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Towards a Separable "Empirical Reality"?

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"To be" or "to be found"? Some contributions relative to this modern variant of Hamlet's question are presented here. They aim at better apprehending the differences between the points of view of the physicists who consider that present-day quantum measurement theories do reach their objective and those who deny they do. It is pointed out that these two groups have different interpretations of the verbs "to be" and "to have" and of the criterion for truth. These differences are made explicit. A notion of "empirical reality" is constructed within the representation of which the physicists of the first named group can consistently uphold their claim. A detailed way of sharpening this definition so as to make empirical reality free of nonlocal actions at a distance is also described.

1. INTRODUCTION

Classical physics has trained us in considering that science can and does describe *things as they really are* (even if approximately only). It would be nice to know whether such a view can be maintained, at least concerning the entities we normally call "things," namely macroscopic objects. And, if so, at what price it can.

Some physicists, John Bell foremost, while they assert that this is possible, are quite specific about the price. The price, they say, is that we cannot be satisfied with standard quantum mechanics (though its *recipes* are quite good) and that we must switch over to some nonstandard model (supplementary variables models or nonlinear models). But others, who dislike such models, still give a positive answer to the question at hand for, they tell us, this heavy price need *not* be paid. Such is the view of many

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promoters of quantum measurement theories. These theorists claim, in particular, that some features characterizing the measurement process make it possible to solve the central problem worrying all the physicists interested in the question, namely the Schrödinger cat paradox and to do this within the realm of standard, linear textbook mechanics, with no "hidden variables" or nonlinear terms of any sort lurking around.

Cats being notoriously unpredictable, theorists used to consider instrument pointers instead. The Schrödinger cat paradox is then converted into the worrying observation that in the general case the density matrix ρ of the apparatus–system combination has appreciable nondiagonal elements even in the basis whose elements are eigenvectors of the apparatus observable. However, this holds true only as long as such a composite system may be considered as isolated, which actually is not the case since the apparatus is a macroscopic system. The matrix ρ is then obtained by tracing the system–apparatus–environment ($S+A+E$ for short) density matrix over the environment variables. According to the theorists we have in mind this is in fact the clue of the enigma; for, as Zurek and others have shown, the nondiagonal matrix elements of ρ then normally turn out to be very small. And, as Zurek puts it[1]:

[Quotation A]

When these are small, as is usually the case, the density matrix can be thought of as describing the apparatus in a definite state. The probabilities on the diagonal of the density matrix are there *because of our (i.e., the observer's) ignorance about the outcome of the measurement. It is, as yet, unknown to us but nevertheless it is definite* (our underlining); and:

[Quotation B]

...the interaction with the environment forces the system to be in one of the eigenstates of the pointer observable rather than in some arbitrary superposition of such eigenstates.

Apparently the message of Zurek (and many other measurement theorists) is clear: due to the environment–apparatus interaction (or the macroscopic nature of the apparatus, which amounts much to the same thing) the phase coherence between two eigenspaces of the pointer is being continuously destroyed. This interaction makes the phase between pointer–basis–states impossible to observe and the pointer then behaves "classically." According to the said message, conventional quantum mechanics (without hidden variables or nonlinear terms of any sort) has thus been shown to be compatible with the views that all cats are either alive or dead and that any pointer lies at (almost) any time in some definite graduation interval (the magic word "classical" has—at last!—been given a rational sense!). What a contrast with the views of Bohm, Bell, Pearle, and many others, who on the contrary consider that substantiation of the views in question is impossible within conventional quantum mechanics, that to speak of "classical systems" with no more justification than this is just a lure and that we *must* switch to nonstandard models!

In this paper our first objective is to try to make up our mind on the question which one of these two groups is "right." In this our analysis is only partial, however. It would obviously be impossible to consider all the published measurement theories in one article. For that reason (and for it only) we concentrate on one of them, namely that of Zurek, already cited. In Section 2 the essential points of Zurek's theory are briefly summarized. In Section 3 the correctness of a treatment that (as Quotations A and B seem to suggest) implicitly identifies the *improper* mixture described by ρ with a *proper* one is discussed both qualitatively and quantitatively. Our conclusion is negative. Within the usual, elementary, commonsense acceptation of the verbs "to be," "to have," and "to lie," Zurek's measurement theory (same as any other one for that matter) does *not* justify the views that the cat is either alive or dead and that the pointer always lies in some definite graduation interval. We agree with Bell that *within such an acceptation* of these words (which was the one taken for granted also in physics until the advent of quantum mechanics) a switch to some nonstandard model is necessary for obtaining the justification in question (a discussion of the criterion of truth used there is deferred to Section 5).

Our second objective—motivated by the fact that none of the nonstandard models seems to have convincing predictive value and that they all are, for this reason, somewhat suspect—is then to formulate an alternative acceptation of the verbs in question, and to formulate it with a degree of precision sufficient for differentiating it from the one within which the foregoing conclusion holds. This alternative acceptation has in fact been known, at least in its principle, for quite a long time; for "to be" means the same as "to really be," the substantive for "real" is "reality," and there exists a sense of the word "reality," well known to the philosophers, which identifies it just with the set of the *phenomena*, that is, to what "appears" or "can appear" to the collectivity of men. As elsewhere before,[2,3] I call this "type" of reality "empirical reality." If reality is identified with empirical reality, this implies giving to the verb "to be" (and related ones) a meaning significantly different from the "elementary, commonsense" one.

In Section 4 we bring together two difficulties that standard quantum theory has to face and that the concept of empirical reality should remove. One is the one already discussed above. We summarize in Sections 6 and 7 a way to remove it already described elsewhere.[3] The other one is the nonlocal problem. It consists in the fact that nonseparability is something very peculiar, which is not "operational" and which, accordingly, the textbooks on quantum field theory do not even need to mention. One may hesitate therefore in calling it a "phenomenon." However, excluding it from the set of the phenomena, as we propose to do here, means that empirical reality must be defined in such a way that it should be "separable." In Section 8 we put forward one precise way (not described in former papers) to achieve this.

2. SUMMARY OF ZUREK'S "BIT-BY-BIT" MEASUREMENT APPROACH

According to Zurek the role of the apparatus–environment interaction (Hamiltonian H^{AE} in his notations) is essential in two respects. It determines the so-called "pointer basis" and it makes the nondiagonal elements $\rho_{n,m}$ of ρ very small. Here we are only interested in the second aspect of the question. In order to explain simply the main points of his theory, Zurek begins by considering the case in which the observable to be measured is a dichotomic one, such as the z spin component S_z of a spin-1/2 particle S, and in which the apparatus coordinate G (the "pointer position") is also dichotomic (let its eigenvalues be $G_\pm$). For describing the measurement process he then introduces a hamiltonian H^{AS} operating only for a short time and describing the system–apparatus interaction then occurring. The H^{AS} action is schematically describable by the symbolds

$$|+\rangle \otimes |V_+\rangle \to |+\rangle \otimes |U_+\rangle \tag{1a}$$

$$|-\rangle \otimes |V_+\rangle \to |-\rangle \otimes |U_-\rangle \tag{1b}$$

where $|+\rangle$ ($|-\rangle$) is the eigenket of S_z corresponding to the eigenvalue $+1$ (-1), in units $\hbar/2$, where $|U_+\rangle(|U_-\rangle)$ (belonging to the Hilbert space of the apparatus A) is the eigenket corresponding to $G_+(G_-)$ and where

$$|V_\pm\rangle = 2^{-1/2}(|U_+\rangle \pm |U_-\rangle) \tag{2}$$

The interaction of S and A (measurement process) then changes the initial state

$$|\varphi_i\rangle = (a\,|+\rangle + b\,|-\rangle) \otimes |V_+\rangle \tag{3}$$

of the composite system into

$$|\varphi_f\rangle = a\,|+\rangle \otimes |U_+\rangle + b\,|-\rangle \otimes |U_-\rangle \tag{4}$$

and a strict correlation between the eigenvalues of S_z and those of G is thereby established.

Nevertheless, $|\varphi_f\rangle$ obviously is still a pure state and for well-known reasons an ensemble of N "$S+A$ systems" described by $|\varphi_f\rangle$ cannot be identified with a mixture of $|a|^2 N$ systems described by $|+\rangle \otimes |U_+\rangle$ (and therefore *having* $G = G_+$) and $|b|^2 N$ systems described by $|-\rangle \otimes |U_-\rangle$ (and therefore *having* $G = G_-$). For later reference let us make one of these reasons quite explicit. It is that here (contrary to what is the case concerning, for example, the isotopic spin Hilbert space) the linear combinations such as $|V_\pm\rangle$ of $|U_+\rangle$ and $|U_-\rangle$ and those such as $|\varphi_i\rangle$ of the $|i\rangle \otimes |U_j\rangle$ $(i, j = +, -)$ cannot be considered as being physically meaningless since $|V_+\rangle$ is the initial state of the apparatus and $|\varphi_i\rangle$ is the initial state of the composite system. This can also be formulated by saying that the hermitian operators which have such linear combinations as eigenvectors are observable, which in turn implies that their mean values on state $|\varphi_f\rangle$ are observable also. Since these mean values imply cross-terms (with coefficients a^*b, b^*a) that are not present when $|\varphi_f\rangle$ is replaced by the considered mixture, it is clear that such a replacement would lead to erroneous predictions that could, at least in principle, be detected.

But Zurek points out that the whole outlook changes very much if we take, as we should, the apparatus–environment interaction into consideration. In order to show this in a simple way, he considers a simplified interaction of this type, starting at time $t = 0$ (just after the S, A interaction has ceased) and described by Hamiltonian

$$H^{AE} = \sum_{k=1}^{N} H_k^{AE} \tag{5}$$

with

$$H_k^{AE} = g_k(|U_+\rangle\langle U_+| - |U_-\rangle\langle U_-|) \otimes (|u_+\rangle\langle u_+| - |u_-\rangle\langle u_-|)_k \prod_{j \neq k} \otimes 1_j \tag{6}$$

Here the environment consists of N two-states systems, the kth of which has a two-dimensional Hilbert space $\mathscr{H}_k$ spanned by the basis $\{|u_+\rangle_k, |u_-\rangle_k\}$. For simplicity it is also assumed that the free hamiltonians of the system, the apparatus, and the environment are all zero.

If at time $t = 0$ the state of the combined system–apparatus–environment is

$$|\Phi(0)\rangle = |\varphi_f\rangle \prod_{k=1}^{N} \otimes [\alpha_k |u_+\rangle_k + \beta_k |u_-\rangle_k] \tag{7}$$

it is then immediately shown that at time t this state becomes

$$|\Phi(t)\rangle = a\,|s_+\rangle \prod_k \otimes \left[\alpha_k e^{ig_k t}\,|u_+\rangle_k + \beta_k e^{-ig_k t}\,|u_-\rangle_k\right]$$
$$+ b\,|s_-\rangle \prod_k \otimes \left[\alpha_k e^{-ig_k t}\,|u_+\rangle_k + \beta_k e^{ig_k t}\,|u_-\rangle_k\right] \tag{8}$$

where

$$|s_\pm\rangle = |\pm\rangle \otimes |U_\pm\rangle \tag{9}$$

The density matrix of the S, A system is then obtained by tracing over the environment Hilbert space and turns out to be

$$\rho = |a|^2\,|s_+\rangle\langle s_+| + |b|^2\,|s_-\rangle\langle s_-|$$
$$+ z(t)\,ab^*\,|s_+\rangle\langle s_-| + z^*(t)\,a^*b\,|s_-\rangle\langle s_+| \tag{10}$$

where

$$z(t) = \prod_{k=1}^{N} \left[\cos 2g_k t + i(|\alpha_k|^2 - |\beta_k|^2)\sin 2g_k t\right] \tag{11}$$

With N large and the coupling constants g_k chosen at random, $z(t)$ soon becomes quite small and remains small a long time thereafter, which means that a small time τ after the system–apparatus interaction has ceased the nondiagonal matrix elements of ρ have themselves become very small. The conditions considered in Quotation A (see Introduction) are thereby met. As we have seen (second part of Quotation A) Zurek then considers it as legitimate to think of the apparatus pointer as lying in either one of the two eigenstates of the corresponding observable. It is true, of course, that, as formula (11) shows, with N finite $|z(t)|$ will return arbitrarily closely to 1 at certain times. But, if T_ε is the time it takes for $|z(t)|$ to reach the value $1-\varepsilon$ again, Zurek points out that with N large this T_ε should normally be extremely long, comparable indeed to the Poincaré recurrence times, and possibly longer, for macroscopic environments, than the age of the Universe. Between τ and T_ε the standard deviation of $z(t)$ from its average value 0 turns out to be of the order of $N^{-1/2}$ only. Under such conditions it is clear that the difference between an ensemble of $S+A$ systems described by ρ and an ensemble of such systems described by $\rho' = \mathrm{Diag}\,\rho$ (same diagonal elements as ρ and zero nondiagonal ones) is practically unobservable.

3. DISCUSSION

Zurek's own interpretation—as expressed by Quotation A—of his —quite uncontroversial—results is supported by a powerful argument which is that, due to the extreme smallness of $z(t)$, no measurement made after time τ on *any* of the physical quantities attached to the $S+A$ system can in practice contradict his assertion that the pointer always *is* in either one of its two possible states instead of in a superposition of both, even if such a measurement is repeated on a whole ensemble of such systems.

On the other hand the restriction "in practice" appearing in the foregoing conclusion may be considered by some theorists as constituting an indication that the matter at hand is not as yet completely settled. We shall return to this below. However, before we do this it seems appropriate that we should expand somewhat more on the physical aspects of the problem.

For this purpose it is advisable to start with the difference which is known to exist[4] between proper and improper mixtures. This is best illustrated by the convenient and often used example of an ensemble $\mathscr{E}$ of N spin-1/2 particle pairs U and V lying in a singlet spin state. In such a case the ensemble E_U of, say, all the N particles U is described by the density matrix

$$\frac{1}{2}\begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix} \tag{12}$$

and the same is true concerning the ensemble E_V of the N particles V. Now, in spite of the fact that this density matrix is *just the one* which describes an ensemble $\hat{E}_U$ of U particles (and also an ensemble $\hat{E}_V$ of V particles) composed of $N/2$ particles having their z component S_z of spin equal to $+1$ (in $\hbar/2$ units) and $N/2$ particles having their S_z equal to -1, still it is impossible to physically identify E_U to $\hat{E}_U$ and E_V to $\hat{E}_V$. The reason is that by assumption every element of $\hat{E}_U$ has a definite value ± 1 of its S_z and so has every element of $\hat{E}_V$; so that if a pair $U+V$ is composed of one element of $\hat{E}_U$ and one element of $\hat{E}_V$ it has (in obvious notations and as a consequence of the very definition of $\hat{E}_U$ and $\hat{E}_V$) either its $S_z^U=+1$ and its $S_z^V=-1$ or its $S_z^U=-1$ and its $S_z^V=+1$ (the other two cases, $++$ and $--$, being trivially ruled out by their false experimental consequences). Let us then consider the ensemble E_+ of the pairs having $S_z^U=+1$ and $S_z^V=-1$. Since (a) by assumption, there are no hidden variables and (b) the set $\{S_z^U, S_z^V\}$ constitutes a *C.S.C.O.* (complete set of compatible observables) within the four-dimensional Hilbert space $\mathscr{H}_U \otimes \mathscr{H}_V$ of the two spins, E_+ cannot differ in any way (how could it?)

from the ensemble $\mathscr{E}_+$ we would have obtained by measuring simultaneously S_z^U and S_z^V on a very large ensemble of (arbitrarily prepared) U, V pairs, selecting the pairs for which the results are $+1$ and -1 respectively and picking out $N/2$ of these. The same argument holds concerning the complementary E_- ensemble ($S_z^U = -1$, $S_z^V = +1$) and the corresponding $\mathscr{E}_-$. This shows that the considered identification (of E_U with $\hat{E}_U$ and E_V to $\hat{E}_V$) would necessarily imply that of $\mathscr{E}$ with the mixture, in equal proportions, of ensembles $\mathscr{E}_+$ and $\mathscr{E}_-$. However, in $\mathscr{H}_U \otimes \mathscr{H}_V$ these two ensembles are described by density matrices that are not only different but even *testably* different in general. Hence the assumption that E_U is physically identical to $\hat{E}_U$ (and E_V to $\hat{E}_V$) has been shown to have a false consequence. It is thereby falsified.

Ensembles such as $\hat{E}_U$ or $\hat{E}_V$ are called *proper*, or *first kind*, mixtures. Those such as E_U or E_V are called *improper mixtures* or *mixtures of the second kind.* To identify improper mixtures with proper ones is definitely illegitimate, at least whenever the difference between the two above-mentioned density matrices has consequences that are in principle observable.[2]

In the problem we are considering, the ensemble of the $S+A$ systems and the ensemble of the corresponding environments, considered at any time in between τ and T_ε, are both, clearly, mixtures of the second kind[3] since these $S+A$ systems and the corresponding environments are parts of the larger $S+A+E$ systems which at any time t are in the pure state $\Phi(t)$ (and on which measurements involving both $S+A$ and E could also be made in principle). Hence, even if $z(t)$ had turned out to be strictly zero, the development summarized in Section 2 would not by itself have been conclusive. The interpretation summarized by Quotation A could even then have been questioned on the basis of the foregoing considerations, at least as long as no investigation of the order of magnitude of possible observable correlations between $S+A$ and the environment has been done. A study of

[2] The relevance of the distinction between proper and improper mixture has been questioned,[(5)] essentially on the ground that, strictly speaking, if the existence of some systems having classical properties and utilizable as apparatuses is not postulated at the start, proper mixtures cannot really be created. The answer to this is that, admittedly, if the concept of a proper mixture is void, there can be no use in introducing it. But, on the other hand, if it *is* really considered as void, then this entails that the statement according to which immediately after an observable has been measured (first-kind measurement in the sense of Pauli) it *has* the observed value is also a void statement, unless we weaken the meaning of the verb "to have" much in the manner explained here in Section 7. The argument based on the difference between proper and improper mixtures is conceived for the benefit of the physicists who would like to consider that the statement in question is *not* void, with the expression "it *has*" having its ordinary, commonsense meaning.

[3] So are also the ensembles of the S systems and of the apparatus A in the above-considered case, in which the environment is not taken into account.

this order of magnitude is therefore quite necessary. If some *observable* associated with S, A and the environment turns out to have a probability or mean value whose expression contains appreciable a^*b terms even for large t, this will be an (additional) indication that the matter must be more thoroughly thought over. If, on the contrary, no such observable can be found, our confidence that the approach described in Section 2 can be meaningful will be increased.

Above, the word *observable* is italicized because it is the key word here. Of course we know quite well that formally a pure state such as $|\Phi(t)\rangle$ cannot be identified with any mixture in the mathematical sense and that therefore there must exist Hermitian operators such that their mean values on $|\Phi(t)\rangle$ differ from the ones they take on any mixture, and in particular on those on the elements of which G takes up definite values. But clearly, if these Hermitian operators corresponded to no observables such differences could be just as void of physical significance as is, for example, the phase of the ket describing the overall state of a system.

In respect to environment the model considered in Section 2 is of course—as compared with any actually existing environment—a highly simplified one. Still, Zurek has shown that it leads to quite useful information, and a substantial part of his argumentation is based on it. It seems therefore appropriate that we also should take it seriously and use it.

As Eq. (5) shows, in this model the environment is made up of N systems, all quite similar to apparatus A in that each of them is described by a two-dimensional Hilbert space. Since we remarked (Section 2) that the Hermitian operators having such linear combinations as $2^{-1/2}(|U_+\rangle \pm |U_-\rangle)$ as eigenvectors *do* correspond to observables, the postulate that the Hermitian operator associated to the kth system and having the linear combinations $2^{-1/2}(|u_+\rangle_k \pm |u_-\rangle_k)$ as eigenvectors do *not* correspond to an observable would constitute quite an artificial restrictive assumption. Let then the kth system be such that

$$|v_\pm\rangle_k = 2^{-1/2}(|u_+\rangle_k \pm |u_-\rangle_k) \tag{13}$$

are eigenkets of an *observable* which we call B_k of the system in question. Without loss of generality we may choose B_k as

$$B_k = 2^{-1}(|v_+\rangle\langle v_+| - |v_-\rangle\langle v_-|)_k \tag{14}$$

with the help of these B_k we may then define an environment observable by

$$B = \prod_{k=1}^{N} \otimes B_k \tag{15}$$

In the Hilbert space of the environment system the operator B is then identical to the one that would represent the product $\Pi_k \otimes s_{kx}$ of the x components, s_{kx}, of an array of spin-1/2 particles whose s_z component would be

$$s_{k_z} = 2^{-1}(|u_+\rangle\langle u_+| - |u_-\rangle\langle u_-|)_k \tag{16}$$

Similarly, let D be the (above-mentioned) apparatus observable having $2^{-1/2}(|U_+\rangle \pm |U_-\rangle)$ as eigenvectors and which, without loss of generality, we may take as

$$D = 2^{-1}(|V_+\rangle\langle V_+| - |V_-\rangle\langle V_-|) \tag{17}$$

In the apparatus Hilbert space D is then identical to the $\hat{S}_x$ operator of a fictitious spin $\hat{S}$ such that

$$\hat{S}_z = 2^{-1}(|U_+\rangle\langle U_+| - |U_-\rangle\langle U_-|) \tag{18}$$

With these notations H_k^{AE} can be written as

$$H_k^{AE} = 4g_k \hat{S}_z \otimes s_{kz} \prod_{j \neq k} \otimes 1_j \tag{19}$$

and we have

$$[D \otimes B, H^{AE}] = \sum_k [D \otimes B, H_k^{AE}] \tag{20}$$

with

$$\begin{aligned}[D \otimes B, H_1^{AE}] &= g_1 \left[\hat{S}_x \otimes s_{1x} \otimes \cdots \otimes s_{Nx}, \hat{S}_z \otimes s_{1z} \prod_{j \neq 1} \otimes 1_j \right] \\ &= g_1((-i\hat{S}_y) \otimes (-is_{1y}) - (i\hat{S}_y) \otimes (is_{1y})) \otimes s_{2x} \otimes \cdots s_{Nx} = 0\end{aligned} \tag{21}$$

and similarly for $[D \otimes B, H_k^{AE}]$ with $k = 2, \ldots, N$.

The observable $D \otimes B$ thus commutes, for all times $t \geqslant 0$, with the total Hamiltonian. The same is then true of any observable such as $S_{\mathbf{n}} \otimes D \otimes B$, where S_N is the component along unit vector $\mathbf{n}$ of the spin of the measured particle S, and in particular of

$$M = S_x \otimes D \otimes B \tag{22}$$

M is therefore a constant of the motion. Its mean value $\langle M \rangle$ remains, at any time $t \geqslant 0$, equal to its value at $t=0$ (i.e., just after the system apparatus interaction has ceased), which is

$$\begin{aligned}
\langle M \rangle &= \langle \Phi(t=0) |\, S_x \otimes \hat{S}_x \otimes s_{1x} \otimes \cdots \otimes s_{Nx} \,| \Phi(t=0) \rangle \\
&= (a \langle + | \otimes \langle U_+ | + b \langle - | \otimes \langle U_- |) \\
&\quad \cdot S_x \otimes \hat{S}_x (a \,| + \rangle \otimes | U_+ \rangle + b \,| - \rangle \otimes | U_- \rangle) \\
&\quad \cdot \prod_k \,(\alpha < u_+ | + \beta < u_- |)_k \, s_{kx} (\alpha \,| u_+ \rangle + \beta \,| u_- \rangle)_k \\
&= 2^{-3} (a^* b + b^* a) \prod_k \,(\alpha_k^* \beta_k + \beta_k^* \alpha_k)
\end{aligned} \tag{23}$$

According to the rules of quantum mechanics, if an experimentalist measures $\langle M \rangle$ at any time $t > 0$, he will get value (23). This value is altogether different from the value zero he would expect to get if he assumed that at the time t just prior to his measurement the pointer of the instrument is in a definite state (which should then be either $| U_+ \rangle$, with probability $|a|^2$, or $| U_- \rangle$, with probability $|b|^2$).

Note that this is but a straightforward generalization of the argument described at the end of Section 2 concerning the elementary case of an *isolated* apparatus. The difference is only that the roles played there by systems S and A are here played by $S + A$ and the environment, respectively.

Note also that instead of M we could have considered any observable of the form

$$M_{\mathbf{p},\mathbf{q},\mathbf{r}} = \mathbf{S} \cdot \mathbf{p} \otimes \hat{\mathbf{S}} \cdot \mathbf{q} \otimes \prod_{k=1}^{N} \otimes \mathbf{s}_k \cdot \mathbf{r}_k \tag{24}$$

where $\mathbf{p}$, $\mathbf{q}$, $\mathbf{r}_1, \ldots, \mathbf{r}_N$ are unit vectors. The mean value $\langle M_{\mathbf{p},\mathbf{q},\mathbf{r}} \rangle$ of such an observable is not a constant, but direct calculation shows that it is the sum of a term qualitatively behaving like $z(t)$ (and thus going fast to zero if N is large, see above) and of a constant term:

$$\begin{aligned}
&p_z q_z (|a|^2 + |b|^2) \prod_k r_{kz} (|\alpha_k|^2 - |\beta_k|^2) \\
&\quad + \left[a^* b p_+ q_+ \prod_k \,(\alpha_k^* \beta_k r_{k+} + \alpha_k \beta_k^* r_{k-}) + \text{c.c.} \right]
\end{aligned} \tag{25}$$

where $n_\pm = n_x \pm i n_y$; $\mathbf{n} = \mathbf{p}, \mathbf{q}, \mathbf{r}$.

Here again, a calculation based on taking at its face value the statement of *Quotation A* would obviously lead for $\langle M_{\mathbf{p},\mathbf{q},\mathbf{r}} \rangle$ to an expression from which the term inside the square bracket in formula (25) would be absent. However, for any observable $M_{\mathbf{p},\mathbf{q},\mathbf{r}}$ this term vanishes only if one or more of the $p_{\pm}$, $q_{\pm}$, $r_{k\pm}$ is zero. Otherwise it can be quite appreciable.

Finally, it may also be noted that instead of mean values we could have considered probabilities. The calculations are trivial and are not reproduced here. Of course the same kind of discrepancy (presence versus absence of a^*b terms) between the correct result and the one based on Quotation A reappears there.

As already noted, the model considered here is admittedly a crude one. Actual apparatus–environment interactions are considerably more complex. It may, however, be expected that the corresponding H^{AE} do not by themselves constitute C.S.C.O.'s for the $S+A+E$ Hilbert space, in which cases observables similar to the M's above are likely to exist.[4] Admittedly they will be even more difficult to measure. But it may be felt that arguments based on such notions as practical inextricability, lack of appropriate time for performing a measurement, and so on are not really removing a difficulty that seems to be of a more fundamental nature. To repeat: it is true of course that not all of the mathematical symbols present in a physical theory necessarily correspond to physically existing features of the investigated systems, as the counterexample of the overall phase of the ket describing a system clearly shows. But M, and similarly definable entities, are different. As we saw, their definition is such that to deny them the status of observables sounds extremely artificial. In the case in which N is small (of the order, say, of a few units) M is unquestionably an observable. The frequencies of the cases in which the values $M=+1$ and $M=-1$ are predicted can be effectively measured by subtle combinations of Stern–Gerlach-like instruments. It is quite true, of course, that as soon as N becomes large (and for the model to approximately simulate macroscopic environment N would have to be of the order of the Avogadro number...) the complexity of such measurements defies imagination. Still, for any finite N it remains conceptually possible.

On this last point (complexity of measurements) the situation here is partly similar to the one which prevails in classical statistical mechanics, where a simultaneous measurement of the coordinates and velocities of, say, all the molecules of a cubic centimeter of a gas is obviously quite

[4] Let K be such an observable, that is to say, let K commute with H^{AE} or, more generally, with the complete Hamiltonian H of the composite system including the environment. Then of course we may consider a new basis, composed of eigenvectors common to H and K. However, these eigenvectors will in general be superpositions in which apparatus kets (corresponding to different G values) are entangled with environment kets, which deprives this new basis of any clearcut physical meaning.

impossible in practice. A great difference, however, is that in the latter case a nonperturbative measurement of such a kind, assuming it were made nevertheless, would provide results *compatible* with our description of the state of the gas previous to that detailed measurement. In the present case, on the contrary, our basic assumption that the rules of prediction of quantum mechanics are correct and universal leads, as we saw, to the conclusion that the result of the measurement of M on an ensemble would *falsify* the description of the system summarized by Quotation A of Zurek (a very similar argumentation can be carried out without considering measurements of correlations involving the environment if account is taken of the fact that for finite times, z [Eq. (11)] is not strictly zero).

All this definitely shows that the conclusions of Zurek (summarized here by Quotation A and/or Quotation B) and of other proponents of measurement theories are difficult to accept for anybody who attributes unreserved validity to a criterion of truth" to which, for later reference, we give the name *criterion A* and which is simply this:

Criterion A

A statement can only be true if all the consequences that can be correctly (i.e., by means of a valid theory) derived from it are true, including those which concern some future time t and bear on measurements that *could be conceived* being made at t, by whatever means, however complex.

On the other hand, notwithstanding its apparent necessity criterion A is worth some comments. These will be presented in Section 5.

4. TAKING THE NONLOCALITY PROBLEM INTO ACCOUNT

As mentioned in the Introduction the view that, due to the apparatus–environment interaction, the pointer of the apparatus can be said to "really be" in either one of its two (in the example) possible states meets not only with the above-considered difficulty—which for later reference we call the "local" one—but also with another one, at least in the case in which the measured system S is, quantum-mechanically, correlated with another one S' that undergoes a measurement at some distant place. If, for example, S and S' are two spin-1/2 particles coming out from a source in a singlet state and if the two measured quantities are their z spin components S_z and S'_z, then, since the results of the two measurements are correlated and since neither of them is predetermined at the source (no hidden variables) it, apparently, must be the case that the event E: "the pointer of apparatus A goes over to a certain definite state upon measurement of S_z" is what *induces* at a distance the event E': "the pointer of the other apparatus goes over to the corresponding state" (or conversely). This "nonseparability" effect—which we may call the "nonlocal" difficulty—is well known and has been made the subject of much discussion. It is true that it cannot be made use of in order to send *signals* or induce responses and that it can therefore hardly be considered as fully belonging to the realm of *causation* as we normally understand this word. Still, it exists and within any conventionally realistic outlook on matter its existence should somehow be accounted for.

For the realist the existence of these two difficulties, the "local" and the "nonlocal" ones, is a challenge. Are we, because of it, in a deadlock? As mentioned in the Introduction some physicists consider that indeed we are, except if we agree to go over to nonstandard, nonlocal models with "hidden" variables or nonlinear terms. To be specific (for the word "realist" has several meanings), let these physicists be called "physical realists." Physical realists may well be right. There are powerful arguments in favor of their conception. In Section 6, 7, and 8 I shall nevertheless try to argue that a rational alternative approach exists to the problems under investigation here.

This other approach implies, in particular, some weakening of the above-stated criterion A. But before we take a detailed look at how this can be done, it is proper that we should wonder whether, by any chance, we could renounce this criterion completely. This is the subject of the next section.

5. QUESTIONING THE "NECESSARY CRITERION OF TRUTH" (CRITERION A)

The validity of the above-stated criterion A is not as obvious as it seems. The main reason is that the word "consequence," which it uses, can be understood in different ways. It must be realized that in its most usual acceptation this word carries a notion of counterfactuality which, in turn, is based on that of free choice. It is because he implicitly has in mind the experimentalist's freedom to make some further measurements on the $S+A$ system *after* S and A have interacted, and to choose at whim which quantity he then cares to measure (provided that it is an observable, of course), that—to nevertheless be able to describe the pointers as always being in some definite graduation interval—Zurek must require the non-diagonal matrix elements of ρ to be vanishingly small. Similarly our questioning of the validity of Zurek's assertions on these matters rests on the—implicitly postulated—experimentalist's freedom to measure observables such as M above, involving the environment along with S and A. Clearly the reason why criterion A is relevant concerning such a questioning is that it uses the word *consequence* taken in this counterfactual acceptation. Now, the notion of *free choice* is, at least in our elementary conceptions, foreign to the domain of physics proper, so that we may see it as nonabsurd that some theoretical developments simply ignore criterion A.

Another reason on the basis of which the relevance of criterion A to physics could be questioned is that, at least as it is used in contexts such as this one, it implicitly refers to an assumed qualitative difference between the past and the future, namely that the past is fixed once and for all, while the future is still "open." Indeed, in all the foregoing developments it was assumed as a matter of course that making a measurement on the $S+A$ system *after* the interaction between S and A has taken place could not change anything to what happened during this interaction. Again, such a distinction between past and future is foreign to the basic laws of physics so that we may see it as nonabsurd that some theories should reject it.

If we keep such ideas in mind we may consider with renewed curiosity some theories based on ideas radically differing from those on which developments such as the one analyzed here are based. For example, we may be interested in Wheeler's delayed choice theory, which denies our inability to change the past, or in conceptions such as those of Griffiths and Omnès, in which, indeed, something similar takes place since, in them, the existence or nonexistence of a definite experimental setup that will interact with a microscopic system S at t has decisive influence on the validity or nonvalidity at a time $t' < t$ of a proposition concerning S (not only on the possibility we have of knowing whether it is true or not, which would be trivial, but on its validity as such, i.e., on any notion that can meaningfully be attached to the word "validity"). Such a conception is closely connected with the interpretation of Niels Bohr's views which asserts that if a property of a system is to be measured by some experimental setup at a time t this property can and should conventionally be attributed to the system before time t, it being understood that if we change the nature of the setup we must change also the nature of the property.[2]

These theories will not be examined in this paper. But the very fact that they could be developed and given appreciable—even if not full—consistency well illustrates the above-made remark that, in spite of appearances, criterion A is not the unescapable touchstone of a physical theory. On the other hand, a violation of the rule that we cannot in any way alter the past would mean that whatever "really exists" is incredibly different from the set of the "physical phenomena" since, clearly, there is a sense in which we cannot change the *phenomena* of the past. Similarly, the idea that it just makes no sense to *leave open* what measurement we shall perform in one hour time on a presently existing system seems to contradict something quite basic in our conception of empirical science.[5] This means that radically renouncing criterion A might eventually lead us to views that would partake more of metaphysics than physics. Consequently, we shall keep here to the general idea expressed by criterion A, even though the necessity of not contradicting actual knowledge will make it necessary to soften it, in manners and context that are made explicit below.

6. IF TWO DISTINCT NOTIONS, THEN A NAME FOR EACH ONE

Up to this point we subscribed to the assumption that science describes—or should describe—"things as they really are." This means we considered, as most classical physicists did in the past, that reason and science, and, in particular, physics, are able to remove—in principle com-

[5] Concerning this particular point, a recent theoretical proposal by M. Gell-Mann and J. B. Hartle[(6)] also seems to raise questions. These authors put forward a theory one of whose main purposes is to assign definite probabilities to alternative histories of the Universe. More precisely they point out that special sets of histories, which they call "decoherent sets," *can* be assigned probabilities; and they claim that "decoherence" replaces, in that role, the notion of "measurement," central to the Copenhagen interpretations. This seems to imply that the thus-assigned probabilities are objective ones. On the other hand, they also point out that decoherence is automatic for histories that consist of alternatives at but one time. This means that, if a system was, at time $t=0$, lying in a certain state ρ (for example, in a pure state $|\psi\rangle$) and if we consider the set k of histories $\{\hat{P}^k_\alpha(t)\}$ at some time $t>0$ ($\hat{P}^k_\alpha$ are projectors, k labels the "question" and α the particular alternative), the elements α of this set k can each be assigned a probability, namely

$$P^k_\alpha = \mathrm{Tr}[\hat{P}^k_\alpha(t), \rho]$$

Now, if these probabilities were really fully objective, in the usual sense of the word, this would have the physical meaning that when a whole ensemble of $\mathcal{N}$ similarly prepared systems is considered there must *be* in it, at time t, a number roughly equal to $\mathcal{N}P^k_\alpha$ of systems *having* property α. For example, in the case where the systems are spin-1/2 particles prepared in a state with $S_x = +1$ and if $\hat{P}^z_\alpha$ ($\alpha = \pm 1$) is the projector onto state $S_z = \alpha$ this would mean that at time t the ensemble is objectively a proper mixture of roughly $\mathcal{N}/2$ particles having $S_z = +1$ and $\mathcal{N}/2$ particles having $S_z = -1$. The usual argument for rejecting this description is of course, as mentioned above, that it leads to false predictions concerning the measurement of any observable other than S_z, an argument that implicitly uses criterion A. Hence we may perhaps conjecture that, although the authors did not mention it explicitly, what they actually have in mind is a rejection of criterion A. If not, then the necessity of interpreting P^z_α as the (intersubjective) probability that "if a *measurement* of S_z is made the result $S_z = \alpha$ has a probability P^z_α to be *found*" seems inescapable.

pletely—what has been called "the veil of the appearances." In other words we assumed that when reason shows the "broken stick" to actually be a straight one, and when science shows the (qualitative) color differences to actually be (quantitative) frequency ones, both reason and science reveal to us the real nature of what exists, quite independently of our modes of perception. For later reference let us call this assumption "physical realism." We assumed physical realism. By considering quantum measurement theories (with special attention paid to that of Zurek) we then observed that it is difficult (to say the least) to reconcile standard quantum mechanics (no hidden variables, no nonlinear terms), assumed universally valid, with physical realism. Basically the difficulty we encountered when trying to do so can be expressed by noting that physical realism, when considered as true, prevents us from adopting any *mitigated form* of the criterion of truth called criterion A above: either we reject this criterion completely (attempts in this direction exist, see the analysis of Section 5) or, if we accept it (and we have seen in that section why this seems to be by far the most reasonable standpoint), we must accept it without reservations. In particular we are not allowed to restrict the class of the measurements it considers to those that can "practically be done". In the last analysis this would mean making the borderline between what is real and what is unreal depend on what the human community can do, and this would obviously contradict the assumption of physical realism, the truth of which we are postulating. But then we must observe that (as shown in Section 3) Zurek can substantiate his basic claims (see Quotations A and B, Section 1) only by actually *making* such reservations (linked to the practical impossibility of measuring such and such observables). We can therefore conclude in a clearcut way: within the assumption of physical realism it is the "school of thought" here emblematically represented by Bell which is right. And the one symbolically represented by Zurek is wrong.

Shall we, then, switch to some nonstandard theory such as the pilot wave theory or the Ghirardi–Rimini–Weber theory[7]? Not necessarily, for another "way out" exists. It stems from the fact that physical realism is *not* the only conception of what we—vaguely to be sure—call "nature" that is compatible with rationality and even *scientific* rationality.

Strangely enough, this fact has been known for a very long time. From the beginning of the last century on, the philosophers have been teaching that what is called here "physical realism" is neither an obvious truth nor even a view supported by really weighty arguments. Indeed, notwithstanding the opinion of most scientists that physical realism still *is* the most rational view, many, perhaps most, philosophers take a standpoint that is quite *opposite* to it. They say, for example, that it is we who "construct" the objects. And such scientists as Henri Poincaré[8]—whose abilities at discriminating what is rational from what is not the scientists seldom question—have adopted quite similar conceptions.[6]

The reason why, up to the present time, no philosophically oriented discussion took place between "pro-physical realism" and "anti-physical realism" scientists, is just that the "anti" could (and did) for a long time supplement their standpoint with what may be considered as a corrective. This corrective—which has been called[(10)] the "full elision of the subject"—was the idea that although, in their views, the subject, the collective "we," is prior to ("creates") the object and not vice-versa, still the "rule of the game" of this same subject is to withdraw *entirely* from the picture of which he is the true author. In the times of classical physics such a principle was fully applicable so that, when discussing scientific matters with a "pro-", an "anti-" could keep completely silent about his philosophical option. If by chance his option nevertheless came to the knowledge of the "realist," the latter could, in turn, consider it as a queer idiosyncrasy, with no possible bearing on a scientific discussion, and which good taste therefore recommended not to allude to. In no field could the notion of gentlemen's agreement better apply....

This situation has now changed. Considerations such as those of Section 3 and similar ones found elsewhere[7] definitely show as we just said that criterion *A* can no longer be applied without some reference being made to the limitation of the possibilities of mankind. This means that if we stick to standard quantum mechanics (barring out nonstandard models) the "elision of the subject" cannot now, by any means, be carried as far as it could in the time of classical mechanics. As a consequence, discretion and good taste are no longer sufficient for reconciling the two points of view just discussed. In particular, we cannot, as we said, consider that the quantum measurement theories of Zurek and others fit with physical realism; so that if these theories are yet to be viewed as significant (and they obviously *are* significant in some sense) this can only be by taking advantage of the fact that, on the other hand, they fit quite well with the conceptions of the—old, respectable, and most serious—philosophical standpoint that rejects physical realism... and by associating them with it.

Such a view is most probably what Zurek and many others have implicitly in mind but, strangely enough, the idea is seldom articulated (it

[6] In fact this now seems to be the case even of Zurek. In one of his later articles[(9)] this author goes as far in this direction as to take Bishop Berkeley's basic axiom *esse est percipi* (to be is to be perceived) as a motto. This means at least that we would not form a faithful idea of Zurek's *present-day* standpoint concerning the interpretation to be given to his mathematical developments if we took the above-cited quotations *A* and *B* (taken from his 1982 paper) at face value.

[7] See, in particular, Ref. 3.

is now by Zurek; see footnote 6). This is a pity since once the idea has been made explicit we soon discover that expressing it in such a qualitative and vague way is not sufficient. Above, it was observed that a conception centered on the quotations *A* and *B* of Zurek has to face *two* quite definite difficulties. We must investigate in detail whether and how these difficulties can be coped with by a theory that would give up the, perhaps unattainable, goal of describing reality *per se* but that would explicitly describe, all the same, a "reality," it being understood that the concept of this "reality" cannot—in view of the foregoing—coincide with the one taken as referent by physical realism.

Our first step in this direction shall, of course, be to acknowledge that since these two concepts *must* differ from one another, their names also should differ. I propose to keep here to a vocabulary already made use of in other papers and to call "independent reality" the referent the "physical realists" have in mind and "empirical reality" the one borne in mind by the physicists who consider Zurek's or other authors' measurement theories as actually solving the measurement problem. For brevity's sake, the question of knowing whether the "independent reality" concept makes sense (I think it does) will hardly be touched upon in this paper. Rather we shall focus on the empirical reality concept.

7. EMPIRICAL REALITY AND THE "LOCAL" DIFFICULTY

According to the general guiding ideas developed in the last section, empirical reality should be defined by closely referring to the phenomena in the philosophical (and etymological...) sense of this word (which, schematically, is something like "appearances about which everybody agrees"). One simple and safe way to proceed along these lines would be to define empirical reality in a strictly operational way, that is, to identify this concept with a mere set of predictive rules, asserting what will be observed in such and such situations. There is much to say in favor of such a standpoint, which I more or less implicitly adopted in several previous writings. But it also has its deficiencies, the main one being made apparent by the very terminology, and more precisely by the need we are in of still using the word "reality." Neither science nor everyday experience is normally expressed in terms of mere "rules of prediction" and it would be most inconvenient, and perhaps even impossible, to translate them quite systematically in such a language. In fact, an overwhelming majority of the statements expressing them are couched in a "realist" language, that is, in a language that makes free use of such phrases as "to be in such and such a state," "to have such and such a property," "to lie in such an such a graduation interval," and so on, without explicitly mentioning our (collective) act of apprehension. It is therefore desirable that the notion of empirical reality should be defined in such a way that a language of this type should apply to it.

Concerning the "local" measurement problem, the one that conventional quantum measurement theories (including Zurek's) deal with, I have already indicated elsewere[3] what I consider as being an appropriate way of doing this. It consists in making one or two *conventions* (and in fact these conventions *are* actually made by the authors of measurement theories, but without, as a rule, making them explicit and noting that they are just conventions). They are either (a) that replacing very large times by infinite times and/or very large particle numbers by infinite numbers is a valid abstraction or (b) that the possibility of measuring observables exceeding a certain degree of complexity is to be considered as nonexistent also in matters of principle, even though a procedure for their measurement can be unambiguously defined and even though the only way we have of making their nonmeasurability compatible with quantum mechanics seems to be to ascribe it to some basic inaptitude of men. Of course within physical realism such conventions would be inconsistent: a finite number is not infinite, and so on. But remember that we are constructing a notion of "reality" which is *not* the "independent reality" physical realism refers to, and remember that we granted at the start that this notion of reality would have to involve some reference to man. To this reality we can still apply the verbs "to be," "to have," and so on, but (see Ref. 3) in a sense that, it must be granted, is a weakened one, since in some cases such as the ones here considered in Section 3 some measurements that are *not* ruled off by any axiom or theorem would falsify what we then say.

Remark: From "and" to "or"

Within the standard formulation of quantum mechanics the problem of understanding the transition from the "and" to the "or"—from the quantum superposition of two macroscopically different quantum states ψ_1 and ψ_2 to the actual presence of the system in *either* ψ_1 *or* ψ_2—is a deeply worrying one and it has been forcefully stressed by John Bell[11] that it definitely does *not* reduce to the one the authors of quantum measurement theories strive to solve. Even if it could somehow be shown that the nondiagonal matrix elements of the $S+A+E$ system (in the notations of Section 2) are strictly void of physical meaning, standard quantum mechanics would still have to face the problem in question and Bell certainly has a major point when he observes that, by contrast, the nonstandard models (hidden variables or nonlinear terms in the Schrödinger equation) *do* solve this riddle. Can the empirical reality approach do something similar? This is a difficult question. I think it can be answered positively by referring once more to the fact that, in contrast with the "independent reality" concept, that of empirical reality does, at least in part, take into account in its very definition the nature of the human (or should we say of human *and* animal?) modes of apprehension. But I do not, at present, feel myself in a position to develop this point further.

8. EMPIRICAL REALITY AND THE "NONLOCAL" DIFFICULTY

Nonlocality (nonseparability) cannot be made use of to send signals of any sort. In this respect it is not "operational" and if we define the phenomena on the basis of essentially operational criteria it is not a "phenomenon," or at least not a "full-fledged" one. Since the empirical reality notion is defined so as to be as closely related to the set of the phenomena as is possible, it seems proper to consider that nonlocality should *not* be included in its features. In other words, it seems adequate to sharpen its definition in such a way that it should be "local" (or "separable").

Of course, as long as we keep to the simple and safe procedure that consists in defining empirical reality in a strictly operational way (that is, by identifying this concept with a set of rules) the just-mentioned objective is reached automatically. But we saw that we should try to do better and define the concept in question in such a way that within this notion the verbs "to be," "to have," and so on should be utilizable, at least in their "weakened" sense. If we do this, can we still define the "empirical reality" concept so as to make it "separable"? We sketch here one way in which this can be done in a precise manner. It is, to an appreciable extent, inspired by recent works of Griffiths[12] and Omnès.[13,14] However, the first author considered his developments as concerning what is here called "independent reality," and the second one, while careful not to actually take sides on this issue, nevertheless described his ideas in such a way that this was by far the interpretation of them that came most naturally to the mind of his reader. Understood that way, both Griffiths' and Omnès' theories lay themselves open to some criticism.[15,16] But within the specific problem we are considering here, namely that of trying to make the "empirical reality" notion more definite, it seems that the ideas on which these theories are based can be made use of.

8.1. The Notion of "Consistent Set of Propositions"

Let us start with the (nonrelativistic) Wigner formula

$$w(a_1,\dots, a_{p-1}, a_p) = T_r[E_p^{a_p} E_{p-1}^{a_{p-1}} \cdots E_1^{a_1} \rho E_1^{a_1} \cdots E_{p-1}^{a_{p-1}}] \qquad (26)$$

expressing the probability that, if observables $A_1, A_2, \ldots, A_p$ are successively measured at times $t_1, t_2, \ldots, t_p$ respectively $(t_1 < t_2 < \cdots < t_p)$ on a system initially described by the state operator ρ, results $a_1, a_2, \ldots, a_p$ are obtained ($E_k^{a_k}$ is the Heisenberg projector onto the eigenspace corresponding to a certain eigenvalue a_k of A_k), and let us consider the case in which the system has originally been put in state ρ by means of the measurement at time $t_0 < t_1$ of some observable A_0 having given result a_0, so that $\rho = E_0^{a_0}$. Let us first take $p = 2$ and let us[15] consider two experiments X and Y. In experiment X all the three observables A_0, A_1, and A_2 are measured, by appropriate instruments, but only the first and the last results, a_0 and a_2, are noted. In experiment Y only A_0 and A_2 are measured. In general, for identical a_0's the probabilities of obtaining a_2 are different in the two cases, due to the fact that in the X experiment the system state is changed at t_1 through its interaction with the instrument measuring A_1. There may, however, be cases in which these two probabilities are equal. They are those in which

$$\sum_{a_1} T_r[E_2^{a_2} E_1^{a_1} E_0^{a_0} E_1^{a_1}] = T_r[E_2^{a_2} E_0^{a_0}] \tag{27}$$

Only in such cases can we say that if, in experiment Y, A_1 had been measured nothing would have been changed concerning the A_2 results. By definition we then say that the set $\{E_0^{a_0}, E_1^{a_1}, E_2^{a_2}\}$ constitutes a *consistent set of projectors* and that the corresponding set $\{P_0, P_1, P_2\}$ where $P_i (i = 0, 1, 2)$ stands for "observable A_i has value a_i at time t_i" constitutes a *consistent set of propositions* or—borrowing Omnès' terminology—*a consistent logic*. Our motivation for introducing such a terminology is that only if Eq. (27) holds true can we be sure, when considering experiment Y, that by conventionally asserting the "truth" of proposition P_1 we do not introduce any inconsistency in the overall description. This notion of a consistent set of propositions can be generalized in a rather straightforward way to the cases $p > 2$, and the equations then generalizing Eq. (27) are either identical with or very similar to Griffiths' consistency conditions.

8.2. True and Trustworthy Propositions

Attempts, such as Griffiths', to apply the foregoing considerations to a description of reality as such ("independent reality" in my language) meet with the (already alluded to) difficulty that given, say, P_0 and P_2 there are in general several (at least two) different and incompatible P_1's that can be associated with P_0 and P_2 in such way as to comply with Eq. (27) or its generalizations. For example, if E_0 and E_2 are projectors onto the states $S_z = +1$ and $S_x = +1$ respectively (in units $\hbar/2$) of a spin-1/2 particle, both sets $K_z = \{E_0, E_1^z, E_2\}$ and $K_x = \{E_0, E_1^x, E_2\}$ are consistent sets, where E_1^z

and E_1^x are the projectors (considered at time t_1) onto the states $S_z = +1$ and $S_x = +1$ respectively. Hence if we were to say that the proposition P_1^z associated to E_1^z is true on the system at t_1 we should also say that the proposition P_1^x associated to E_1^x is true, which is impossible since they are defined at the same time and since observable $S_z(S_x)$ constitutes a *C.S.C.O.* all by itself. All that we can say is that together with P_0 and P_2, P_1^z defines *a consistent logic* L_z in which it holds and P_1^x defines *another* one L_x in which it holds. The logic L_z can of course be trivially enriched with propositions such as (in obvious notations) P_t^z with $t_0 < t < t_2$, and the same for L_x. These enriched L_z and L_x are also consistent logics. But clearly a proposition such as P_1^z does not hold in *all* the consistent logics that can be constructed on the pair $\{P_0, P_2\}$. Such a proposition cannot therefore be said to be *true*, in the sense of factually true, since a factually true statement must be true independently of any mental choice we care to make of this or that "consistent logic." We say it is *trustworthy within some given consistent logics*[8] including here L_z and the above-defined enriched L_z.

8.3. True Propositions within Empirical Reality

But then, when all is said and done, does there exist *any* proposition bearing on contingent states of affair (we do not consider propositions such as "the electric charge of the positron and the proton are equal") and of which it could be said that it is not only *trustworthy* but actually *true*, where "true" means, or at least imply, "trustworthy within any consistent logic containing all the true facts"? As long as we think in terms of "independent reality" the answer is *no*, simply because within standard quantum mechanics (no hidden variables, no nonlinear terms) considered as an exact and universally valid description of what "really is" there are neither classical properties nor even classical systems, and therefore no "really existing" *facts*. Even if an assertion such as "the pointer of such and such an instrument lies in such and such a graduation interval" appears as being *true* to the community of presently existing men, some of its consequences could *in principle* be falsified as shown, for example, in Section 3. For this reason it is clear that the distinction between true and trustworthy propositions makes sense only with regard to empirical reality, in which context the foregoing objection loses its power. Within the realm of the empirical reality concept criterion A has to be weakened as we saw, and consequently assertions such as the one above—and more generally most of the

[8] Notwithstanding some differences there is a close parallelism between the here introduced notion of a "truthworthy proposition within a given consistent logic "and that, put forward by Omnès, of a "reliable proposition within a given consistent logic." The considerations of this section are inspired in part by those of Omnès' 1990 AJS paper.[14]

assertions bearing on macroscopic systems—may be considered as being really *true* (the consistent logics in which they would not be trustworthy are rooted in propositions that are much too "far fetched" to be testable by human beings).

On the other hand most, if not all, of the propositions bearing on microsystems are at best only trustworthy. In particular, propositions asserting that, after some measurement has been done on such a system, this system is in such and such a state (wave packet reduction) are only trustworthy. This is clearly seen on the foregoing example. Suppose that the propositions P_0 and P_2 of this example are true propositions (we construct them with reference to macroscopic measuring apparatuses operating at time t_0 and t_2). Wave packet reduction asserts in particular that at time $t_0 + \varepsilon$ the particle is in the state $|S_z = +1\rangle$. Let P_z^ε be the proposition stating this. The proposition P_z^ε is obviously trustworthy within L_z. It is also trustworthy within L_x if $t_1 > t_0 + \varepsilon$. But it is not trustworthy within *any* consistent logic that can be constructed on P_0 and P_2 since it is not an element of the consistent logic $\{P_0, P_x^{\varepsilon/2}, P_2\}$, where $P_x^{\varepsilon/2}$ is the proposition that at time $\varepsilon/2$ the x component S_x of the spin has value $+1$. More generally it seems it can be considered that no proposition asserting the state of a *microsystem* is "true" in the above-defined sense (a definition which, it will be remembered, itself makes sense only within the realm of the empirical reality concept and which avowedly rests on the anthropocentric concept of a measurement).

8.4. A Separable Empirical Reality

The foregoing conclusion does open a possibility of defining empirical reality in such a way as to make it separable. The procedure is to restrict the bearing of such words and expressions as "locality," "separability," "influences at a distance," and so on to what concerns "true" propositions. In other words it consists in considering that the "separability" of empirical reality can only be demanded in connection with what we call there "true" propositions, *not* in connection with just trustworthy ones. If we consider the assumption which is at the basis of the derivation of the Bell inequalities, namely

$$p(A \mid \lambda, \mathbf{a}, \mathbf{b}, B) = p(A \mid \lambda, \mathbf{a}) \tag{28}$$

(where, as usual, $p(X \mid Y)$ means the conditional probability that X *if* Y, and where A, B are measurement results, $\mathbf{a}$, $\mathbf{b}$ are unit vectors specifying the direction of the Stern–Gerlach instruments, and λ specifies the objective state of the system) we can then, if we accept the foregoing convention, consistently demand equality (28) *only* if the set of the λ's is a severely restricted one, namely the one that merely specifies what propositions are *true* at the source.[9] Since, with the conventions considered here, the set of the true propositions *cannot* include propositions bearing on the microsystems that constitute the said source (such propositions are at best trustworthy), it is clear that this restricted set of λ's is but a very small subset of the one that is considered in derivations of the Bell inequalities. But then, with *these* λ's *only*, there is no reason to identify a violation of Eq. (28) with a violation of locality.

9. CONCLUSION

To the idea that empirical reality—the set of the "phenomena"—is separable, the objection is often made that such experiments as those of Clauser, Fry, and Aspect do give quite strong indications favoring nonseparability and that these experiments, as any other ones, do bear on *phenomena.* In the last section we have seen that this objection can be circumvented. The essential reason why it can is, of course, that Bell's theorem is based on the idea—quite explicit in Bell's work— that physics has to deal with "be-ables," that is with elements of what is called here "independent reality." On the basis of this idea it is impossible to escape the view that the notion of objective state is meaningful also concerning microsystems and the Bell theorem follows. Here we have been interested in investigating the opposite view, namely that the real referents of physics are *not* be-ables, and what we have shown is that then the set of our collective experience—"physics" according to this view—can be accounted for in a *language* that *simulates* the one in which the be-able notion is central. We have, moreover, shown that (by introducing suitable restrictions on the bearings of certain words) this language can be construed in such a way that not only "instrument pointers can always be said to be lying in definite graduative intervals" but also "influences at a distance, nonseparability, and so on can be said not to be present."

On the other hand, the price we paid for this is very high. In particular it includes a renouncement of any genuine *explanation* of the correlations at a distance observed in experiments of the Clauser–Fry–Aspect type. Wave functions are tools for predicting then. They do not genuinely *account* for them since there is no sense in which they are "real." Still, these correlations exist. They must have a cause, even if that cause is foreign to the empirical reality realm. Hence the notion of a reality whose existence depends in no way on our existence must remain, I think, a necessary one. But, on the other hand, the idea that such an "independent reality" is, in

[9] Such an idea was already expressed by the author (Ref. 17, p. 95). In its present form it may be considered as a transposition, within the here described theory, of a similar idea of Omnès.[14]

principle, fully knowable is certainly not a logical necessity. The possibility of defining an empirical reality that subsumes nearly all the "appearances" is an indication that the idea in question is not even necessary for explaining the observed regularities. Perhaps, after all, the experiments of Aspect and others represent, as Shimony once suggested, our first steps in the elaboration of an experimental *meta*physics....

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