumps of $(\vec{p}, E)$ magnitudes disrupts the deterministic description of motion in apparent compliance with the complementarity principle and characterizes QM as a theory that needs an irreducibly statistical description.

A careful consideration of the content of B1-B6, H1, and H2 raises a number of questions. Why should microscopic (atomic) effects be instantaneous ? What model of the micro-particle does the CI propose ? How is a unique measurement datum obtained ? Etc.

The instantaneity assertion is really central since the entire CI hinges on it. Indeed, assume that micro-events as momentum-energy exchange that happen, say, in $\vec{x}$-measurements are not genuinely instantaneous but only very fast from the viewpoint of macroscopic physics. The, however, we would not have acausal jumps and all that but would have to do with particular processes, the description of whose continuous (though fast) evolution in time would represent a fundamental new physical problem in which concepts as uncontrollability, complementarity and so on would be just irrelevant, as would be the entire CI itself.

The idea about the existence of instantaneous jumps, however, poses a problem. Namely, what we have now is a postulate which needs an experimental proof like any other statement in physics. But how could one possibly prove experimentally the genuine instantaneity of micro-events ? A direct way to do it is not seen. At the same time, natural physical notions are just incompatible with instantaneity of e.g. emission and absorption of energy quanta.

Indeed, it can be assumed that
(i) a physical entity carrying a finite charge, mass, energy, etc, cannot have a zero size in position space (in other words, finite physical characteristics cannot be concentrated at a mathematical point), and
(ii) matter in any form (field or particle) cannot move infinitely fast.

It is obvious then that under such assumptions say electrons or protons could not instantaneously emit or absorb electromagnetic quanta (the latter possessing finite energies).

But Bohr was convinced in the existence of genuine jumps when atomic electrons pass from one stationary state to another even before he accepted the existence of light quanta (an idea which he firmly rejected until 1925, by which time he regarded spreading of light as a continuous wave process). Since $(\vec{p}, E)$-conservation is known to exist in a physical system when the latter is described with the aid of causal (differential) evolution equations, a natural consequence of acausal electronic jumps would be momentum-energy nonconservation in the system, which effect in itself would also represent an indirect proof of the physically sound character of the instantaneity idea. So in 1924 Bohr published a paper co-authored by Kramers and Slater [22] in which was predicted, in particular, $(\vec{p}, E)$-nonconservation in a system of atoms undergoing electromagnetic interactions (cf. also ref [11] for details). This prediction was almost immediately disproved by experiment (see [11] and references therein). But the hope to find an experimental disproof of the conservation laws for the causal $(\vec{p}, E)$ magnitudes clearly was not abandoned by Bohr even after he accepted the existence of electromagnetic quanta and wrote his foundational paper [19b] on the CI, so certain characteristics of the continuous spectrum of β particles induced him to predict once again energy nonconservation in a 1932 paper (ref. [19e]), this time in nuclear phenomena, and with the same success. (This prediction provoked Pauli to advance the idea about the existence of the neutrino, which was experimentally discovered much later). Why did Bohr nevertheless continue to reiterate throughout his life his assertions about discreteness, uncontrollability, etc in a practically unchanged form is not very clear, at least to me. Most probably, he somehow sustained the idea that $(\vec{x}, t)$-measurements - uncontrollably affecting $(\vec{p}, E)$ magnitudes - are totally unfit for any definition of these magnitudes and their possible nonconservation, whereas $(\vec{p}, E)$-measurements not only define experimentally these magnitudes but always display the existence of conservation laws for them as well. (This is certainly guesswork but we are offered no other possibility : cf. e.g. Bohr's 1936 note [19g], specially dedicated to conservation laws in QM, in which things are "elucidated" by asserting B3 - which can only raise doubts about the existence of $(\vec{p}, E)$ conservation - as an explanation why experiment has not given any evidence of $(\vec{p}, E)$ nonconservation).

At first sight Heisenberg's viewpoint H1, H2 makes it possible to circumvent the above problem generated by Bohr's outlook. Indeed, Heisenberg talks just about *serious* (rather than uncontrollable) perturbations by observations on measured effects [21a]. Besides, he talks in H1 and H2 about *objective* events and *factual* phenomena, and also, essentially, about nature as it is and nature as we see it, the point evidently being that what we see in the micro-world is so strongly deformed by the crudeness of our macroscopic perceptions and observations that we cannot say how things are in reality. So he sees no a priori reasons to doubt the objective existence of physical processes even in the micro-world (and - as a logical consequence - the factual existence of momentum-energy conservation) but states that what is accessible for us is just their Potentia which can be expressed in a mathematically clear probabilistic form. Therefore, what we see indeed is an irreducibly fortuitous nature and we have to get used to the fact that the strict laws can contrive about it can only be probabilistic.

A similar viewpoint on nature as it is and nature as we see it is shared by Bohr too. ("It is wrong to think that the task of physics is to find out how nature is. Physics concerns what we can say about nature". ; see ref. [15], p. 45, and the original reference therein for this citation). There is a certain problem with this attitude. Namely, it is a variant of agnosticism : objectively existing micro-processes are de facto acknowledged but proclaimed inaccessible to our understanding.

Certainly, no sensible person could insist that physical theory could be a photocopy of nature. However, what we have here is just an attempt to check conceptual developments at the level of macroscopic apparata. I regard this attempt as the actual generator of the outburst of no-HVs "theorems" that endeavour to perpetuate the CI.

It is only a postulate of Bohr - taken up by Heisenberg - that what is inaccessible to the macroscopic means of observation must be inaccessible to our understanding as well and hence that complementarity of observations could entail complementarity of understanding too. Indeed, this is only stated, not proved. (Who could possibly prove it ?) What is of some interest is how did the idea that presumable peculiarities of our way of thinking prevent overall understanding occur to Bohr. The answer may be found in Jammer's book [11]. Bohr was strongly impressed in this youth by the teachings of the Danish philosophers S. Kierkegaard (1813-1855) and is follower and Bohr's contemporary H. Høffding whose lectures Bohr attended. The doctrine in question states, in particular, that one is never an impartial observer in life but a participator too, that the distinction between subjective and objective is always arbitrary, and that progress in life can be achieved only by sudden decisions or "jumps". A jump is not a phenomenon and cannot be described, so causality cannot be described as well ([11], ch. 4.2). Bohr was also strongly influenced by the psychological works of the American philosopher W. James, from whom he borrowed the very term "complementarity". (According to James, human conscience can be split into several complementary parts which distribute the objects of knowledge between themselves (ref. [11], ch. 7.2). And in the end, Bohr had certain idea of his own about the limitations of our means of expressing ourselves which made the concept of complementarity natural for him (see Heisenberg's recollections in ref. [23] and e.g. the concluding part of [19b] or the more detailed consideration in [19c], where one can also find parallels between Bohr's view on QM and psychic phenomena).

The abovesaid makes it clear that the alleged obstructions in our reasoning, words, and knowledge about nature was underlain by a certain metaphysics of which physics had to be only a particular illustration. (complementarity features were noted by Bohr not only in physics and psychology, but in biology, art, human societies and human cultures too [19m]. It is a pity, though, that Bohr was not acquainted with (or just did not like if acquainted) the philosophy of Heraclitus "the Obscure" who taught 2500 years ago that each couple of opposites as day and night, war and peace, young and old, ugly and beautiful, good and bad, etc, represents a unity whose nature "is fond of concealing itself" and hence difficult - through not impossible - to attain ; in order to attain it, however, one must have a psychological inclination for knowledge...

(By the way, the power of thought of the Obscure is also evidenced, say, by his stupendous definition of God as the unity of all opposites).

Returning to Bohr's two-sided complementarity of $(\vec{x}, t)$ and $(\vec{p}, E)$ measurements, we have to note that, in spite of his belief that the difficulties of language and the limitations of all our means of expressing ourselves have found their complete expression in atomic theory [23], there is not seen to exist anything of the sort of two-sided complementarity in the mathematical apparatus of present-day QM. Indeed, there exist various (in fact infinitely many) Hermitian operators in QM satisfying various uncertainty relations and possessing complete orthonormal sets of eigenfunctions. In a historic perspective [19b] the two-sided complementarity thesis was induced by the wave-particle dualism but the said thesis offers no physical picture of QM particles and waves. Because of its rejection of any possibility for humans to coherently comprehend objective micro-reality (B4, B5), the only thing the CI could offer as a picture of the QM entities was Bohr's definition of them as "unsharply defined individuals within finite space-time limits" (cited in ref. 5 and ref. 8, p. 75-76).

Let us see then what an explanation could the CI give for the wave-like behaviour of such an object in the well know two-slit experiment. Let the interference picture on the screen behind the slits be created by the passage of electrons through the slits. There exists a general agreement on the idea that the interference is due to the fact that each electron somehow feels the simultaneous existence of both slits (which would also apply to a classical wave in a similar situation). But how exactly does an electron - a charged particle - feel both slits in the process of its passage through them ? And how does it pass through them is reality : simultaneously, by dividing its charge into two parts that reunite again behind the slits, or the charge passes through one of the slits and "something else" through the other ? The CI declines any physical answer to this question. What Heisenberg could say would sound as "the very formulation of this question is affected by our crude notions about reality ; if you would ask about the potentialities of the electrons in this experiment, then clear mathematics would give you a precise answer of a statistical kind".

It is also well know that QM admits wave function ψ of an individual particle (say electron) that can be nonzero at a given instant t_0 in several nonintersecting volumes in free space, say two, i.e. $\psi(t_0) \neq 0$ only in volume V_1 and V_2, $V_1 \cap V_2 = \emptyset$. Once again, we could ask in what sense is the electron simultaneously present in V_1 and V_2 at moment t_0 ? Is its charge somehow divided between V_1 and V_2 and if yes, then why do we always observe only entire charge quanta in experiment ? And if the charge is in reality (in the absence of position measurement) situated in only one of these volumes, then what is there in the other that will cause the appearance in ψ of an interference picture at certain moments $t > t_0$ in the process of free evolution of the two “subpackets” ψ_1 and ψ_2 of ψ ? The answer would no doubt be similar to the one above, e.g. ”the electron is potentially present in both V_1 and V_2 at $t = t_0$ in a way that is somehow consistent with the fact that charge measurement at t_0 will always display one and the same indivisible charge magnitude either in V_1 or in V_2 ; the factual nature of the said consistency and the subsequent interference effects is incomprehensible for us due to the limitations of our understanding, the overall wave function $\psi(t) = \psi_1(t)+\psi_2(t), t \geq t_0$, giving all our possible knowledge consisting of certain probabilities exhibiting interference effects at $t > t_0$.” (A consideration of analogous problems can also be found in ref. [1]).

Explanations of this kind do not tell us anything about the nature of wave-particle duality. What they aim at is the perpetuation of the CI which appears to be connected with the very essence of human knowledge. What kind of understanding the physics underlying ψ do we achieve from the assertion that ψ describes our knowledge ? It is clear from the above that we are simply urged not to ask questions as “is the wave-like character of ψ an expression of a wave-like character of our knowledge or of a concrete physical cause ?”, etc, but to turn instead to the statistical recipes related to a given ψ and extract from there the particular probabilities and averages corresponding to ψ’s Potentia. The situation would change in a “jump-like” fashion if one would use “information” instead of “knowledge” and reformulate the said assertion accordingly, namely :

The wave function ψ contains information of a statistical nature about the physical properties of the quantum system.

But the CI would not offer or brook such a statement if it is not part of a larger one containing Potentia of intermediate realities, indefinite entities, etc. Indeed, the reformulated statement leaves the door open for the possible existence of information of a non-statistical nature or eventual incompleteness (and imprecision) of the statistical information given by ψ. That is, it acknowledges indirectly the possibility for substantial changes of standpoints in the future, whereas the CI had the pretension of having discovered an ultimate truth about the possibilities of human reasoning and knowledge, viz their inescapable obstruction due to complementarity.

A question that has not been answered as yet is why measuring devices (call them M) need to be *genuinely* classical in the CI. The idea that due to the classicality of our devices we remain always tied to classical notions in describing particular experiments does not appear to offer a clear explanation why our devices should be exactly classical rather than just nearly classical (quasiclassical, as it is sometimes asserted [24] (§7)) but still described by QM. A familiar reason to require (though inexplicitly in Bohr's papers) genuine classicality of M is that if one would describe M quantum-mechanically, then the ùeasurement process within $M+S$ (S satnding for the measured QM system) should also be described quantum-mechanically via the QM evolution equations (the Schrödinger equation in nonrelativistic QM) taking into account the interaction of S with M. But as pointed out by many (cf. e.g. [13], [25]), the QM evolution equations are causal whereas measurement, as already mentioned, must be acausal in the CI in order to comply with the laws of fortuity. In particular, in a QM description of observation one arrives at an unlimited chain of $M_i, i = 1, 2, \cdots$, of which M_1 must observe and reduce the superposition state of S, M_2 reduce that of $M_1 + S$ and so on (von Neumann's chain or regression). A genuinely classical M would eliminate the regression at the very first step, the "pointer" of M always giving, by definition, a sharply defined unique classical indication.

But a not less important role of a genuinely classical M in the CI is to also ensure a "clash" of the classical description of M with the distinct QM description of S that would render unanalysable in principle the specific way in which each particular datum for the QM system finds its realization in individual measurement and thus circumvent a fundamental difficulty by announcing impossibility of description of what actually happens in each particular act of observation. (This would be in unision with Heisenberg's idea about unobservable reality). Besides, unanalysability of individual events directly fits their treatment as discrete, uncontrollable and so on, so genuine classicallity of M is a basic factual requirement of the CI, aimed at evading difficulties and guaranteeing self-consistency.

But such a self-consistency of the CI does not entail consistency of the CI with QM. Really, QM requires that the physical content of the state $\psi(t)$ be determined at any instant t since generally $\psi(t_1) \neq \psi(t_2)$ at $t_1 \neq t_2$. The CI requirement that any physical information about the state should be obtained by an instantaneous act of measurement with a classical set-up, in conjunction with the above QM requirement, has as its consequence the requirement that one must be able to carry out an instantaneous act of measurement at any arbitrarily chosen and perfectly controllable instant of time t_0 (or, if necessary, a set of such acts in identical repetitions of a given situation). But the mathematical unanalysability of the system quantum object + genuinely classical measuring instrument rules out controllability of t_0 of the said kind. As a consequence, one would not know to which state exactly the obtained datum applies, so a true believer of the CI would have to declare $\psi(t)$ an inadequate concept since it requires definite (though statistical) statements at controllable instants of time, whereas within the CI such a controllability does not prove to be experimentally feasible.

Clearly, we have to do here, in particular, with an artificial inhibition of a quantal description of M that was unacceptable even to people as won Neumann, one of the goals of whose book [13], as mentioned, was to provide a rigorous mathematical backing precisely for the CI and its implications. Indeed, if a quantal-classical hybrid could be admitted at all, then this should evidently be done in a way satisfying the requirements of the basic theory : QM. So in order to be in agreement with the CI thesis asserting, essentially, that "everything is instantaneous measurement", von Neumann posited instantaneity - and hence acausality-of quantum measurement ([13], sec. V.1 ; this is his *Process 1.*, to be discerned from the causality of Schrödinger's evolution : *Process 2.*). On the other hand, in order to satisfy the requirement that the QM state be physically sensible at an arbitrary instant t, he propounded in Sec. V.1 the view that measurement could be nothing else than switching-on (at an evidently controllable, arbitrarily chosen instant) of an appropriate interaction between M and S that can supply the necessary experimental information by bringing about the acausal reduction (collapse) of $\psi(t)$ to an eigenstate of the measured magnitude.

But postulating Process 1., von Neumann encountered a difficulty at the very first step (ref. 13, ch. V.1). Interpreting the time-energy "uncertainty relation" $\Delta E \Delta t \gtrsim \hbar$ as a relation between the duration Δt of energy measurement and the expected imprecision ΔE of the measured value of E, he noted that a finite error ΔE required a noninstantaneous measurement of E of duration Δt. But if E-measurements are to be noninstantaneous then what could guarantee that measurement of any other physical magnitude would not be noninstantaneous too ? So he concluded that, with respect to measurement, "instantaneous" is to be regarded, essentially, as fast enough and adduced certain not quite convincing arguments in support of this.

In such a way he arrived at a contradiction with his own classification of QM processes since if we cannot talk about Process 1. with respect to S, then we have Process 2. with respect to $S + M$ - even if very fast - and hence absence of measurement acausality, the latter being as we know a condition sine qua non for the CI. (Wigner, who examined von Neumann's measurement theory in ref. 25, was aware of this and pointed out explicitly that the observable in the orthodox measurement theory are just *instantaneous*. Indeed, Process 2. for $S + M$ is not connected with any stochasticity [25]).

In order to get out, within the CI, of the above difficulty and reconcile von Neumann's requirement (the only natural one) for a QM description of any measurement with the instantaneity requirement of the CI about measurement one evidently needs an interpretation of the energy-time "uncertainty" relation that is different from the one adopted by von Neumann. To this end something has to be (and actually is) sacrificed by both advocates of the CI and people who are looking for general formulations, namely, part of the general *reduction postulate* to which he adhered (see e.g. [12]). As we recall, with respect to arbitrary R-measurements, in a simplest setting, the original reduction postulate says that if a given state $\psi(t)$ of a QM system S is written as

$$\psi(t) = \sum_{n=1}^{\infty} a_n(t)\psi_n, \tag{1}$$

where $\psi_n, n = 1, 2, \cdots$, stand for a complete orthonormal set of eigenfunctions of the physical magnitude R, corresponding to nondegenerate eigenvalues R_n and $a_n(t)$ is a set of time-dependent numerical amplitudes, then $|a_n(t)|^2$ gives the probability to find the eigenvalue R_n by measurement at the instant t, S itself being in the eigenstate ψ_n of R immediately after the act of observation of the eigenvalue R_n. In particular, if S was in the eigenstate ψ_n immediately before observation at the instant t, then S must be in the same eigenstate immediately after observation as well.

If we would preserve only the instantaneous-measurement assertion and the above interpretation of $|a_n(t)|^2$ but reject the collapse requirement $\psi(t) \rightarrow \psi_n$ for a measured value E_n of E at the instant t, then we would obtain the treatment of Landau and Lifshits in § 44 of ref. [24] (Landau was Bohr's disciple), and also that of more or less conventional authors as Blokhintsev and many others of the particular case of E-measurements. Namely, it is held that we can measure instantaneously E with an arbitrarily high precision but should not expect that S would be in the eigenstate ψ_n immediately after the act of E-measurement that has given an eigenvalue E_n of E. The various authors differ only with respect to their interpretation of the $E - t$ uncertainty relation but it is inessential for our consideration, so we shall not get involved in this tricky problem. (Still, it would be of some interest to adduce a particular example of an interpretation of the said relation which differs from that of von Neumann and this could be the one in ref. [24], which says that $\Delta E \Delta t \gtrsim \hbar$ gives the precision with which energy conservation in the overall system $S + M$ could be checked via two instantaneous measurements separated by a time interval Δt).

More generally, "destructive" measurements of the mentioned kind are acknowledged for magnitudes $R \neq E$ too : cf. e.g. [24], § 44, [26], ch. 2.3, and [12].

Now that the literal understanding of instantaneity of measurements within the CI is restated, we can pass to von Neumann's consideration in ch. VI of [13] that de facto rests on such an understanding and is intended to substantiate the so-called principle of *psychophysical parallelism* formulated, as emphasized by him, in agreement with Bohr's conceptions. (Von Neumann has in mind ref. [19c] in which Bohr asserts, in particular, nonexistence of a strict boundary between QM phenomena and the means of their observations ; cf. also B2). As held by von Neumann, according to the said principle (my italics) " the *extraphysical* process of subjective perception can be described as if it were existing in the physical world". (That is, he admits the possibility that such a state of things could be just illusory since perceptions lie beyond the physical world but the said principle requires that, in any case, perceptions should not appear to be in conflict with factual physics). For the particular consideration in ch. VI this principle states that, denoting by Ob the observing individual, one would arrive at the same measurement statistics both when $M+$ Ob is regarded as a single measuring system that instantaneously reduces the state of S at a given moment t_0 and when $S+M$ is regarded as a single QM system undergoing Process 2. and reduced by Ob's extraphysical perceptions at the instant t_0+t. Assuming the existence of certain quite special evolution operators for $S + M$ and adhering to the original reduction postulate (which fact narrows within the CI the scope of validity of this conclusions) von Neumann demonstrated that the said principle can be substantiated. (But to this end the reduction of $S + M$ by Ob must be treated as an extraphysical event indeed since in the absence of such an interpretation of the putative collapsing role of Ob one would immediately arrive, as mentioned, at von Neumann's infinite regression). In such a way the above principle and argument reintroduce the Observer in the physical picture, at that endowed - in addition to his complementary manner of reasoning - with the extraphysical capacity of turning wave functions into noninterfering statistical mixtures. The latter role of the Observer is obviously intended to supersede the genuine-classicality-of-M assumption.

Rather than introducing queer principles and arguments in order to sustain Bohr's ideology in its main points, von Neumann could adopt a more natural viewpoint having to do with physics. He could assume, for instance, that measurement plays an exaggerated role in the CI. He could also assume that instantaneous turning-on of measurement interactions at a given moment t is not a good idea (this is never done in the literal sense in experiment), though inevitable if the t-dependence of ψ is not to be regarded as the consequence of objective fundamental factors outside the context of measurements. And in the end, he could assume that QM is not a theory that could describe everything in physics, say the objectification of measurement results. (The latter conclusion was made e.g. by Wigner [25], cf. also [12]. it might offend only those who are chasing the mirage of the Final Physical Theory as embodied in QM).

In any case, it would be of definite interest to learn Bohr's opinion about von Neumann's conscientious efforts to vest his interpretation of QM in strict apparel. Surprisingly enough, such an opinion does not appear to exist, at least "officially" : nowhere in refs. [19] and the practically complete collection of Bohr's philosophical papers [20] did I see any mention of von Neumann's name, in contrast with that of, say, Heisenberg that is is omnipresent. Clearly, Bohr was offended by von Neumann's approach. The idea of the latter to admit a QM rather than a purely classical description of measuring devices could be one possible reason for this.

Von Neumann's book [13] is the work of a mathematical genius possessing a very serious background in physics, who proved able to make a substantial progress in the strict formulation of practical formal rules for the QM mathematical operations. But these contributions are to be discerned from his efforts to find a mathematical language for the Copenhagen doctrine since QM and the CI are not one and the same thing. In the latter case his approach shares a number of the fundamental difficulties plaguing the CI. Some of these were already considered here. Others are also worth mentioning. Namely, we do know that in QM the "diameter" D of the region in which the wave packet $\psi(t)$ for, say, a free particle S is $\neq 0$ may be arbitrarily large, e.g. equal to that of the Milky Way or n times larger, $n \to \infty$. How could we measure, according to von Neumann or the CI, the position of S within such a wave packet at a given instant t? In principle, the only thinkable possibility is to emit at t a signal from the Earth that will cover at the same instant the entire region in which $\psi(t) \neq 0$ and come back, once again at t, with the necessary information. That is, we have to admit infinitely large velocities of material effects. (A similar objection due to Einstein is examined in ref. 8, p. 74-76). Besides, in a given inertial frame K an observer Ob could measure instantaneously at moment t the positions $\vec{x}_1 \neq \vec{x}_2$ of two QM objects whose state at that moment was $\psi(\vec{x}_1, \vec{x}_2, t)$. But for an observer Ob$'$ in a frame $K' \neq K$ the two measured positions $\vec{x}'_1$ and $\vec{x}'_2$ would not generally be simultaneous if what special relativity tells us is true, so the CI has to say that, for Ob$'$, what Ob does is not correct position measurement which has to be instantaneous for the former too. There would exist a general agreement among observers on what they measure only in presence of an absolute simultaneity concept.

Both this cases show that, in principle, the CI and its measurement theory due to von Neumann represent idealizations underlain by Newtonian mechanics. I could find no mention of this fact (or of any of the other difficulties of the CI) in Bohr's papers.

A curious feature of the considerations in this disproportionate section is that what we did was, to a large extent, interpreting the Copenhagen interpretation! Indeed, it was propounded in a fuzzy and half-expressed form that made it streamlined and difficult enough for critique. In combination with the straightforward belief that great physicists as its creators ought to "automatically" be great philosophers as well, the latter fact could easily drive one at the time to the idea that one's own lack of good (or any) understanding of the CI and the success of QM in generating good experimental numbers were somehow due to the profundity of the philosophical basis of the CI that could be attainable only to a few elevated thinkers. One was not supposed to surmise that there were higher-grade philosophical attitudes (at least to my tastes) already at the dawn of philosophy in ancient Greece (recall Heraclitus) or that the numerical success of QM was explicable with a well guessed equation of motion and system of practical postulates which needed no metaphysical substantiation for being posited. Another reason for the dominant position of the CI until the 1960's may be found in de Broglie's explanation of this (cited in ref. 5) : Bohr was the pugnacious leader of an influential school whose massive intellectual pressure could easily suppress individualistic heresy. (Recall that names as those of Pauli, Kramers, Landau, Peierls, etc, figure too in the list of advocates of the CI). And in the end, it would be just hypocrisy not to mention one more factor for the long-lasting ideological success of the CI that is typical in the implantation of mass-scale beliefs of any kind. It is well illustrated by the words of ter Haar [27], the former editor of Physics Letters : "The 'accepted' wisdom as concerns the philosophy of quantum mechanics is the one proposed by the so-called Copenhagen School. This is very strongly backed and I remember once being told by the publisher of a journal of which I was an editor that I should not publish some work by F. J. Belinfante as it dared to question and to consider alternatives to the Copenhagen interpretations. These instructions were based on advice given by one of the high-priests of the Copenhagen School".

2. De Broglie's interpretation

This interpretation is the antipode to the CI. It wholly rests on physical assumptions and images. As a consequence of that its conceptual advantages are visible "to the eye", as well as its problems and difficulties, which are not concealed with transcendental arguments. Just the other way round, the difficulties noted by its author are considered in detail and possible ways out are indicated. The same applies to his attitude to the critiques against the interpretation. All this makes it possible for anyone to decide whether the approach in question contains (or not) valuable insights.

The existence of exhaustive expositions of this interpretation as those cited above enables me to examine here briefly only some of its features that demonstrate well enough its essence. It was first formulated in 1927 as the theory of the double solution (see [8], ch. IX, for historic details and references) whose basic principle reads :

To every continuous solution $\psi = a\exp(i\varphi/\hbar)$ of the equation of propagation of wave mechanics there must correspond a singularity solution $u = f\exp(i\varphi/\hbar)$ having the same phase φ as ψ, but with an amplitude involving a generally mobile singularity ([8], p. 99).

The function u is called upon to give account of both particle-like properties (represented by the position and motion of the singularity) and the wave-like properties (since u is a wave function). The picture which such a u-wave was expected to confirm consisted thus of a particle imbedded in a wave phenomenon that had to guide the particle's motion in a way consistent with the diffraction and interference properties of a wave. Clearly, this would be a physical explanation of what is called wave-particle duality.

This picture encountered a serious difficulty. Indeed, if we write u as

$$u = u_0 + v, \tag{2}$$

where u_0 is the singular part of the u-wave and v is, by assumption, regular and proportional to the solution ψ of the respective (relativistic or nonrelativistic) evolution equation to which u is a solution as well, then since both u_0 and v are solutions to one and the same linear equation they would evolve independently, so the v-part would have no piloting effect on the u_0-part of u. In conjunction with the boom of the CI at that time, this made de Broglie abandon his interpretation and return to it about a quarter of a century later under the impact of Bohm's paper [28] ; that author had independently rediscovered, in particular, a truncated variant (pilot-wave theory) of de Broglie's initial theory.

De Broglie saw a way out of the above difficulty by invoking nonlinearity for the description of the physical u-wave that gives account of the properties of the individual QM object [8, 9]. Namely, it was no longer necessary to envision the particle-like "kernel" u_0 of the wave as the carrier of a genuine mathematical singularity ; it suffices to treat u_0 as a very large and fastly varying function of position in a very small region D (of "diameter" $\leq 10^{-13}$ cm) and practically zero outside D. Inside D the equation of motion is assumed to be essentially nonlinear, which nonlinearity also "solders" u_0 and v to each other ([9], p. 60). Outside D (where $u_0 \approx 0$) there remains practically only the v-part of $u = u_0 + v$, so there the equation of motion practically coincides with the respective linear equation of customary QM (since v is proportional to ψ in the said region which is much larger than D). Near the end (edges) of the wave packet ψ nonlinearity is assumed to play an essential role once again, thus preventing the smearing of v with time, which behaviour is typical for ψ in the linear case. In this way one can eliminate another difficulty too, i.e. the "divesting" with time of the u-wave of its v-part whose role is to give account of the wave-like properties of the QM entity.

The abovementioned simplest characteristics of de Broglie's ideology contain so strong a potentiality for physical explanations and implications with regard to micro-phenomena that it is worth considering these without undue delay.

Objectivity. The QM particle is no longer the totally indefinite object of the CI : it has an objectively existing position all the time, namely, the (approximate) position of the kernel u_0, the latter being, as mentioned, the carrier of the particle-like properties. The kernel reacts to both external (classical) potentials and to the so-called *quantum potential* [8] due to the physical (and hence objective) v-wave and representing the particular mechanism through which the diffraction and interference properties of the v-wave lead to wave-like statistical position distributions in macroscopic experiment in the presence e.g. of various obstacles and slits (that can be described mathematically by boundary conditions), etc.

The two-slit experiment. The model contains a ready picture of what happens in this (thought) situation : the particle's kernel u_0 passes through one of the slits, whereas the v-wave that guides the kernel's motion passes through both slits thus taking account of their simultaneous existence. The statistical interference picture appearing in many repetitions of the experiment is assumed to be due to the *hidden* statistics of the initial conditions for the quantum entity.

Disjoint ψ-wave packets. For the case when the customary QM ψ-function is nonzero in only several disjoint regions $V_i (i = 1, 2, \cdots, n)$ of space de Broglie's model says that the kernel (and hence the charge, etc) of the particle, together with part of the overall v-wave is in only one of these regions, whereas empty v-waves (devoid of a kernel but nonetheless physical) exist in the remaining volumes V_i too. If free evolution is allowed, these empty waves will cause interference phenomena.

The Pfleegor-Mandel experiment [29]. In this experiment two independent lasers are aimed at one and the same region V in space. In spite of the fact that the system contains no more than one photon at a time, the interference picture in V is characteristic of two waves.

A straightforward explanation of this experiment that needs no mathematical formulas says [30] that the individual photons are guided by the joint action of two de Broglie v-waves emitted by the lasers, one from each laser.

No instantaneous micro-effects. The theory is causal and hence inherently adverse to acausality of any kind, instantaneous micro-effects in that number. This feature will be evident in de Broglie's measurement theory as well.

Finite (nonzero) size of micro-particles. The finite (though small) size of the kernel u_0 is a salient feature of the model. In a sufficiently developed field theory of this type it could lead to the absence of divergences that plague contemporary quantum field theory.

A more general theory than conventional QM. It so happens that any new physical which can be regarded as a generalization of an older theory eliminates certain basic features of the latter. In principle, de Broglie's approach possesses such a feature when compared with conventional QM : the former is nonlinear, whereas the latter is a linear theory. At the same time, in typical quantum phenomena de Broglie's approach is envisioned as satisfying a specific "principle of correspondence" with conventional QM. For example, in the two-slit experiment the passage of the kernel u_0 through one of the slits introduces a certain asymmetry in the interference picture when compared with the purely symmetric way in which the homogeneous ψ-wave of conventional QM passes through both slits. But having in mind the presumable very small size of u_0 in comparison with the width of the slits, the accompanying v-wave will behave in practically the same way as that of ψ, so the difference between the two pictures will be negligible.

We pass now to a brief consideration of de Broglie's measurement theory which substantially rests on the ideology delineated above and, in particular, on the objective character of particle position (which is not created by position measurement as asserted in the CI but just revealed by such an observation in de Broglie's interpretation). We shall examine only the case of measurement on individual particles carried out with the aid of a macroscopic apparatus M, which is nevertheless not regarded as a classical body. (For a detailed more general consideration cf. ref. 9).

This theory takes into account the fact that a macroscopic M is not a device obeying von Neumann's measurement theory. The typical case is that such an M consists of an analyser and a detector. (A Stern-Gerlach-type set-up for the separation of ψ-components corresponding to different projections of the internal magnetic moment may serve as an example). The role of the analyser is to decompose -within finite time and via quantal interaction - the wave function (packet) of interest into a set of practically nonoverlapping "subpackets", each of which corresponds to a particular value R_n of the measured magnitude R. (For simplicity, we examine the discrete case). The role of the detector is trivial : it has to register the objective presence of the particle's kernel in a particular subpacket (the remaining subpackets corresponding to empty waves), which event will supply the observer with the R-datum obtained in the particular act of measurement. In such a way the objective position of

u_0 supersedes in de Broglie's theory the macroscopic pointer of von Neumann's theory. If one wishes to measure a magnitude Q whose operator $\hat{Q}$ does not commute with the operator $\hat{R}$ corresponding to R, then one would have to employ a measuring set-up that is essentially different from (and hence incompatible with) that measuring R.

The above theory has evidently nothing in common with any variant of the CI outlook on measurement that rests on instantaneity. Indeed, de Broglie's theory de facto says that in order to measure a definite R-magnitude in the state $\psi(t_0)$ (Eq. (1)), we must have at our disposal fields ensuring the conditions in which the initial state $\psi(t_0)$ will disintegrate with the course of time ($t > t_0$) into the necessary set of well separated subpackets. This is an objective physical process whose finite duration in time can be determined in principle via the relevant evolution equation of QM. During this process, in de Broglie's terminology, the *predicted probabilities* (by QM) turn into *present probabilities.* (Present statistics is the one obtainable by direct detections, without any need of further preparation, by repeating the measurement experiment in identical physical conditions ; cf. also ref. 1 and 4 ; clearly, position statistics is always present in the theory under consideration).

Is such a viewpoint admissible in principles ? The answer is yes. Indeed, it turns out, in particular, that the role of the analyser may be taken up by empty space itself in measuring the statistics of momentum distribution by the time-of-flight technique (see e.g. refs. 31 for momentum (velocity) measurements in the relativistic spin $\frac{1}{2}$ and spin 0 cases, respectively, and for further references). For example, if the QM probability amplitudes $a(p)$ for momentum - both relativistic and/or nonrelativistic - are $\neq 0$ only in intervals (p_1, p_2) and $(p_3, p_4), p_1 < p_2 < p_3 < p_4$ along the z-axis, then the initial wave packet $\psi(z, t_0)$ will disintegrate with the course of time into two spatially separated wave packets ψ_1 and ψ_2 along z such that the form of ψ_1 will be determined only by $a(p), p \in (p_1, p_2)$ and that of ψ_2 by $a(p), p \in (p_3, p_4)$. At that, the predicted QM momentum statistics (given by $|a(p)|^2$) will be exactly reproduced by position detections in the limit $t \to \infty$ for an arbitrary initial state $\psi(z, t_0)$. In de Broglie's terminology, the position statistics of $\psi(z, t_0)$ in an ensemble of identically prepared systems is objectively present at the initial moment t_0 of interest, whereas momentum statistics applying to $\psi(z, t_0)$ will become present at sufficiently large times $t > t_0$, when $\psi(z, t)$ will be drastically different form $\psi(z, t_0)$ but, at the same time, of a form permitting the unequivocal determination of the momentum distribution corresponding to $\psi(z, t_0)$. Therefore, we have here also a concrete corroboration of the thesis that noncommuting observables need essentially different sets of wave packets, obtained in incompatible apparata, for their experimental determination.

An example of wave packet separation in the case of measurement of stationary-state energies in a Stern-Gerlach-type set-up can be found in ref. 32 (§ 111). The same set-up can be employed for spin-component measurements too and it is of interest to consider the case of electrons (spin $\frac{1}{2}$) in an eigenstate of the spin z-component, entering a x-oriented Stern-Gerlach set-up. The two subpackets that will obtain in result of the said procedure will correspond to $\pm\frac{1}{2}$ spin eigenvalues along x, so the spin component of any one of these along z is no longer definite. If, however, instead of registering the positions of the two packets we would create conditions (with the aid of auxiliary fields as described in ref. 25) for them to reunite, the new single wave packet would represent a spin $\frac{1}{2}$ eigenstate along z once again. In terms of de Broglie's conception this ought to mean that empty waves can have a strong influence on the spin variable in cases of interferences with a full wave (containing the kernel).

In such a way de Broglie's ideology propounds a graphic admissible picture for QM effects and measurements contrary to the CI interdictions and the no-HVs arguments. The major problem (and difficulty) within its frame is to find out the physical principle that could determine in a unique fashion the basic nonlinear equation of the theory. Such a principle could also shed light on the nature of de Broglie's (presently hypothetical) v-wave. In the absence of such a clarification it would be quite difficult to see which experiments (recent ones in that number) unequivocally confirm or disprove its existence. Indeed, it is sometimes argued, say, that the wave-like diffraction patterns due to scattering from crystal gratings could be explained with discrete exchange of momentum between the micro-particles and the grating (see e.g. [18] for a brief discussion and references). But it is to be noted that micro-particles exhibit unusual non-pointlike properties in other kinds of diffraction and/or scattering as well, e.g. in the idealized two-slit experiment or low energy scattering from an impenetrable sphere of radius r, the total scattering cross section $\sigma = 4\pi r^2$ being then four times larger the geometric cross section of the sphere ; also, as well known, in certain nuclear reactions the respective cross section can be about 10^5 times larger that the geometric one. These effects are evidently not in line with the mentioned assumption in [18]. Therefore, the explanation of the nature of such effects is a problem of primary importance, and the only cogent physical conception for the time being appears to be that of de Broglie's v-wave in general and empty v-waves in particular ; their eventual experimental discovery could possibly open up a new page in physics.

However be it, de Broglie's theory has a fundamental philosophical significance : it demonstrates with its very existence that beliefs about what is possible and what not for human physical reasoning can only be part of personal metaphysics rather than physics.

3. The consistent-histories approach

The goal of this new approach (cf e.g. the reviews [33-35] is to achieve an interpretation of QM and, in particular, of closed QM systems that could be based on a minimal number of quantum postulates permitting a cogent outlook on the character of measurement procedures, quasiclassical transitions, purely quantal properties of closed systems, etc, and at the same time evading some unacceptable features of the CI as the presumable jump-like character of micro-effects, genuine classicality of measuring apparata, and role of the observer's conscience. (At first sight the objective of Everett's relative state interpretation [36] is the same but its inspection reveals a large number of inexplicit postulates and also certain objectionable features, so one could not possibly admit that the latter approach has achieved its goal ; cf. also [12] and [26] for critical remarks and references).

The basic tools of the consistent-histories (C-H) approach are Schrödinger dynamics and the customary Hilbert space formalism of QM plus two specific postulates, one of these being interpretational. It states [34] :

Every description of a physical system should consist of histories belonging to a unique consistent family and every reasoning should consist of legitimate implications.

The definition of the central concept of consistent histories obtains by defining first the concept of *physical property (attribute)* of a QM system. The C-H approach adopts von Neumann's definition according to which a property is expressed by the assertion that the values of an observable R belong to a given range r at a given moment of time t, each such statement being in one-to-one correspondence with a definite projection operator P in Hilbert space. But in contradistinction with von Neumann, properties are not necessarily associated with measurements.

(Really, the C-H approach must say in its own language what does measurement mean in a closed system $S+M$, so it has to evade assertions about measurements that are external to the theory). In such a way, although in an abstract form, the C-H approach adopts a broader outlook on physical events compared to that of the CI. (For the latter, "event" could only mean measured event, so in it, say, the decay of an atomic state, if not registered, is nonexistent ; in the C-H approach this would be an event).

A history of a system is a sequence of properties occurring at times $0 \leq t_1 \leq t_2 \leq \cdots t_n$, the corresponding projectors being $P_{\alpha_1}(t_1)$, $\cdots, P_{\alpha_n}(t_n)$, where $P_{\alpha_m}(t_m) = e^{iHt_m/\hbar} P_{\alpha_m} . e^{-iHt_m/\hbar}$ (the initial moment of time and statistical operator being $t_0 = 0$ and $\rho(0) = \rho$, respectively), $\alpha_m, m = 1, 2, \cdots n$ standing for the properties of interest and H for the Hamiltonian of the (closed) system.

The next step is to define a candidate probability for a given history :

$$p = Tr\left[P_{\alpha_n}(t_n) \cdots P_{\alpha_1}(t_1) \rho P_{\alpha_1}(t_1) \cdots P_{\alpha_n}(t_n)\right]. \tag{3}$$

Formula (3) represents the second major postulate of the C-H interpretation.

If this candidate probability proves also a good probability, i.e. if it is positive, normalized to unity and additive for mutually exclusive histories belonging to a family of histories under consideration, then it is said that the histories in question are *consistent.* These are the only histories that are of interest in the C-H approach, their probabilities being treated as classical probabilities admitting meaningful statements about the system under consideration by using classical logic. (For example, it is assumed that measurements are to be discussed in the language of consistent histories). The histories which are *inconsistent* (i.e. which are not included in any consistent family) are treated as "meaningless" [34], [37] and even as "quantum nonsense" [38]. Such are, say, the histories connected with the two-slit experiment, in which we have two double-time histories, namely : the particle passed through slit 1 at moment t_1 and hit the screen at point $\vec{x}$ at moment t_2, and also : the particle passed through slit 2 at moment t_1 and hit point $\vec{x}$ at moment t_2. Due to the interference phenomenon on the screen, the position distribution there cannot be represented as the sum of two noninterfering probabilities connected with each one of the two histories, whereas for sets of consistent histories interference must be negligible for all practical purposes. The C-H approach declines any interpretation of such interfering histories.

We shall not go any further in the mathematical and ideological development for the C-H approach since what we have hereunto will be sufficient for certain comments. (The mathematics and ideology of the C-H interpretation are involved enough and one could get detailed information about them in the cited references and references therein). The first thing to note is that, within the range of QM in its contemporary form, the C-H interpretation represents a large step forward compared with the CI since it rests on no a priori metaphysics. Instead, it endeavours to derive a consistent outlook on the micro- and macro-world mainly from the mathematical apparatus itself of QM (see also the beginning of this section). It is stated that the approach has attained a number of its goals and significant results as objectivity (observer-independence), reduction of the wave function without sudden far-away effects, and locality of QM, i.e. "nothing done somewhere can affect instantaneously something at a large distance" [34]. In principle, this appears to be the near-maximum that may be achieved within quantum theory without infringing its mathematical foundations.

At the same time, it is clear that problems of fundamental importance are left unanswered by the C-H interpretation. First and foremost, the said approach flatly refuses to interpret the interference effects and hence to offer any hypothesis about their physical mechanism ; this is certainly natural for an interpretation resting on mathematics that predicts the existence of such effects but does not go any further ; in particular, it contains no model of the micro-particle.

The C-H interpretation does not explain as well (and this is acknowledged) how - and why - is a unique datum selected and actualized in reality. That is, it offers no mathematical picture of what happens e.g. in the individual noninstantaneous act of measurement and in the individual noninstantaneous atomic transition. This is natural too for an approach that rests on the statistical apparatus of QM and disregards the possibility for a more detailed description of quantum phenomena (say, with the aid of presently unidentified - "hidden" - variables). But the mentioned individual processes and interference effects exists as physical phenomena in reality and it can be lawfully expected that this be at least acknowledged in principle for the time being. (As for the future, there is little doubt that the challenge will be met sooner or later since this always happens in physics).

My last remark concerns the general character of the C-H interpretation. As mentioned, the mathematics and the ideology of this approach are very intricate. Things become even more difficult in the numerous occasions in which the interpretation has to do with the many-body problem (e.g. in considering measurements), where model considerations based on “ruthless oversimplification” ([37], p. 241) are inevitable. In such conditions it is possible indeed that a number of present-day conclusions of the C-H interpretation may undergo substantial changes in the future. For example, Omnès treats complementarity as an unavoidable feature of microscopic events ([35], p. 15). According to him, complementarity appears in the C-H interpretation “as the existence of several different history families which can describe the *same physical situation* (my italics) but are mutually incompatible, though each of them is consistent”. Now, as we know, in the CI complementarity applies to *different* physical situations connected with incompatible $(\vec{p}, E)$ and $(\vec{x}, t)$ measurement procedures. (The respective closed systems are $S + M_1$ and $S + M_2, M_1 \neq M_2$. It is thus quite possible that the feature mentioned in [35] corresponds to the fact that nonrelativistic QM (within whose frame the C-H approach is formulated) admits various mutually exclusive representations for one and the same S as position representation, momentum representation, etc, but the latter fact is not related to complementarity.

4. The no-hidden-variables arguments

As noted above, the no-NVs arguments could be regarded as the grandchildren of the CI, endeavouring to substantiate Bohr’s psychophysical views on QM as essentially valid and predetermining the irreducibly probabilistic nature of any admissible theory of the micro-world. Specifically, these arguments (which are often unhesitatingly termed theorems by their authors) assert nonexistence of HVs theories, which could give a more detailed description of the micro-phenomena than QM and at the same time preserve the results of the QM formalism ?

The first question that comes to mind is why should HVs theories strictly preserve the results of QM ? Relativity theory, e.g., preserves practically nothing of Newtonian mechanics but nonetheless the latter theory is of tremendous utility in its range of applicability. On the other hand, quantum theory does not preserve its own results : nonrelativistic QM, say, is less precise than the Dirac equations in describing the energy spectrum of the hydrogen atom ; theDirac equation cannot describe the

Lamb shift ; the latter appears to be described by quantum electrodynamics but at the price of introducing recipes (that are not part of the theory) for the removal of divergences, the divergences themselves representing a reluctantly acknowledged catastrophe of the theory, etc. Still, it would be instructive to see what certain well known no-HVs arguments have to tell us.

The first, most famous, widely believed, and long-lived among these was that of von Neumann ([13], ch. IV). Starting from the customary QM apparatus : operators, density matrices (in particular pure states φ), etc, and certain generic statistical definitions and rules for computation, von Neumann demonstrated that there do not exist dispersionless ensembles in QM, whereas the most detailed information possible is the one that can be obtained from pure (normalized) states φ by the familiar QM algorithms. Any theory that offers a more detailed information would come, according to him, in contradiction with one of the rules :

I. if $\hat{R}$ is the operator corresponding to the magnitude R, then $f(\hat{R})$ corresponds to $f(R)$ and
II. if $\hat{R}, \hat{S}, \cdots$ correspond to $R, S, \cdots$, then $\hat{R} + \hat{S}, + \cdots$ corresponds to $R + S + \cdots$,

The "theorem" obtains if one would regard, together with von Neumann, rules I. and II. as firmly established, once and for all.

The above argument was disproved by de Broglie in the 1950's ([8], p. 70, 71 ; [9], p. 29, 30 ; cf. also [4]) on the basis of his conception of probabilities (Sec. 2) : von Neumann's consideration applies to the predicted QM probabilities that are checked by measurement and is irrelevant to the hypothetical initial hidden probabilities in an HV theory. The very existence of the de Broglie- Bohm pilot wave theory in itself invalidates the "theorem". But the latter crashed down in reality (most probably for psychological reasons) only after Bell's attack against the application of rule II. to HVs theories [39] : in the case of noncommuting $\hat{R}$ and $\hat{S}$ it may be applied only to the average $< \hat{R}+\hat{S} >$ rather than $R(\lambda) + S(\lambda)$, λ standing for a particular hidden variable, as $R(\lambda)$ and $S(\lambda)$ cannot be expected to be simultaneously existing then.

Later on the said step of von Neumann was called silly by Mermin [40] (and the latter quotes Bell for also qualifying it in an interview as silly and even foolish). Assume, however, that von Neumann's argument were free of the said flaws and there were no critiques as those mentioned above. What would he have actually shown then, in principle ?

Clearly, his argument would have given an answer to a yes/no alternative : Will and HV theory give the same results as QM ? No. But he did not show how big was the “no” ; it could be negligible or even we could have an HV theory that contains QM as a limiting case in a similar way to the relation between Newtonian mechanics and relativity theory. So his “no” is pointless.

He evidently felt that he had gone too far with his argument, so he had to say at the end of Sec. IV.2 that “no doubt, QM in its present state is incomplete and it might even turn out wrong”, though he found such a possibility quite improbable in the light of the amazing success of QM. So what did he really prove in the long run ?

He proved nothing. With the same success Laplace, say, could have proved, on the basis of similar logic, a no-go theorem for any theory different from Newtonian mechanics in the light of the amazing success of that mechanics at Laplace’s times. The only thing he would have to require for the proof would be coincidence of the basic features of the mathematical apparatus of any new theory with those of Newtonian mechanics, thus ruling out relativity theory and QM.

It could be thought that if the “perfect-logic machine” (as an admiring colleague of von Neumann referred to him at the time) had undergone such a fiasco with his argument people would take a more sober point of view on what could actually be proved in physics and what should better not be undertaken. What happened was quite the opposite : a multitude of new no-HVs arguments appeared. Three of the most well known among these are those of Jauch and Piron [41], Bell [42], and Kochen and Specker [43]. (By the way, the basic theorem in [43] represents an answer to a yes/no alternative as was the case with von Neumann’s argument). From one viewpoint or another all of these were criticized in many works (cf. e.g. Bell’s critique [39] against [41] and [43], Bub’s [15], Lochak’s [2, 4], and our [44] critique against [42], and Bub’s critique against [41] and [42] in ref. 15). I shall discuss here a common unwarranted feature of the argument [41-43] in the spirit of refs. [44] (perhaps generalized to a certain extent).

All three arguments rest - explicitly or inexplicitly but essentially - on the assumption that an HVs theory ought to obey classical probability theory. (In a recent paper [45]it was asserted that Bell’s inequalities can be obtained without employing probabilities ; the faulty character of this statement is evident in the derivation of the inequalities in [45], which essentially employs probability theory without explicitly using its terminology). This conjecture appears to be so natural that it is not perceived at all as a hypothesis that needs justification. But the statement than an HV theory need not contain, in itself, any a priori concept of probabilities has the same right of existence from the viewpoint of logic as its mentioned negation.

Indeed, what we know about the case of macroscopic measurements is that the axiomatics of QM predicts probabilities $p_{QM}(R)$ for the possible values R of the quantum magnitude $\hat{R}$ that ought to be written as

$$p_{QM}(R) \equiv p_{\psi,M_{\hat{R}}}(R) \tag{4}$$

in order to give account of the fact that probabilities can be assigned to a given QM state ψ only within the context of a given measurement procedure $M_{\hat{R}}$ (($\hat{R}$ is a fixed symbol (operator), whereas R is a variable ; as we know, the identity (4) holds both in the acausal CI and the causal de Broglie's conception about measurements, but we are not committing ourselves now to any particular interpretation in order that the consideration be as general as possible. But the identity (4) means that the assertion "quantum probabilities are generated by measurements" is lawful since the QM probabilities assigned to ψ are seen to be essentially dependent on the measurement procedure and have no likeness to objective individual characteristics of the micro-object as charge and mass. At the same time, since a sensible probability theory requires a strict definition of the concept of stochastic experiment in order to arrive at valid probabilities, the identity (4) is nothing else than such a definition for the case of QM.

In Kolmogorov's axiomatics of the theory of probability, offering a general outlook on probabilities (and directly applicable, in particular, to classical statistical mechanics) an appropriate definition of stochastic experiment (probability space) means defining a triplet (Ω, F, μ), where Ω is the set of all fortuitous elementary outcomes, F is a σ-algebra (Boolean algebra) of sets of outcomes, and μ is a probability measure defined on each event $\in F$. (The Kolmogorovian theory is most often called "classical" but we shall not do so here in order to evade confusion). Two stochastic experiments are identical only if the respective triplets are identical. Specifically, triplets (Ω, F, μ_1) and $(\Omega, F, \mu_2), \mu_1 \neq \mu_2)$, that differ only in the last members represent different stochastic experiments on the same set of outcomes and events.

In what follows we shall make use of certain analogies that are to be treated as nothing else than thought-provoking examples. So as an elementary illustration of the above situation we may examine a cylindrical cavity directed along the $z-$axis ($0 \leq z < \infty$), in contact with a thermostat at temperature T and containing a classical point particle. We may ask what is the normalized probability density for the particle's position ? Although we obviously have to do with a stochastic situation (the unknown position z can be arbitrary), the question makes no sense since the stochastic experiment is not yet fully determined and no normalized position probability density (measure) exists. If we now "switch on" a homogeneous gravitational field along z, the probability space (stochastic experiment) will be fully defined and a normalized barometric-type probability density $\rho_{T,g}(z) \propto exp(-mgz/kT)$ will be "born". If we then turn on a gravitational field of different intensity, we shall obtain the essentially different density $\rho_{T,G}(z) \propto exp(-mGz/kT), G \neq g$, on the same set of outcomes and events, i.e. we shall have a different stochastic experiment. At a will, one can note a correspondence $\psi \leftrightarrow T$ and $M_{\hat{R}} \leftrightarrow f$ between what we have in (4) and the example considered.

We can now return to the problem of the possible existence of HVs theories underlying QM. From the above consideration it follows that the least restrictive assumption about such a theory would hold that there can exist no a priori Kolmogorovian probability space on the set of possible HVs ("outcomes") λ, in analogy with the case of Newtonian mechanics which admits no a priori probabilities of any kind with respect to its states. Examine a given QM state ψ and let the set of HVs λ, corresponding to it, be $\Omega \equiv \Omega_\psi$. Similarly to what we had in the classical barometric case prior to the turning-on of the gravitational field along z, we know that we are dealing now with a fortuitous situation but we do not have as yet a probability space since we have not fully defined the stochastic experiment. Assume then that we decide to perform a (macroscopic) R-measurement. What QM could say for the situation under consideration is: if the state ψ is underlain by a set Ω_ψ of HVs λ, then R-measurement will turn Ω_ψ into a probability space $(\Omega_\psi, F_{\psi,\hat{R}}, \mu_{\psi,\hat{R}})$. (In a similar way, the inclusion of the gravitational field turned be initially nonnormalizable set $\Omega = [0, \infty)$ of positions z into a probability space $(\Omega, F, \mu_{T,g})$, where F is the σ-algebra of all Borel sets in $\Omega = [0, \infty)$ and $\mu_{T,g}$ is given by the barometric formula). Specifically, the analogue of the identity (4) can now be written as

$$\mu_{HV}(\{\lambda\}_R) \equiv \mu_{\psi,\hat{R}}(\{\lambda\}_R), \tag{5}$$

where $\{\lambda\}_R$ stands for the set of HVs λ that generate an eigenvalue R of $\hat{R}$. More generally, the symbols $\hat{R}$ and R can be regarded as denoting all the observables of a given complete set of mutually commutative operators and their eigenvalues, respectively.

If the device $M_{\hat{R}}$ is described by a set of parameters π and $\mu_{\psi,\hat{R}}$ admits the introduction of a density function $\rho_{HV}(\lambda)$, then the identity (5) can be written as

$$\rho_{HV}(\lambda) \equiv \rho_{\psi,\pi}(\lambda). \tag{6}$$

Examine now a Q-measurement for which $\hat{R}\hat{Q} \neq \hat{Q}\hat{R}$. Both causal and acausal measurement conceptions hold that the respective macroscopic measuring devices are incompatible and we can base our consideration on such an assumption for the sake of definiteness of the argument. We would thus have two essentially different measurement procedures that would generate essentially different probability spaces $(\Omega_\psi, F_{\psi,\hat{R}}, \mu_{\psi,\hat{R}}) \neq (\Omega_\psi, F_{\psi,\hat{Q}}, \mu_{\psi,\hat{Q}})$. It is could happen though that $F_{\psi,\hat{R}} = F_{\psi,\hat{Q}}$. We must necessarily have then $\mu_{\psi,\hat{R}} \neq \mu_{\psi\hat{Q}}$ since otherwise the probabilistic experiments would be identical, which would be a contradiction. That is, in this case a common event for both probability spaces (if such exists) would generally be assigned different probabilities in R-measurements and in Q-measurements but this fact would be as noncontradictory as was the analogous fact in the classical barometric case, when $g \neq G$: incompatible stochastic experiments generate incompatible mathematical probability spaces and $\mu_{\psi,\hat{R}} \neq \mu_{\psi\hat{Q}}$ is one way to express this. And we need not be bothered by the problem of what could be the specific mechanism that might turn Ω_ψ into an appropriate probability space in a particular stochastic physical experiment : this would be a problem of particular HVs theories. (In de Broglie's theory, if free of the hidden-probabilities hypothesis, this would be the familiar problem of demonstrating the decomposition of the initial wave packet into a set of appropriate wave packets). What is important is that the unwarranted character of the "self-evident" assumption in [41-43] about the a priori existence of a Kolmogorovian probability space in the set of the possible HVs is now clear enough : Probabilities do not exist by themselves, without relation to stochastic experiments. The incompatible probability spaces (Ω, F, μ) generated by incompatible stochastic experiments on one and the same (initially probability-free) set of outcomes Ω could be as physical as say the fact that in special relativity one and the same object will be assigned different spatial dimensions in the different inertial frames of reference. In a somewhat curious parallelism with the terminology of the latter theory, we can say that probabilities on Ω are relative (to measurement), not absolute.

It would be in order here to clarify, specifically, a point related to Bell's main argument [42], which endeavours to rule out possible local HVs theories ; as a rule the said point is not well understood and hence it is not well understood too that Bell's argument falls short of its goal. I have in mind the problem of *statistical dependence* (or *independence*) and the necessity to discern it from the quite different problem of *dynamical independence*. We shall say that the states of motion of two systems (e.g. particles) S_1 and S_2 are dynamically independent at given times (say time intervals) if these systems do not interact in any way at the times in question.

The definition of statistical independence of events E_1 and E_2 connected with S_1 and S_2, respectively, is well known : the probabilities of interest must conform to the equation $p(E_1 \& E_2) = p_1(E_1)p_2(E_2)$. It is important to realize that the motions of S_1 and S_2 can be dynamically independent "now" but at the same time statistically dependent due to past interactions between them. In other words, in a stochastic approach to the dynamical theory, present statistical correlations between the motions of S_1 and S_2, however intricate, cannot generally be treated as being due to present dynamical dependence of these motions since these could be just the consequence of past interactions that vanished long ago. This was not well realized, say, by Wigner whose assertion (his eq. (1)) in a paper [46] on Bell's argument amounts to stating that dynamically independent systems must also be statistically independent.

As a simple illustrations of the abovesaid one can examine the case of disintegration of a classical point particle into two particles of equal mass, interacting via a finite-range force $f(\vec{x}_1 - \vec{x}_2) = 0$ if $|x_1 - x_2| > r$. When the latter inequality holds true, the two particles will be dynamically independent but a statistical picture of their position distribution (in the case when the moment of disintegration is unknown) will contain the nonfactorable function $\delta^{(3)}(\vec{x}_1 + \vec{x}_2)$ for parent-particles located at the origin.

We now go over to Bell's argument itself [42]. It deals with couples of widely separated particles S_1 and S_2 in space (so that any dynamical dependence "now" of their motions be ruled out) and examines correlated values $A = \pm 1, B = \pm 1$ of two-valued physical magnitudes A and B (say spin $\frac{1}{2}$ projections normalized to unit magnitude) connected with S_1 and S_2, respectively. Denoting by a and b the parameters (e.g. orientations) of two noninteracting devices M_1 and M_2 (say Stern-Gerlach set-ups) that measure A and B, respectively, Bell correctly posits that, if described by local HVs λ, A and B should be functions of the kind $A(a,\lambda)$ and $B(b,\lambda)$ inside M_1 and M_2, respectively, i.e. A should not dynamically depend then on the parameters b of M_2 and vice versa. He then postulates the formula

$$p(a,b)_{HV} = \int \rho(\lambda)A(a,\lambda)B(b,\lambda)d\lambda \tag{7}$$

as the general expression describing the statistical correlations between A and B in such a local HVs theory ; the function $\rho(\lambda)$ above is an a,b-*independent* normalized probability density measure in the space Ω of HVs λ.

The "self-evident" assumption about the very existence in this case of a (normalized) probability measure in $\Omega \equiv \Omega_\psi$ is an expression of the abovementioned restrictive conception about the a priori existence of a certain probability space connected with the HVs λ. The additional assumption that $\rho(\lambda) \equiv \rho_\psi(\lambda)$ is a, b-independent (which is vital for Bell's argument) is a new unwarranted restriction due to lack of clear understanding that dynamical independence and statistical independence are quite different things.

Indeed, writing $A = A(a,\lambda)$ and $B(b,\lambda)$ is tantamount to saying that we deal with *locality* (dynamical independence). The most general assumption that the probability measure ρ is a,b-dependent cannot cancel this ; it can only call attention to the fact that, in a statistical theory, dynamical independence of physical magnitudes can coexist with arbitrary statistical correlations (statistical dependence) among them due to unknown past interactions. A specific parameter dependence of ρ, on its turn, would represent an imprint on the initially probability-free HV space Ω_ψ of the specific character of the stochastic macroscopic experiment carried out on the HVs. All this is concisely contained in the identity (6), in which π is to be replaced by the couple a,b for the case in question, nonidentical couples corresponding to incompatible stochastic experiments on S_1 and S_2.

[The form of eq. (7) does not make it clear enough which HVs λ are considered : those prior to the decay of the initial particle λ_{in} or those of the decay products - λ_1 and λ_2 - just before entering M_a and M_b ; in the latter case we have to write $\rho_{ab}(\lambda_1,\lambda_2)$ instead of $\rho_{ab}(\lambda)$ and require that the marginal densities, say $\rho_a(\lambda_1) = \int \rho_{ab}(\lambda_1,\lambda_2)d\lambda_2$, be independent of the orientation of the far-off device. The latter restriction certainly does not mean that $\rho_{ab}(\lambda_1,\lambda_2) = \rho_a(\lambda_1)\rho_b(\lambda_2)$.]

We come thus, finally, to Mernin's argument [40] (the last one to be considered in this work), which endeavours to substantiate, in particular, Bell's assertion about the nonexistence of local HVs theories, without employing probabilities. The argument can be understood easily enough, so we shall only comment on its basic assumptions about HVs theories. Mermin terms his assumptions *plausible* for HVs theories and formulates them as follows ([40], Sec. II).

"We wish to entertain the heretical view that the results of a measurement are not brought into being by the act of measurement itself. This heresy takes the states vector to describe an ensemble of systems and maintains that in each individual member of that ensemble every observable does indeed have a definite value, which the measurement merely reveals when carried out on that particular individual system... If two observables fail to commute, then the uncertainty principle does not prohibit both from having definite values in an individual system. It merely insists that it is impossible to prepare an ensemble of systems in which the values of neither observable fluctuate from one individual system to another".

In contradistinction with the opinion of that author, I find his assumptions about HVs theories implausible. The observables of present-day QM, although attributed to microscopic entities, are in a sense macroscopic since they are determined by macroscopic measurements. In this - and only this - sense they are "brought into being by the act of measurement itself" and if one would carefully consider e.g. the implications of de Broglie's theory of measurement (Sec. 2), one could see that this may be understood without any recourse to conceptions as Potentia of half-realities. Indeed, as we know, that theory says that in order to determine the value of a QM observable referring to the QM state $\psi(t_0)$, we must have at our disposal physical conditions that will make possible the noninstantaneous resolution at time $t > t_0$ of the (initial) state $\psi(t_0)$ into a set of wave packets that carry the necessary information by the objective position itself of the particle within one of them (position being the only observable whose measurement needs no special preparation in de Broglie's theory). This will happen (and happens indeed) at times and distances that are large enough and the idea in question finds its direct corroboration in the time-of-flight momentum measurements and in spin-component measurements via Stern-Gerlach set-ups (Sec. 2). In the time-of-flight momentum measurements we will generally have to wait until the $\psi(t_0)$ of interest would evolve at times $t > t_0$ into a form admitting the very definition of (macroscopic) momentum. In Stern-Gerlach spin $\frac{1}{2}$ measurements we would have to wait until $\psi(t_0)$ would fully disintegrate at times $t \gtrsim t_0 + T, T > 0$, into two well defined subpackets which - by their very existence - would demonstrate the existence of a two-valued spin characteristic of the particle. In the absence of any idea about the essence of the subquantum internal motions that generate the concept of spin (which possible motions would be of the competence of HVs theories) one could not (and should not) say anything about the possible values of spin components on axes that do not coincide with the one determined by the orientation of the Stern-Gerlach "magnetostat", and even about spin components on the Stern-Gerlach axis itself at intermediate times $t_0 < t \lesssim t_0 + T$. (Analogously, when a many-body system S possessing no definite temperature is put in contact with a thermostat whose temperature it T, the only thing that might generally be asserted about the temperature of S is that it will be $= T$ after a sufficiently long lapse of time, whereas at intermediate times S would not possess any temperature at all). Even in Bell-type situations with strictly correlated opposite spin-component values σ_{1z} and σ_{2z} of the two particles S_1 and S_2 along one and the same direction z the only thing that we could lawfully assert is that (macroscopic) measurement of, say, σ_{1z} will give information (without any physical influence on S_2) about the result of macroscopic *measurement* (if *performed*) of σ_{2z} in each individual case ; the present status of the theory does not admit more definite statements (say about the actual value of σ_{2z} or its very existence prior to its measurement ; cf. also our discussion of Stern-Gerlach spin measurements at the end of Sec. 2).

Contrary to this viewpoint - and in agreement with his cited basic assumption - Mermin's consideration ([40], Sec. VIII) aiming at the elimination of local HVs rests on the idea that an HV way of reasoning requires the actual existence of $\pm\frac{1}{2}$ spin component values for each electron at each moment of time along all possible directions in space. So his consideration is as successful as that of Bell in the elimination of such HVs.

The actual problem with the no-HVs arguments is now clear enough : these arguments offer certain assumptions that appear plausible to their authors ; at the same time, these arguments offer no proofs of the legitimate character of the assumptions made.

5. Conclusion

Quantum mechanics is a charming science which demonstrates the existence of delicate phenomena in the micro-world of a kind that was totally unfamiliar in older physics. Practically at the very time of its formulation, however, well known researchers announced the final character of quantum knowledge due to alleged characteristics of human cognition. Since that time things have dramatically changed : the CI is practically in the archives of physics nowadays and we are in a more or less normal period in the development of this science, when people simply apply its axiomatics to the solution of current problems. But the absence of a clear understanding of the physics underlying quantum phenomena will invariably be a challenge for visionaries as de Broglie whose thought cannot be arrested by inhibitions of any kind, the more so having in mind that inhibitions fall off sooner or later, but always at the right time when the right person would undertake the right step.

Acknowledgement. I am indebted to Professor R. Omnès for kindly supplying me with reprints of his cited works.

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(Manuscrit reçu le 31 août 2000)