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Empirical Reality, Empirical Causality, and the Measurement Problem[1]

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Does physics describe anything that can meaningfully be called "independent reality," or is it merely operational? Most physicists implicitly favor an intermediate standpoint, which takes quantum physics into account, but which nevertheless strongly holds fast to quite strictly realistic ideas about apparently "obvious facts" concerning the macro-objects. Part 1 of this article, which is a survey of recent measurement theories, shows that, when made explicit, the standpoint in question cannot be upheld. Part 2 brings forward a proposal for making minimal changes to this standpoint in such a way as to remove such objections. The "empirical reality" thus constructed is a notion that, to some extent, does *ultimately refer to the human means of apprehension and of data processing. It nevertheless cannot be said that it reduces to a mere name just labelling a "set of recipes that never fail." It is shown that our usual notion of macroscopic causality must be endowed with similar features.*

INTRODUCTION

A "realistically minded" physicist of the last century could interpret physics as a faithful—though presumably incomplete—description of "what really is," without encountering any difficulty internal to science (any objection to this standpoint could only come from *a priori* philosophical considerations). Still today some physicists consider that their science should hold fast to this ideal. But most of them assign a more modest goal to physics and to knowledge in general. Science, they say (and ordinary knowledge as well), is indissolubly linked with human experience. Once

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and for all it must therefore give up the unattainable goal of describing whatever some thinkers may mean when they speak of "reality in itself" or "reality as it really is." Its purpose can only be a description of the *phenomena*, that is, of things, events, and so on, as they are organized by human collective experience. The human means of apprehension and the human means of data processing on which this human experience rests cannot be kept out of consideration and science should not try to do so. Although such a conception was a part of Kant's philosophical doctrine, it is considerably less detailed and specific than the latter was, so that it is not necessary to be a "Kantian" to subscribe to it. In fact it constitutes the—explicit or implicit—viewpoint of a great many thinkers and physicists of our times, most of whom hardly heard of—and at any rate never took any interest in—the Kantian philosophy. For short, let us refer to these people as "phenomenists."

Obviously this conception has much in common with the one that may be summarized by the sentence "human knowledge is but the set of all the ever-successful recipes," so much so indeed that on a first approximation these two views may be—and have been—considered as identical. The remark which lies at the basis of this article is that, however, a more detailed reflection shows the identity not to be complete; more precisely, it shows that the first view cannot *a priori* be said to reduce to the strict operationalism the second one consist of. There may exist several ways of "enriching" pure operationalism, but up to now only one of them was put into practice. It consists in supplementing it with ideas that are compatible with it and that are so natural-looking ones that they are currently—though implicitly—taken for granted both by the man-in-the-street (including us, in ordinary life) *and* by the philosophers.[3] They are usually considered as corresponding to "most obvious things concerning objects and their properties." Presumably they were tacitly accepted even by Kant, since they seem more or less implied by his use of the words "object" and "reality." The physicists of course—including the phenomenists alluded to in the above first paragraph—also have a strong *a priori* tendency to take them for granted. For what follows it is useful to formulate some of them quite explicitly, even if preciseness, here as well as in other fields, entails some degree of "heaviness." They read thus.

Idea A. There must exist a sense of each of the words used such that the assertion "At any time the center of mass of any macroscopic solid body has the property of being at some definite place (i.e., within some

[3] It seems that what many philosophers mean when they use the term "world" is a conception more or less implicitly based on the here described ideas.

small region R) and of not being anywhere else (i.e., within regions disjoint from R)" is meaningful and true.

Such a "property" is obviously variable in the sense that it can change in time. In what follows the word "property" is used to mean a property which is variable in this sense. As for the expression "observable prediction" occurring below, it means a prediction bearing on the result of a measurement that could conceivably be done.

Idea B. A *property* that a system is said to *have* must be operationally *but counterfactually* defined; that is, its definition must refer to the ways of measuring it but it should not refer exclusively to the cases in which a measurement is actually done, nor to the instrument actually used for this purpose. Instead, when we say that a physical system S "has a property P" we mean that *if* anybody came with some instrument suitable for measuring this type of property and applied it to S he *would* read on his instrument quite a definite result, corresponding to the one he and others have read on their instruments on similar circumstances and have conventionally associated with the statement "S has property P."[4]

Idea C. Let $\{S_1,..., S_i,..., S_N\}$ be an ensemble of physical systems prepared at time t_0 in some given way and let $\{\cdots P_n \cdots\}$ be a set of properties that systems of this type may have. If the computation rules R of a theory T which is acknowledged as valid are applied to the assumption "at time $t > t_0$, S_i has property P_{n_i}, $i = 1,..., N$," if the computation thus made predicts that at some time $t' > t$ some definite set of results will be observed on the ensemble of the S_i, and if the observations actually made at time t' disprove this prediction, the considered assumption is thereby disproved.

Idea C is a mere particularization of the non-contradiction principle. But in conjunction with idea B it has an important corollary, for, if idea B is accepted, the assumption "at time $t > t_0$, S_i has properly P_{n_i}, $i = 1,..., N$," in no way implies that, at time t, an instrument suitable for measuring the P_n's is actually present. Hence this assumption can then be *formulated* even in the case (which in fact is just the one, all of us have intuitively in mind when considering such matters) in which the systems S_i do not interact with any measuring instrument at all during the whole time interval (t_0, t'), thus undergoing during this time interval an evolution that simply obeys

[4] Of course we say that a system has a property P only in the cases in which what we know (e.g., about the system preparation) makes us say so. This definition of what we *mean* by attributing a property to a system should obviously not be interpreted as implying the (false) assumption that anything we could measure has some definite value.

the rules R of the theory T which is here supposed to be valid (in practice, quantum mechanics). For the purpose of testing the validity of the assumption in question, we can therefore compare the predictions at time t' derived from applying rules R to it, to those derived from applying these rules to what we know about the initial preparation of the system at time t_0. Hence we can state the following corollary to B and C.

Corollary. Under the conditions considered in stating idea C the assumption "at time $t > t_0$, S_i has the property P_{n_i}, $i = 1,..., N$" is false whenever it is the case that some observable predictions at time $t'(> t)$ derived from applying rules R to this very assumption contradict the observable predictions at time t', derived just from applying rules R to what is known about the initial preparation of the ensemble of systems at time t_0.

Idea D. The observable predictions at time t' that the corollary refers to are not restricted to those that could be experimentally verified at present, considering the present stage of our techniques. They also include those which are such that we could in principle (that is, without violating any basic axiom of theory T) conceive of instruments suited for verifying them, even if actually preparing such instruments would imply such practically unthinkable processes as, for example, a substantial man-induced entropy increase of the whole of our galaxy.

Finally let us add to this set of ideas another one, idea E, which is only used in Part 2 of this article.

Idea E. It is impossible to influence the past. More precisely, given the initial values of a set of dynamical variables of a system we can, within certain limits, decide on the corresponding final values (and act in such a way that the variables do take these final values), but the opposite is not true. Given the final values, we can never freely decide on the corresponding initial values.

Remark 1. Within a strictly deterministic theory, idea E would be trivial (for the impossibility would then also extend to the future), and within a theory that would not allot to free will some special status of its own, it is doubtful that an idea similar to E could consistently be formulated. But this does not prevent idea E from being a very basic element of our normal way of thinking.

Remark 2. It would be an oversimplification to say that idea E is disproved right at the start, independently of the other ones listed here, by the mere existence of the so-called "delayed choice experiments."[1] The experiments to which this name was given are truly "delayed choices" only if some additional assumptions are explicitly or implicitly made, such as that of locality (for example, no delayed choice exists in a conception in which the wave function is the sole basic reality and may collapse at a distance).

But are, actually, all these ideas true? More precisely, can we reconcile these ideas—which are precise formulations of a few things we think we know for sure—with the known data and with the predictive rules of quantum mechanics (not to be confused with the interpretation(s) of this theory)? Of course, it is a universally accepted fact that a number of basic concepts of quantum physics run counter to our naïve intuition. However, this is not what is a stake here. Ideas A to E are very general. They imply no "naïve" postulate bearing on such things as, for example, the particlelike or wavelike nature of matter. And it is neither an obvious fact nor a "well-known theoretical result" that the ideas considered here are incompatible with the quantum rules and with the data. Indeed, a number of theories (mainly measurement theories) were recently propounded that claim to reconcile the data and the quantum rules with natural-looking general ideas about macroscopic objects and about the meaning of such general notions as that of property; and it might therefore *a priori* have been expected that these theories would, in particular, be compatible with ideas A to E. In Part 1 of this article it is, however, shown that this is not the case, at least as regards the theories the existence of which the author is aware of.

Part 2 of the article can be read independently of the first one, if one just accepts the negative conclusion just stated. Its subject matter lies on the borderline between philosophy and physics since it is directed to the question of ascertaining whether or not the conclusion in question (assuming it is general) entails the necessity of retreating to the philosophical position of a strict operationalism (science and knowledge, even of ordinary things, viewed as mere prediction recipes). It is not *a priori* obvious that this is the case, for it is not inconceivable that modifications can be brought to the set of ideas A to E that will not amount to discarding them completely, and it may be hoped that, presumably at the price of changing some of our ingrained views about time, space, or macroscopic objects, the new set of ideas thus obtained will still, to some extent, play the same role as the old one, namely that of making factual knowledge something somehow more basic than just a set of good recipes for action. Accordingly, the second part adresses the question whether or not a new conception of the phenomena, or of empirical reality, can emerge that will play more or less the same role as the one entertained by the phenomenists of the past, but without contradicting the data and the quantum rule. It shows in particular that for this purpose the notion of an "empirical causality" endowed with somewhat surprising features must necessarily be introduced.

To be sure, quite an appreciable number of physicists readers will consider this second part as being "much too philosophical for their taste." But not all physicists shun philosophical problems. Many are aware of the fact that contemporary physics unavoidably raises some important problems of such a nature, and prominent among them is the figure of Professor Prigogine. Moreover, among the leading thinkers of our time, Ilya Prigogine is one of those who have shown by their deeds that a fruitful investigation of the problems in question must imperatively involve a significant contribution coming from the physicists themselves. For the present author it is therefore quite a specially great pleasure to dedicate this work to him.

PART 1

Our purpose in this part is to consider a number of recent theories that could *a priori* be considered as reconciling quantum physics with what we are tempted to consider as "most obvious things concerning objects and their properties," and show that this reconciliation fails by proving that the theories in question are incompatible with ideas A to E. This, in a way, is a continuation of a former work,[2] in which the author did the same as regards more ancient measurement models. The content of this section should not be considered as a criticism of the scrutinized theories, since the authors of most of them more or less explicitly say that they discard some of the ideas in question and in particular *idea D*. Rather, it should be viewed as a contribution to an attempt at determining somewhat better what kind of picture of an empirical reality we are allowed to uphold, thanks to their help.

1. Machida and Namiki

The theory of Machida and Namiki[3,4] is about the quantum measurement problem. More precisely its purpose is to solve this problem by making use of the macroscopic nature of the measurement apparatus. More precisely still, it expresses this nature by describing the apparatus in question by means of a continuous direct sum of many Hilbert spaces. Here we shall not need any very detailed description of the sequence of formulas by means of which this theory is made explicit, so that the reader is referred to the original articles for systematic information. We are only interested in trying to make as precise as possible the nature of the assumption on which this theory rests. As everybody knows, one of the most basic difficulty (some even say *the* basic difficulty) of quantum measurement theory schematically is that if we consider a pure state ensemble of quantum systems each of which interacts with an instrument appropriate for measuring a certain physical quantity, the instrument is expected to register some definite result in each case and to be afterwards in a state corresponding to this result. In the case in which the initial ensemble of measured systems is in a superposition of several distinct eigenstates of the quantity in question, this implies that the final ensemble of the combined systems (measured system plus instrument) must be a mixture, even though the initial ensemble of the measured systems was a pure case. Hence some cross-terms must somehow disappear in the representative statistical operator ("reduction of the wave packet"). For achieving this the authors resorted to the well-known idea of averaging over the phases, but they used this idea in an original manner which makes their argumentation worth much more attention than the former attempts by other authors. Briefly summarized, their idea is that although the relevant part A of the apparatus may be quite small, nevertheless it is macroscopic, that therefore we cannot sharply determine its energy and particle number, even by spending the longest time interval available to us, and that consequently it should be represented by a statistical operator having the form

$$\rho_A = \sum_{N \in (N_0, \Delta N)} W_N \rho_N^A \tag{1}$$

where $(N_0; \Delta N)$ stands for an interval with width ΔN around N_0, W_N for a positive weight factor ($\sum_N W_N = 1$), and N for the particle number of A. In a second step they take advantage of the fact that N_0 is very large while $\Delta N/N_0$ is very small, and they replace the discrete sum occurring in Eq. (1) by

$$\sigma^A = \lim_{N_0 \to \infty} \rho^A = \int dl W(l)\, \rho^A(l) \tag{2}$$

wherre $l = aN$ and where $W(l)$ is a normalized weight function centered on $L = aN_0$ with a width $\Delta L \simeq a\Delta N$. When the A's thus described are made to interact with the microscopic systems, initially assumed to be in a pure state described by $\psi = c_1 u_1 + c_2 u_2$ (the case of a spin 1/2 system with σ_z eigenvectors u_1, u_2 is considered here for simplicity, σ_z being the observable to be measured), the density matrix of the (ensemble of the) combined system(s) becomes, in the authors' model (the details of which we skip here),

$$\Xi_t = \sum_{i,j} c_i c_j^* \Xi_t^{i,j}, \qquad i, j = 1, 2 \tag{3}$$

where the integrals

$$\left[\int dl_i W(l_i)\, e^{-ip_i l_i} \cdots\right]\left[\int dl_j W(l_j)\, e^{ip_j l_j} \cdots\right] \tag{4}$$

appear in the terms $\Xi_t^{i,j}$ for which $i \neq j$ (the dots representing smooth functions of the l's) whereas only integrals bearing on smooth functions of the l's appear in $\Xi_t^{i,i}$ $(i = 1, 2)$. In Eq. (4) the p_i play the role of effective momenta $(\hbar = 1)$ and are determined by the details of the model. In the limit in which $p_j \to \infty$, the integrals in Eq. (4) vanish (Riemann–Lebesgue theorem), so that the desired result is achieved.

As stressed by the authors themselves, the averaging procedure described by Eq. (2) is quite essential for their theory to go through. As we saw, its justification is based on the fact that in practice *we* cannot determine precisely the number of particles composing A, and on the view that this large but finite number can be replaced by the limit $N_0 \to \infty$. Clearly, a reference to the limitations of human ability is involved there. This reference contradicts idea D. Moreover, even if the quantities p_i are large, they are not infinite, so that strictly speaking it cannot be asserted that the values of the integrals in (4) are exactly equal to zero. For these and similar reasons it was stressed by H. Araki that the reduction of the wave packets taking place in this theory is only approximate.

Since the reduction of the wave packet is only approximate, there is no reason to expect that for *all* possible observables the predictions derived from the reduced density matrix are the same as those correctly derived from the nonreduced one. Admittedly the theory shows that this must be the case for all the observables that we, as human beings, can reasonably expect to be able to measure in practice, and this may be viewed as an essential achievement of the theory. But the theory claims no more, and in fact there must exist Hermitian operators for which these two types of predictive recipes lead to different result. There is no reason or principle for assuming that these Hermitian operators do not correspond to observable quantities. Under these conditions it must be concluded that the reviewed theory conflicts with the set of ideas A to E. More precisely, in the case such as that of a Stern–Gerlach measurement, for example, if, by assumption, idea D is true, there is no state reduction. In other words, after the spin–instrument interaction has taken place, a pointer geared to register which one of the counters has fired cannot lie in any one definite graduation interval, contrary to idea A (if it did then, on an ensemble, some of the physical quantities that are measurable in principle would not have the values quantum mechanics enables us to derive from the known initial conditions; see ideas B and C and the corollary).

2. Araki

In nonrelativistic physics the number of particles composing a system cannot change and there exists no observable quantity that would allow us to distinguish a "pure state" represented by a linear superposition, with coefficients c_N, of state vectors describing states with different particle numbers N from a "mixture" with weight $|c_N|^2$ of states with definite particle numbers. In such cases it is convenient (see above) to consider several Hilbert spaces H_N, indexed by N, the observables being described by self-adjoint operators defined within each H_N (i.e., not connecting different H_N's), and it is said that a superselection rule exists. When N is unknown (with mean value N_0), the description of a state of the system by a statistical operator then takes the form of Eq. (1). Taking the limit $N_0 \to \infty$, $\Delta N \to 0$, as is done in the Machida–Namiki theory, then obviously corresponds to going over from a discontinuous to a continuous superselection rule. It is therefore clear from the content of the foregoing pages that the consideration of a continuous superselection rule is an essential component of the Machida–Namiki theory.

On the other hand, the introduction of a continuous superselection rule in a measurement theory is not sufficient, as we saw, for making the latter an "exact" theory, where "exact theory" means here a theory compatible with ideas A to E. It is on this question (of how to make an exact measurement theory) that Araki recently made an important contribution.[5] Again, we refer to the original work for details and we merely summarize the results. There are three distinct ones.

(i) A continuous superselection rule can give rise to reduction of wave packets in a quantum mechanical *separation* procedure (in contrast to a measuring procedure) in the infinite time limit (in Araki's terminology a separation procedure differs from a measurement procedure in that the instrument is not brought in distinct states by it).

(ii) The reduction of wave packets is impossible in the case of a *discrete* selection rule (or of no selection rule whatsoever).

(iii) Even in the case in which a continuous superselection rule is present, the reduction of the wave packets and the measurement procedure proper must proceed in two distinct steps. No setup can exist that would produce them simultaneously. *First* the reduction should take place (and it takes an infinite time) and *next* the measurement should be done, with some conventional apparatus. At least, this, in the author's words, seems to be the best that can be achieved. For completeness, let it be noted that the author does not consider the cases in which the measurement alters the state of the measured system (2nd kind measurements). It is, however, doubtful that taking such cases into consideration could alter the author's general conclusions.

As regards the continuous superselection rule, it is of course not surprising that it should be associated with a classical variable. This variable is N in the Machida–Namiki case. In Araki's model it is a classical magnetic field $\mathbf{h}$. The general argument concerning this association is, as we know, that the variable that serves as a label for the various Hilbert spaces obviously has a sharp value on *any* pure state. By definition, such a variable is called "classical." Hence a classical variable is necessarily associated with a superselection rule. Now, since the presence of a superselection rule is a necessary condition for an exact measurement model to be possible, it follows that such a model is only possible within a general theory enlarging elementary quantum theory by incorporating classical variables. In principle this is not a difficulty since the theories based on the algebra of observables do precisely this (some comments on this point appear below). On the other hand, an essential feature of Araki's model is that the possible (classical) states of the magnetic field system should be described by *continuous* functions (thus barring δ-functions). This rules out the possibility for this state to be a "pure state," since a pure classical state (in Segal's sense) corresponds to a δ function probability distribution. In other words, the possibility for the magnetic field $\mathbf{h}$ to have a sharp value is definitely excluded. As the author puts it, "this restriction may be interpreted to represent our inability to control the external magnetic field with absolute accuracy."

With respect to the question here investigated, Araki's article is important, to begin with, in that it proves that the ordinary quantum measurement theories (including the Machida–Namiki one as we said) are incompatible with ideas A to E. According to it, in our search for models that would be compatible with this set of ideas we are left only with candidates of a rather unusual type, in which a reduction procedure is made *before* the actual measurement takes place. But, when all is said, even these models cannot be reconciled with the set of ideas in question. The reason for this can be formulated in two stages which, taken together, constitute a strong argument. The most obvious one is that the reduction procedure takes an infinite time and we only have a finite time at our disposal. Trivial as it may appear, this remark is nevertheless most significant. The only way in which we could perhaps hope to be entitled to overlook it would be to argue that somehow and for some reason we are allowed to replace large numbers (here, times large compared with average atomic times) by infinite ones. But if we indulge in this mathematically questionable manipulation, it is difficult to understand why we should refrain from replacing very small numbers by zero, and in particular why we should exclude the "pure states" of the **h** system. The advocates of mathematical rigor cannot be one-sided. If they forbid the second move, they must forbid the first one too.

3. The Environment Theories

Briefly summarized, these theories consider the real cause of the "wave packet reduction" to be the fact that the instrument is not strictly isolated from its environment. Some of them (see, e.g., Refs. 6 and 7), moreover, claim that the environment also ultimately determines *what* physical quantity is actually measured, by creating a "preferred basis."

Here we are only interested in the reduction problem, and again we need not enter into the finer details of these theories. In them the mechanism of wave packet reduction is as follows. To the right-hand side of an equation such as (1) above they add a factor describing the environment variables. To account for the fact that we do not measure any of these variables, they then suggest to take the partial trace of the full statistical operator over the environment space. And it can then be verified that when the time t becomes large the coefficients of the cross-terms $c_i^* c_j$ $(i \neq j)$ all become extremely small.

Our purpose here is not to appreciate the value of the environment theories as compared with other quantum measurement theories. It is just to investigate whether these theories are compatible with ideas A to E. It turns out that they are not, and, here again, it must be stressed that their authors do not claim they are. In Zürek's words "information is not destroyed, it is merely transferred." The point is that it is transferred to entities that, for practical reasons, human beings are quite unable to measure. From a strictly operationalistic point of view, this is obviously quite sufficient, and we must therefore say that these theories do indeed reach their objective. But this of course does not imply that they are compatible with ideas A to E. The reason why they are in fact not is qualitatively the same as in the case of the Machida–Namiki theory. Within any finite time the coefficients of the cross terms never strictly vanish. Some self-adjoint operators therefore exist, the mean values of which are not the same on the actual ensemble as on the reduced one. If the environment is composed of N atoms and if N is large, the difficulty of measuring the corresponding observables must be great, and it may increase beyond limits, that is, it may become a strict impossibility, in the idealized case $N \to \infty$, $t \to \infty$. Admittedly, if we only consider as conceivable the measurements that have a given, finite degree of difficulty, we may then be able to find a number N_0 such that whenever $N > N_0$ the cross terms are not detectable. But, on the other hand, to any given finite N there must correspond a degree of difficulty such that if we may conceive of measurements the difficulty of which exceeds it, the cross terms *are* detectable. The conclusion is then the same as in the previously considered models. If idea D is to be upheld, measurements of any degree of difficulty are conceivable, the cross terms *are* in principle detectable, and the quantum mechanical predictions concerning the observables which make it possible to detect these terms are incompatible with the assumption that idea A is valid, hence idea A cannot be upheld.

4. The Algebraic Theories

Some authors have claimed that a number of conceptual difficulties appearing in "ordinary" quantum mechanics could be removed by resorting to the more elaborate theory known as algebraic quantum mechanics. However, the distinctive features of these theories are most apparent in the case of systems endowed with an infinite number of degrees of freedom and, as far as the quantum measurement problem is concerned, this has the consequence that they lead to interesting results essentially in the idealized limit in which finite but very large numbers are replaced by infinite numbers. Since this is just the type of idealization which was shown above to be incompatible with idea D, the algebraic theories do not bring here anything new. In fact, apart from the already reviewed Araki theory—which can in some sense be considered as belonging to this realm—the only algebraic quantum measurement theory known to this author is the one by Hepp.[8] This theory was discussed by Bell,[9] and the sequence of these two articles makes it quite clear that, in fact, the above analysis of the environment theories applies just as well to Hepp's theory (a more detailed study of this point was already presented in Ref. 2).

5. Griffiths

The way in which Griffiths tries to overcome the conceptual quantum mechanical difficulties is altogether different from those analyzed above. In this author's views (for a full and balanced description of which the reader is referred to the original article[10]), the wave function as we know it has no privileged role, and a measurement informs us, just as in classical physics, of the value the measured quantity had immediately *before* the measurement was performed. Since, however, the quantum aspects of the atomic world cannot be forgotten, Griffiths has to bring in nonclassical features. This he does by means of the idea that for the same sequence of events not one, as in the classical mode of thinking, but several "consistent histories" may in general be considered. For example, let S be a spin 1/2 particle propagating along Oy, let a measurement of its spin component S_x along Ox be made at time t_1 and a measurement of its spin components S_z along Oz be made at a time $t_3 > t_1$, and let the results be +1/2 in both cases. If t_2 is an intermediate time, $t_1 < t_2 < t_3$, then, according to the author, two (at least) consistent histories H_1 and H_2 can be considered. In both of them S_x *has* the value +1/2 immediately after t_1 and S_z *has* the value +1/2 immediately before t_3. But in H_1 S_x has the precise value +1/2 at t_2 whereas in H_2 it is S_z that has the precise value +1/2. Calling D and F the two above-mentioned measurement results, obtained at t_1 and t_3 respectively, and using the symbol $(Y|X)$ to denote the conditional probability that Y be true if X is true, it is thus possible, in this theory, to simultaneously assert the validity of both propositions:

$$(S_x(t_2) = +\tfrac{1}{2} | D \wedge F) = 1 \tag{5}$$

and

$$(S_z(t_2) = +\tfrac{1}{2} | D \wedge F) = 1 \tag{6}$$

where $S_n(t_2)$ is the value of S_n $(n = x, z)$ at time t_2 and where $\wedge$ means "and." Nevertheless Griffiths rejects the hidden variables assumption. He therefore claims that the simultaneous validity of (5) and (6) should *not* be interpreted as implying

$$((S_x(t_2) = +\tfrac{1}{2}) \wedge (S_z(t_2) = +\tfrac{1}{2}) | D \wedge F) = 1 \tag{7}$$

in other words, while he considers as separately valid both the statement ≪if D and F are true, "$S_x(t_2) = +1/2$" is true≫ and ≪if D and F are true, "$S_z(t_2) = +1/2$" is true≫, he does *not* consider as valid the statement ≪if D and F are true, "$S_x(t_2) = +1/2$" and "$S_z(t_2) = +1/2$" are *both* true≫.

Now, if this theory is interpreted as a tentative strongly objective description of independent reality (which seems to be its author's view) then it is difficult to accept the above stated claim. For let us consider the case in which D and F *are* both true. Then, according to (5) and (6), the propositions "$S_x(t_2) = +1/2$" and "$S_z(t_2) = +1/2$" are both true. But, even in quantum and other nonconventional logics, it is considered that, whenever a and b are propositions that both are true, then the proposition "a and b" is also true. In fact, it even seems difficult not to consider that the two sentences "a is true and b is true" and "a and b are both true" are just two ways of saying the same thing. But then, the truth of (5) and (6) *must* imply the truth of (7). This was, moreover, to be expected. If both H_1 and H_2 are strongly objective consistent histories and if we decide not to make a choice between the two, then it seems unavoidable that the events described by each of them should all be true. On the other hand, if Griffiths' theory were modified along these lines, it would just become a hidden variables theory, and one, moreover, of a rather primitive type, unable to pass the tests of the Bell theorem.

A General Remark

Because of its relevance to the problems investigated here, we restate here a remark already formulated elsewhere (Ref. 2, p. 195). It bears on the smallness of the difference between the observable predictions derived from the nonreduced statistical operator and from the reduced one. In view of this smallness, we could be tempted to say that, after all, the state of affairs considered here does not significantly differ from the one existing in classical physics since, there also, objects can never be considered as being "strictly separated and noninteracting." Could the fact that if idea D is upheld, idea A cannot strictly be maintained (and that therefore a pointer cannot properly be said to *be* in any one graduation interval) be just another instance of this trivial impossibility? The answer is *no*. The two cases are in fact quite different. In classical physics an approximate statement S about a system T can always be replaced by a strictly true statement S'. For example, if T is composed of two distant objects, the approximate statement "the two objects have practically no interaction with one another" can be replaced by a precise statement of the form "the interaction between these objects is smaller than x." On the contrary, concerning the combined system (including apparatus and environment) considered at a time $t < \infty$ after the interaction, there is no precise statement embedding idea A that can be reconciled with idea D. The precise statement that such and such a quantity is smaller than a certain value bears only on those mathematical artifacts that ensembles are. It does not bear on the *actual* combined system.

Conclusion

There are two conclusions to Part 1. The first one is, of course, that the set of ideas A to E turns out not to be compatible with the recent measurement theories reviewed here (just as a basically equivalent set of ideas turned out to be incompatible with the older measurement theories, examined in the author's 1976 book[2]). Of course we could not study *all* the measurement theories, existing or as yet to come, but it can nevertheless be concluded that the plausibility of the assumption that a new measurement theory, compatible with the set in question, will eventually turn up is becoming extremely small. We take no great risks in discarding this assumption.

The second conclusion is that, as previously noted, the just mentioned negative result cannot *a priori* be considered as compelling us to fall back on a strictly operationalistic conception of knowledge (science as a mere set of recipes); for it is conceivable that the set of ideas A to E can be replaced by another one that can be reconciled with quantum physics. The quest for this new set is the theme of Part 2.

PART 2

For partly philosophical and partly physical reasons unfolded elsewhere,[11] the present author considers that the notion of an independent, veiled reality is necessary. However, this reality is *not* the subject matter of the present article. As already noted, our problem here is to steer a midcourse between the conception that the purpose of science is to *unveil* this ultimate reality (a view sometimes called "physical realism") and the one according to which science and knowledge are but successful *recipes* for predicting what a community of human beings will, under given circumstances, eventually observe (a view sometimes called *strict* or *philosophical operationalism*, or *instrumentalism*). At this stage many physicists will insist that, strictly speaking, the problem in question is not a scientific one. This may well be admitted (defining science lies beyond the scope of this article), but at the same time it must be stressed that the philosophers are not adequately equipped for studying the question unless they have learned physics. Hence this study *calls* for physics. Such an observation may be considered as an adequate justification for the fact that at least *some* physicists to take some interest in this question.

The midcourse the idea of which is here considered consists in trying to enrich the purely operationalistic viewpoint by adding to it some ideas, much in the same way as the "phenomenist" physicists of the past did instinctively add, as we saw, some ideas to this same operationalism, and wrought up this way a conception of what could be called an *empirical reality*, somehow endowed with appreciably more "substance" than a set of recipes can have. The difference is that most of the phenomenists in question did this implicitly, guided as they were by "commonsense," whereas we must proceed in full awareness of every step we take. Another difference is of course that, as we also saw, the older phenomenist viewpoint was still too near to naïve realism to accommodate quantum physics whereas our objective is to construct a new conception that should be compatible with its, here assumed, *unrestricted* validity.

Notwithstanding what was just mentioned, it is clear that our new, constructed, notion of an *empirical reality* can be of any significance only if it keeps as close as possible to our "apparently obviously true" ideas, concerning, in particular the macroscopic world. This implies, among other things, that we should try to keep as much as we can of the substance of ideas A and E (see Introduction). As regards idea B, the only possible alternative to it sems to be to replace counterfactual definitions by "partial definitions" as they are called. The partial definition procedure consists as we know in attributing a meaning to the sentence "system S has property P" only in the cases in which an experimental setup is actually positioned in such a way as to allow a check of the validity of the sentence. Basically this was Bohr's way of defining the properties of the microscopic systems, and indeed, once and if the class of these systems has been defined, the procedure is consistent. But to apply it to macrosystems would mean that the use of the conditional mood should be avoided also concerning the properties of these systems, and this seems awkward. Moreover, since we have no fully consistent criterion for distinguishing macro- from microsystems the rule according to which the partial definition procedure should only be applied to the properties of the microsystems would be an ambiguous one, if not in practice, at least in principle. The alternative considered is therefore unattractive.

Hence let us keep idea B. Since idea C is just a particularization of the noncontradiction principle, it must also be kept. Thus, finally, it seems the only remaining possibility is to modify idea D. This should not at this stage be taken as a strictly sharp statement since we only found reasons for keeping "as much as possible" of the content of ideas A and E.

The Axiom of Empirical Reality

The substance of idea D can be expressed by asserting that when discussing questions of principle no limitation should be set on the sophistication of the measurements the possibility of which is considered, as long as no quantum mechanical principle is violated thereby. It was pointed out in Part 1 that the notion of an unbounded degree of sophistication is obviously an abstraction, but that this abstraction is a natural counterpart to the one that consists in considering as infinite some measurement times that are actually finite, or some numbers of particles that are finite too. The above proof of the fact that the measurement theories studied here are incompatible with ideas A and D (taken together) rested on the observation that for any finite time—long as it may be—or for any finite number of particles—however large—observables and measurements can be principle be conceived of, the sophistication of which exceeds the limit up to which measurements detect no difference between the reduced and unreduced statistical operator, so that idea A is falsified as regards such things as instrument pointers—at least in some cases—and cannot therefore be considered as being universally true. To avoid being forced to this conclusion, it is necessary to find a substitute for idea D. And the first point we want to make here is that this can be done in quite a precise and unambiguous manner, just by stating explicitly an idea which lies in fact at the basis of all the various quantum measurement theories but which is always kept implicit there. This idea is best expressed as an axiom, which may be called the axiom of empirical reality. It reads thus:

Axiom of Empirical Reality. *A theory of empirical reality is obtained by postulating (a) that replacing very large times by infinite times and/or very large particle numbers by infinite numbers is a valid abstraction, and (b) that, on the other hand, the possibility of measuring observables exceeding a certain degree of complexity is to be considered as nonexistent, even in matters of principle and even though this possibility, in principle, actually exists.*

Clearly, when, in the set of ideas A to E, idea D is replaced by the *axiom of empirical reality*, the incompatibility between the set in question and the predictive rules of quantum mechanics vanishes. In accordance with idea A, we can then say that within the system-plus-instrument(s) ensemble and when the measurement is over, the pointer of any instrument has some definite macroscopic location. It is to be observed that the axiom in question is in fact an explicit formulation, not only, as we said, of the implicit ideas lying at the root of various quantum measurement theories but also of many of the arguments used in physics and consisting in "approximating" large numbers by infinity. Reference is made here, in particular, to the approximations that make it possible to "bridge the gap" between quantum physics and chemistry (see, e.g., Ref. 12) by making use of such methods as the Born–Oppenheimer procedure and, more generally, to the "approximation" that makes it possible to connect quantum and macroscopic physics by artificially going over to systems with an infinite number of degrees of freedom, thus endowing the algebras of observables with a "center" (see again Ref. (12)).

On the Status of the Empirical Reality Concept

We asserted above that the axiom of empirical reality makes it possible to attribute a definite macroscopic position to any macroscopic object, including the "pointer" of an instrument; and that this even covers the cases in which the instrument previously interacted with a quantum system not initially lying in one of the eigenstates of the quantity the instrument can measure. But let us try to be precise as regards the meaning of this assertion. It is counterfactually defined—as idea B requires it to be for any assertion bearing (as this one does) on a property of a system—but it must be granted nevertheless that the wishes lying at the root of our intuitive reasons for demanding counterfactuality are not fully met by the assertion in question. In fact, these wishes and reasons revolve around the idea that even if we cannot know "the ultimate nature of things," still the macroscopic objects can be said to be fully mind-independent and can be described as such. With the *axiom of empirical reality* replacing idea D, this view can no longer be strictly maintained since, at a crucial point in the whole picture, this axiom limits the sophistication of the measurements that are taken into account, by referring either to some free decision of ours or to practical limitations in the aptitudes of the human species.

The status of the empirical reality concept therefore turns out to be a subtle and hybrid one. As described here, this notion, clearly, has much in common, if not with our naïve idea about "real things," at least with the views the most thoughtful minds have held on what should be called real. As recalled above, it makes it possible to endow the algebras of observables with a center, in such a way that, in many important cases, we can use in an unambiguous way expressions of the type "at such and such a time such and such a system *has* such and such properties"; and for this reason it goes much beyond a mere set of recipes, in the direction of realism. On the other hand, as we just saw, it definitely does not go as far in this direction as—*a priori*—the upholders of philosophical realism, and even most phenomenists, would have wished we could go. In fact, it allows us to use the verbs "to have" (in assertions such as "this system *has* this property" and "to be" (in assertions such as "this system *is* in such and such a domain of space") only in somewhat weakened senses, since in some cases some measurements that are in principle possible and that we ruled out just "by decree" would falsify what we then say.[5] Empirical reality may then be defined as the set of all the subjects of the verb "to be," taken in this weakened sense, whereas independent reality (a notion on which, as

[5] Should we here take advantage of the presence in our languages of the two verbs "to be" and "to exist," and use one of them as a synonym for "to be" in the strong sense, while using the other one as a synonym for "to be" in the weak sense? Some philosophers do make a distinction between the meanings of these two verbs and we could of course follow their example. Whether a common standpoint would thereby be reached between them and us remains, however, a debatable question.

previously noted, the present article touches but superficially) would be the set of the subjects of the verb "to be," taken in the strong sense in which the realists of yore took it (supposing of course reliable sentences could be constructed with such subjects, account being taken of what we know now).[6]

Empirical Causality

As already stressed, the empirical reality concept can constitute a significant complement to pure operationalism only to the extent that it makes it possible to refine upon the ideas we naïvely consider as being obvious, while keeping much of their spirit. In view of this, we still have an important point to explore, for there is one idea that *a priori* we would very much like to keep. This is our common usual way of *explaining* what takes place. It is a fact that we account for most of the details we observe (either in daily life or in macroscopic investigations of a scientific nature) by attributing them to the occurrence of definite events belonging to the past history of the presently observed system or systems, hence in particular to events on which our *present* observing procedure has no effect (idea E). Suppose, for example, that a counter placed within one of the outgoing beams in a Stern–Gerlach device is connected to a bulb so as to make the bulb light up some time (say a few seconds) after it has fired. Under such circumstances we like to *explain* our observation at some given time of lighting up of the bulb by saying that at a somewhat earlier time the counter *did* actually fire, and induced the bulb to light up, just as we explain the presently observed U-shaped valleys by saying that during the ice age glaciers were *actually* there and did give them their present shape. One of the psychological reason why at first sight we consider as grotesque the idea of a counter being in a quantum superposition of states (states of having and of not having fired) is just that this idea *prevents* us from mentally conceiving of a chain of relationships between observed macroscopic effects and some (one or several) *definite* macroscopic causes, thus throwing doubts at quite a deep level on an essential element of the mental scheme by means of which we explain to ourselves (and to other people) those things that we believe we understand.

But are we really prevented from building the chain in question? At first sight the adoption of the axiom of empirical reality would seem to remove at one stroke the difficulty for if, in the considered example, we

[6] In the present author's opinion the use some theorists—in particular Primas[12,13]—make of such words and expressions as "to be," "ontic states," and so on can be reconciled with other statements made by them only if, in their writings, the verb "to be" is given this weakened, man-centered sense.

apply this axiom to the lighting up of the bulb (thus making this lighting up a definite event, which does or which does not take place), we of course also apply it to the firing of the counter, which is also a macroscopic event, and which thereby is made definite too. However, this argument does not completely clear up the matter at hand, as can most easily be seen by considering a hypothetical case in which, instead of having happened just a few seconds before the lighting up of the bulb, the firing of the counter took place, say, a million years ago, and in which the "causal chain" between these two events involved many intermediate steps. The point is that applying the axiom implies shaping up the empirical reality according, in part, to human decisions (about the level of complexity at which experiment should stop) or, at any rate, according to human ability. Under the circumstances just described, applying this axiom to the counter means therefore that the empirical reality of the past—that empirical reality we refer to when we speak of past macroscopic events such as the formation of the sun and so on—can in some way and in some cases be dependent on some decisions (in the above sense) or on human ability.

Macroscopic causality understood in this weakened sense may be called *empirical causality*. This conception is a (subtle) alteration of idea E since in it, as we just observed, the individual events composing the empirical reality of the past, such as, in the above example, a counter firing or not firing, depend in a way on us—not, of course, in the sense that we could *decide* whether they occurred or not but just through the fact that, at the considered time and in the strong sense of the verb "to be," the counter actually "was" neither discharged nor undischarged. *A priori* this seems to raise the following puzzling question: "Why is it the case that (assuming we have enough information) we all, on the basis of what we see, *agree* to say either that the counter fired or that it did not? Basically, however, this question is neither more nor less puzzling than the one concerning the agreement between several persons who *now* observe a counter used in such a Stern–Gerlach experiment. The fact that we do agree with one another as to what we see on instances such as this one (and more generally on observed events taking place in macroscopic, empirical reality) is not as trivial as a long practice induces us to think. Indeed it is an enigma. But this, after all, is not surprising since the only explanation ever given (and which seemed obvious) of the agreement in question was based on a specific assumption (which *also* seemed obvious) about the independent reality of the localized objects, since we now know that this conception was too naïve, and since we do not know for sure what other conception should take its place (relative state theory, nonlocal hidden variables theories, nonmathematical descriptions: there are possibilities but certainly no definite knowledge).

As regards empirical causality, another riddle is of course present. This is the one concerning the E.P.R. correlations between the results of measurements that are spatially separated. As Bell's theorem shows, there is no way of accounting for these correlations by signals or by energy transfers travelling at subluminal or luminal speed. Yet measurement results (firing of counters and the like) belong essentially to the empirical realm, and empirical reality should not be endowed with nonseparability features if we want it to remain close to anything we experience (the notions of *local* objects and events are so essential to it, as we saw, that merging them into a "sea" of nonseparability and holism would deprive the empirical reality concept from significance). Here again, to search for an answer we must look in the direction of the independent reality notion. More precisely we have at our disposal a well-defined set of rules, namely the principles of quantum mechanics, that allows us to predict without any ambiguity what correlations of this type will eventually be observed. Raising some or other of the algorithms (kets etc.) used in formulating these rules to the status of an exact description of independent reality would yield an "explanation" of these corelations. This, however, cannot be considered as a final answer since (as in well known and as we checked once again in Part 1) any postulate according to which such and such quantum algorithms are exact descriptions of independent reality leads to severe difficulties. Hence, here again, it must be acknowledged that we have good *descriptions* but no fully reliable *explanations* of the facts under consideration.

Empirical versus Independent Reality and Conclusion

A remark on independent reality is in order here. The pervading practical and scientific importance of empirical reality stands in contrast with the elusive role of the independent reality concept. Even at present, many physicists who write the word "reality" actually mean "empirical reality," and sincerely believe they neither need nor do make reference to what we called "independent reality." However, there exists a simple criterion that makes it possible to check whether or not they actually do, in this respect, what they believe they are doing. It consists in examining whether or not they use the word "nature" and in what sense. It will then be discovered easily that they all do, that, in the contexts in which they use this apparently familiar word *nature*, it can only mean "independent reality," and that they could not express their ideas if they abstained from using it. Both the "empirical reality" and the "independent reality" concepts must therefore be kept.

Surely, some, who long for a mathematically expressible solution to the basic troubles, may dislike the solution proposed here on the ground that introducing as we do a mind-dependent empirical reality in the picture amounts to somehow moving in an *ad hoc* way the borderline between (empirical) classical and quantum objects, and that this is a hazy—not to say an easy—answer. In our opinion this objection would, however, not be well founded. The reason is that what human beings actually find does not always coincide with what they expect to find, that mathematically expressible strict solutions of the "realistic" variety seem to be escaping us, and that it is rationally possible that the movableness in question, when its ins and outs are clarified and made an inherent part of the theory, should in fact constitute *the* correct answer.

In conclusion, it seems that the project of steering a midcourse between metaphysical realism and strict operationalism without violating quantum physics is not an impossible one. The central piece of our proposal for carrying it out is the axiom of empirical reality, replacing idea D and making it possible to keep idea A (localization of macroobjects), with the specification, however, that the verbs "to have" and "to be" must be taken in their weak sense. Idea E (no influence of the present on the past) then remains tenable, provided that the word "was" should also be taken in the weak sense, and provided that the usual (and reasonable) requirement that intersubjective agreement be not only described but also *explained*, be watered down. The assertion that the macroobjects are mind-independent can then be upheld, even though this is only possible under the (here steadily recurring) condition that the verb "to be" be given its weak sense. Similarly, our normal way of explaining macroscopic events by sets of definite macroscopic causes can be preserved, be it only in the form of "empirical causality," without contradicting basic, that is, quantum, physics. As regards the internal consistency of man's rational thinking, this result was most desirable and, even if the price paid for it here may look quite high (watering down of rather normal requirements, see above), still, the fact that it is obtained may be considered as reassuring.

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