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Some reflections inspired by my research activity in quantum mechanics

GianCarlo Ghirardi

Department of Theoretical Physics of the University of Trieste, The Abdus Salam International Centre for Theoretical Physics, Trieste, Italy
and
Istituto Nazionale di Fisica Nucleare, Sezione di Trieste, Italy

E-mail: ghirardi@ts.infn.it

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Abstract

By taking advantage of my long lasting research activity on the conceptual foundations of quantum mechanics I reconsider some of the basic problems which have been at the centre of the recent debate on this theory. Specific attention is given to topics like quantum nonlocality, the impossibility of faster than light communication and the so-called measurement or macro-objectification problem. A large part of the paper deals with the dynamical reduction program and discusses its merits and achievements, as well as its limitations. The above considerations lead me in a natural way to express my personal views on the present status of the foundational studies.

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1. Introduction

In this paper I will express my personal views on quantum mechanics (QM) going through some important moments and facts of my career which have been influential for my position about this theory. This paper is organized in the following way: after mentioning a crucial moment of my evolution I will briefly recall some of my scientific works which have been stimulated by it, even in those cases in which the two processes are separated by various years.

2. The unavoidability of the macro-objectification problem

The first event I want to recall goes back to the period which immediately followed my getting a degree in physics. I attended a seminar by G M Prosperi on the so-called Daneri–Loinger–Prosperi [1] proposal for overcoming the difficulties of the measurement problem. That seminar has been a sort of revelation for me. In fact, even though I had followed an excellent course in QM, I had not been so smart as J S Bell and B d'Espagnat who, in their

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words, when exposed to this theory have immediately spotted its unsatisfactory status. In my case it was my reflection on what I heard from Prosperi which lead me to realize:

- That if one requires that all natural processes obey the linear laws of the theory it turns out to be impossible to account for the way the theory itself postulates measurement processes to take place, in particular for wave packet reduction (WPR).
- That the solution proposed in [1] did not represent a satisfactory way out of the difficulties of the formalism. It belonged to the line of thought that some years later J S Bell has characterized [2] as a FAPP point of view: the very basic rules of the theory are logically inconsistent and lead to unacceptable situations but, due to difficulties of a practical nature, one can use the formalism as it stands if he is exclusively interested in making practical predictions[1].

The necessity of clarifying the macro-objectification problem at a fundamental level has become a guiding line of my successive investigations, even though before starting to work systematically on foundational problems some years had to elapse, years in which for various contingent reasons I worked mainly on formal scattering theory and on symmetry principles. But, motivated by this experience, I always followed with great interest and attention the debate on foundations.

Concerning the point I have just mentioned, repeated reference to it can be found in almost all papers I wrote in the last thirty years but my most significant contribution appears in [4] which deals with the macro-objectification problem in an extremely general way. The paper, coauthored with A Bassi, has been inspired by a previous analysis I had presented [5] at a Conference celebrating Bohr's centenary. The nice features of our approach consist in the fact that it avoids all the restrictive assumptions which characterize the proofs based on the use of von Neumann's ideal measurement scheme[2]. Actually we have shown in all generality that even if one takes into account possible malfunctionings of the apparatus, its unavoidable entanglement with the environment, its good but not absolute reliability, its many fundamentally uncontrollable degrees of freedom, and one assumes the general validity of the linear nature of quantum mechanics, one can easily devise situations in which the final state cannot correspond to any definite and acceptable situation of the part of the universe we perceive, i.e., one which matches our definite perceptions. In brief, if one assumes that there are devices which allow us to obtain (not certain but only reasonably) reliable information concerning two orthogonal states of a microsystem, then, in an ensemble, the large majority of these devices, when triggered by an appropriate linear combination of the previous states, end up in a macroscopically meaningless situation.

It is worth mentioning that among the few assumptions of our approach, besides the one that all natural processes (the microscopic as well as the macroscopic ones) are governed by the linear laws of QM there is the completeness assumption, i.e. that the knowledge of the

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[1] I consider it appropriate to mention here a paper [3] by J M Jauch which has the merit of having made clear what was not so precisely outlined in the Daneri–Loinger–Prosperi paper, i.e. that, to take seriously this proposal one should admit that the set of all conceivable macro-observables of a macroscopic system constitutes an Abelian set. Stated differently there are superselection rules between the macroscopically distinguishable linear manifolds of such systems, so that a statistical mixture turns out to be indistinguishable from a pure state. As it has been clarified in various papers and books, this position is untenable since it enters into an irremediable conflict with the fact that the system–apparatus interaction actually violates such superselection rules since the 'ready' state of the apparatus is macroscopically different from the one in which its pointer points at a position correlated to the measurement outcome.

[2] It seems appropriate to mention at this point also my interest in the mathematical aspects of the measurement process which has led us [6] to generalize the celebrated Wigner–Araki–Yanase theorem [7] limiting the possibility of performing ideal measurements in the presence of additive conservation laws.

state vector represents the most exhaustive information that one can have, in principle, about an individual physical system.

The paper I have just mentioned, [4], has given rise to an interesting exchange of views [8] with B d'Espagnat. His position has been stressed once more in his contribution [9] to this special issue.

3. The EPR-Bohm Gedanken experiment, Bell's inequality and quantum nonlocality

In the 1960s, as everybody knows, an absolutely revolutionary step towards the understanding of natural processes took place: the derivation, by J S Bell, of his celebrated inequality [10]. All those interested in the foundational problems of the theory had certainly paid the due attention to the Einstein–Bohr debate which arose in connection with the so-called EPR paradox [11], especially after its simpler reformulation by D Bohm [12]. I remember clearly the enormous impact Bell's paper had on me. I red it again and again, and I was more and more surprised by its unexpected implications: one must recognize that natural phenomena exhibit basic nonlocal features, this conclusion being completely independent from the formulation and/or the interpretation of the theory and stemming simply from the experimental predictions of QM. This is a fact of nature that neither Einstein, nor his opponents, had contemplated during their long lasting debate[3].

For me and my colleagues A Rimini and T Weber one of the aspects of the formalism which deserved a particular attention in view of Bell's result was the issue of whether the nonlocal aspects associated with entangled states of far away constituents might conflict with the requirements of special relativity. Even though there was a widespread opinion that this was not the case, a detailed analysis of this important issue was missing, apart from an interesting contribution by P Eberhard [13]. We decided to tackle this point and worked out a rigorous proof [14] that there is no way of using entanglement and nonlocality to send faster than light signals between distant observers. This result has been subsequently generalized [15] to cover the case of measurement processes involving effects in place of projection operators and POVM measures in place of PVM measures.

Before leaving this topic I consider it appropriate to recall a marginal fact strictly related to it. In that period I have been involved in refereeing papers claiming that, by resorting to appropriate experimental devices and set-ups, one might actually use the process of WPR for superluminal signalling. One of these papers was due to N Herbert [16] and made use of an hypothetical cloning machine. I spent some days in spotting the flaw in the argument and in so doing I derived and I included in my referee report[4] what became known as the *no cloning theorem* in a form which is practically identical to the one which was presented almost 1 year later in the celebrated paper by Wootters and Zurek [18].

After this parenthesis let us come back to our main theme.

4. Facing the macro-objectification problem

As mentioned in a previous section, if one assumes that measurement processes are physical processes just like all other processes (and, accordingly, they must be described by the theory) and that the theory is complete, one has unavoidably to face the problem of accounting for

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[3] Before going on I consider it appropriate to stress that since then I have always been extremely surprised by the serious misunderstandings about Bell's theorem, misunderstandings that arose immediately, which persist even now and make clear how many authors fail completely in grasping that what Bell has shown is that natural processes are basically nonlocal.

[4] See also [17].

our definite perceptions concerning the fact that macroscopic objects—and in particular the macroscopic 'pointer' of the measuring apparatus—are in macroscopically definite situations and not in the superpositions implied by the linear nature of the formalism. Such a problem has been, since the early times of the debate on quantum mechanics, the paradigmatic example of the conceptual unsatisfactory status of the quantum view of nature. The orthodox way out resorting to the WPR postulate, besides being basically inconsistent as pointed out by scientists like A Einstein and E Schrödinger, as recognized by J von Neumann, and as proved in its greater generality in [4], has two extremely unsatisfactory implications:

- It attributes a peculiar conceptual status to the entire theoretical framework viewing it as simply describing 'what we observe' and being silent on 'what it is'. In fact the theory must make an unavoidable reference to an 'outside world', which is classical, to account for the quantum world it deals with, and, moreover, it leaves absolutely ambiguous the division between these two different levels of reality.
- It attaches an absolutely particular role to the conscious observer whose free will decision to perform a measurement draws the universe from the 'vagueness of waves' associated with quantum superpositions to the 'definite status' matching our definite perceptions.

This unsatisfactory situation has been stressed innumerable times by great scientists, but no one has more lucidly put into evidence its unacceptability than J S Bell in almost all papers he devoted to foundational issues[5]. The text [20], which is included in this special issue, reproducing his lecture on the occasion of the 25th Anniversary of the Abdus Salam ICTP, is a nice and illuminating example of the deep and lucid position of this great thinker on this fundamental point.

Obviously, this problem was worrying also me since my university days. I would like to take the opportunity that its consideration offers to me to briefly comment on the evolution of the position of the scientific community concerning the foundations of quantum mechanics. After the long debate between Einstein and Bohr there have been various proposals to overcome the macro-objectification problem. The best known is due to J von Neumann [21] and requires to relate reduction to consciousness, a proposal which has been supported also by E P Wigner [22] for a certain time[6]. Much before the just-quoted paper by Wigner, the absolutely revolutionary proposal of D Bohm [24] appeared.

I must confess that I did not pay immediately the due attention to this suggestion which undoubtedly marked a turning point for the debate on the interpretation of quantum theory since it constitutes an explicit example that the no-go von Neumann's theorem asserting the impossibility of a deterministic completion of quantum mechanics is irrelevant and moreover it paved the way for the fundamental investigations of J S Bell [10]. I owe my real understanding and appreciation of this fundamental contribution to the stimulus which I got various years later in reading the lucid papers [25] by my friends D Dürr, S Goldstein and N Zanghí. My interest in this topic is testified by a paper, [26], in which, with my PhD student E Deotto, I have proved that there are infinitely many possible theories having the same formal structure of Bohmian mechanics, i.e. which adopt as their primitive ontology that the 'beables' of the theory are the positions of all particles of the universe and which account for their distribution at any time—a distribution completely agreeing with the one of standard QM—but which, however, assign completely different trajectories to the individual particles. I believe that this is a point which, in spite of its seemingly odd and marginal relevance, deserves some attention in the debate about this beautiful theory.

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[5] An exhaustive collection of these papers can be found in [19].

[6] However, it would be unfair not to mention that later this great scientist has contemplated [23] the possibility of some physical process leading to the desired diagonalization of the statistical operator at the macro-level.

At this point I will comment briefly the situation in the period going from the 1950s to about 1964, the year of the fundamental paper by J S Bell. In that period, many important physicists, among them Bohm, Jauch, Loinger, Prosperi, Rosenfeld, Wigner, Everett III and others were keeping alive the debate on the interpretational problems of the theory, but the scientific community at large did not pay too much attention to these fundamental issues. Actually, I remember very well that the shared attitude was more or less the following: these problems are nonscientific problems and might interest exclusively philosophically minded people, but are of no relevance for the scientific enterprise. The practical counterpart of this position was that it made extremely difficult, even for people deeply involved in the subject, to be taken seriously and to get the due academic recognitions. In the subsequent years (1965–1990) the situation changed, slowly but continuously, this being due mainly to the gradual realization that Bell's result was of great relevance[7], accompanied by an increasing attention to his repeated lucid and critical papers [19] concerning the mathematical, conceptual and epistemological unsatisfactory status of the theory.

A first important change occurred in 1970 when B d'Espagnat organized the Varenna Enrico Fermi School on the Foundations of Quantum Mechanics. In his Letter to Participants, opening the meeting, he wrote a quite illuminating sentence [28]:

> Like the Delphic oracle Theoretical Physics rests on three legs: experience, mathematics and a workable set of general ideas. Some would like to cut the third leg away ... We are all convinced that a mere collection of wholly or partially successful recipes—be they even beautifully formal—cannot be substitute for a genuine understanding.

The commitment of B d'Espagnat led him also to publish [29] a very nice book in 1971, a book which became a sort of 'reference point' for all scientists interested in the matter.

Let me go on. H Everett III published his paper [30] proposing the so-called many universes interpretation of QM in 1957. However, this idea attracted a lot of attention only after the publication [31] of the paper by B S DeWitt in 1970 and of his book, coauthored with N Graham, in 1973. Thus, taking also into account the late appreciation of Bohmian mechanics, we can appropriately identify the 1970s and the 1980s with one of the most alive periods for the foundational studies on our best theory. In those years many important attempts to overcome the macro-objectification problem appeared. I will briefly mention the most relevant ones: the *modal interpretations of quantum mechanics* [32], the *many-minds interpretation* [33] and the two approaches, having strict relations between them, of *decoherence* [34] and its formalization leading to the line of thought characterizing the *decoherent histories* [35–37] approach. The last proposal I have to mention is the so-called Dynamical Reduction Program [38]. Also quite serious and deep investigations concerning previous suggestions appeared in this period, the most important one being the systematic and completely exhaustive study of Bohmian mechanics by D Dürr, S Goldstein, N Zanghí and their group [25].

The same period has seen the publication of many important books and papers on the subject by eminent philosophers of science. Among them I would like to mention A Shimony, J Butterfield, M Redhead, B van Fraassen, D Albert and T Maudlin.

Finally, the Oxford Meeting on the occasion of Scrödinger's centenary [39] and the Erice meeting in 1989 [2] have been, at least for me, the two most exciting moments of this period, first because on these occasions J S Bell has really been the 'leader' of the debate by focussing lucidly and by giving voice to his deep views concerning all positions about QM, secondly because he has expressed his deep interest in what we had done.

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[7] An extremely interesting analysis illustrating the unreasonable delay of the scientific community in accepting the implications of Bell's result appears in [27], to which I refer the interested reader.

Coming now to my contributions in these years I will first of all describe some of my investigations on proposals by other scientists[8], in particular those invoking decoherence to overcome the difficulties affecting standard quantum mechanics, and only subsequently I will discuss the already mentioned *Dynamical Reduction Program* which represents, in my opinion, the most interesting original contribution I have given, together with my colleagues A Rimini and T Weber, to the foundations of quantum mechanics.

5. Decoherence and decoherent histories

An attempt to make the quantum description of natural processes compatible with our definite perceptions at the macrolevel derives from invoking the decoherence which arises from the unavoidable and uncontrollable interactions with the environment. This line of thought which has been initiated by E Joos and H Zeh [41], has been supported by Joos himself [42] and, among others and with great emphasis, by W Zurek [34]. It can formally be expressed in the following terms[9]. Let us consider a microsystem S in a normalized linear superposition of the eigenstates $|\varphi_i\rangle$ of a micro-observable Φ which we want to 'measure' by means of an apparatus A in a ready state $|A_0\rangle$, appropriately coupled to the microsystem, yielding the standard unfolding of the measurement process:

$$\sum_i c_i|\varphi_i\rangle \otimes |A_0\rangle \to \sum_i c_i|\varphi_i\rangle \otimes |A_i\rangle, \tag{1}$$

$|A_i\rangle$ being the macroscopically different orthogonal final states of the apparatus. The argument proceeds as follows: the macroapparatus, just due to its macroscopic nature, cannot be kept isolated from the environment, different macrostates of the apparatus giving rise to different states of the environment, so that a more accurate description of the process would be:

$$\sum_i c_i|\varphi_i\otimes\rangle|A_0\rangle \otimes |E_0\rangle \to \sum_i c_i|\varphi_i\rangle \otimes |A_i\rangle \otimes |E_i\rangle. \tag{2}$$

The final states $|E_i\rangle$ of the environment have some basic features:

- They involve many elementary constituents, such as atoms or molecules of the air etc,
- The states of these constituents cannot be controlled by the experimenter,
- When the indices referring to the environment differ, at least two of its constituents are in states which are orthogonal, or, alternatively, many constituents are in different states (even if not strictly orthogonal) and, accordingly, the scalar product of many such states practically vanishes, so that, in all cases, $\langle E_i|E_j\rangle \cong \delta_{i,j}$.

The conclusion is obvious: to describe the final situation which interest us, i.e., the one of the system–apparatus system, one has to take the partial trace on the unavoidably uncontrollable environmental degrees of freedom, and, in accordance with the above properties, the final situation is described by the statistical operator:

$$\rho_{S+A} = \sum_i |c_i|^2 |\varphi_i, A_i\rangle\langle\varphi_i, A_i|, \tag{3}$$

which can be looked at as describing a statistical mixture $\mathcal{E}$, with weights $|c_i|^2$, of the states $|\varphi_i, A_i\rangle$, a fact matching precisely the WPR postulate. It goes without saying that this part

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[8] For my general views about this point I refer the reader to [40].

[9] Here, to make the argument simple we will make reference to von Neumann's ideal measurement scheme, but the reader will meet no difficulty in understanding that precisely the same considerations hold when one adopts the extremely general approach of [4] or even its generalization, due to G Grübl [43], to the case in which the initial state of the microsystem is not a pure state.

of the argument is perfectly correct, i.e., there is no doubt that if one is exclusively interested in a statistical ensemble of identically prepared systems and disregards the orthogonal and uncontrollable states of the environment one gets from the standard formalism the statistical predictions given by Born's rule. But this is not the whole story. In fact, what some of the 'easy-solvers' of the measurement problem along this line assert, is that, since the final situation is exhaustively described by the statistical operator of equation (3) and since such an operator coincides precisely with the one of the above-mentioned statistical mixture, one can claim that, in practice, at the end of the measurement process one is actually dealing with such an ensemble. For instance, W Zurek has stated [34] that, due to the interaction with the environment[10]

> . . . a coherent superposition of states [like those of equation (1)] . . . is continuously reduced to a mixture. A preferred basis of the detector, sometimes called a 'pointer basis' has been singled out . . . Decoherence prevents superpositions of the preferred basis states from persisting.

A similar position has been taken by Anderson who, in reviewing a book [45], stated:

> The last chapter . . . deals with the quantum measurement problem . . . My main test, allowing me to bypass the extensive discussion, was a quick unsuccessful search in the index for the word 'decoherence' which describes the process that used to be called 'collapse of the wavefunction'. The concept is now experimentally verified by beautiful atomic beams techniques quantifying the whole process.

The reason for which the conclusion that decoherence solves the macro-objectification problem is illegitimate should be obvious: it completely disregards that in quantum mechanics the correspondence [statistical ensembles] → [statistical operators] is infinitely many to one and that there are infinitely many statistical ensembles whose individual constituents are in superpositions of macroscopically different states which are described by the same statistical operator (3). What makes then legitimate to associate this mathematical entity to the physical situation matching our definite perceptions, perceptions which are always definite, even at the level of a single experiment and not only with reference to statistical ensembles? Actually, many authors who have stressed the unavoidability and the great relevance of decoherence have been careful and have admitted that something is missing in these approaches. In their seminal paper on decoherence as a source of spatial localization Joos and Zeh [41] state:

> Of course no unitary treatment of the time dependence can explain while only one of these dynamically independent components is experienced,

and in a subsequent paper [42] Joos himself, even though supporting arguments which make appeal to decoherence, has felt the necessity of specifying that the fact that our perceptions are always definite might

> perhaps be justified by a fundamental underivable assumption about the local nature of the observer . . . and his way of perceiving.

Many authors have presented similar or related criticism to the naive assertion that decoherence solves the conceptual problems of the standard theory. One interesting recent paper is due to S Adler [46].

Up to this point I have underlined in a sketchy way the role of decoherence. But in the 1980s there has been a serious attempt [35–37] to formalize this aspect and to base on it a

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[10] See also our answer [44] to his letter in Physics Today.

'new and consistent solution of the macro-objectification problem' by proposing the so-called decoherent histories approach we are going to describe.

Let S be any physical system (a microscopic one, such a system plus the apparatus or even the whole universe) which is associated, at the initial time t_0, with the statistical operator W and let $U(t, t_0)$ be the unitary operator describing its evolution. One then chooses n arbitrary instants of time $t_1 < t_2 < \cdots < t_n$ and, for each of them an exhaustive set $\{P_m^{\alpha_m}\}$ of mutually exclusive projection operators:

$$\sum_{\alpha_m} P_m^{\alpha_m} = 1, \qquad P_m^{\alpha_m} P_m^{\beta_m} = \delta_{\alpha_m \cdot \beta_m} P_m^{\alpha_m}. \tag{4}$$

One also considers the following projection operators:

$$Q_m^{\alpha_m} = \sum_{\alpha_m} \pi_m^{\alpha_m} P_m^{\alpha_m}, \qquad \pi_m^{\alpha_m} = 0, 1. \tag{5}$$

One history is then defined by the sequence of the considered times and a corresponding sequence of projection operators, each of them taken from the set $\{Q_m^{\alpha_m}\}$:

$$H_{\text{his}}^{(\alpha)} = \{(Q_1^{\alpha_1}, t_1), (Q_2^{\alpha_2}, t_2), \ldots, (Q_n^{\alpha_n}, t_n)\}. \tag{6}$$

When the operators appearing in equation (6) belong to the set $\{P_m^{\alpha_m}\}$, $H_{\text{his}}^{(\alpha)}$ is said to be maximally fine-grained, while consideration of the operators $Q_m^{\alpha_m}$ corresponds to taking into account a coarse-graining of the histories. We can come now to the central concept of the approach: a family of histories is a set whose elements are all histories[11] having the form (6).

For a given family one considers the *decoherence functional*:

$$\begin{aligned} D(\alpha, \beta) = \operatorname{Tr}\big[&P_n^{\alpha_n} U(t_n, t_{n-1}) P_{n-1}^{\alpha_{n-1}} U(t_{n-1}, t_{n-2}) \cdots U(t_1, t_0) W U^{\dagger}(t_1, t_0) \cdots U^{\dagger}(t_{n-1}, t_{n-2}) \\ &\times P_{n-1}^{\beta_{n-1}} U^{\dagger}(t_n, t_{n-1}) P_n^{\beta_n}\big] \end{aligned} \tag{7}$$

in which the projection operators with apex α characterize the history $H_{\text{his}}^{(\alpha)}$ and those with apex β characterize another history of our family. A family of histories is then said to be decoherent iff:

$$D(\alpha, \beta) = \delta_{\alpha,\beta} D(\alpha, \alpha), \tag{8}$$

i.e. if the decoherence functional vanishes for any pair of maximally fine grained histories which do not coincide. On the other hand, when they coincide the expressions $D(\alpha, \alpha)$ are assumed to yield a probability distribution over the maximally fine-grained histories of the decoherent family, and in such a case one can also introduce the associated coarse grained histories and their probabilities.

Some remarks are at order. The theory, at its fundamental level, does not attach any particular role to measurement processes (even though it is perfectly legitimate to consider a history including the statement—represented by the corresponding projection operator: *at this time the pointer of the apparatus lies in the interval we associate to the outcome 'it points at 3'*). No WPR postulate has to be taken into account.

We note that given a decoherent history, i.e. a history belonging to a decoherent family, the associated probability is precisely the one which standard quantum mechanics attaches to the process: the system, prepared in the state W is left to evolve (according to Scrödinger's

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[11] We stress that the projection operators at different times can make reference to completely different 'observables': for instance at time t_1 one might make a statement referring to the observable L_z of a particle (such as $L_z = m\hbar$) while at time t_2 one might make assertions concerning the observable L_x of the same or of another particle, and at time t_3 one might consider the projection operator corresponding to the pointer of an apparatus being in a given interval Δ, etc.

evolution equation) up to time t_1, at this time a measurement of the observable labelled by the apex α_1 takes place and the outcome associated with the eigenvalue 1 of the projection operator $P_1^{\alpha_1}$ is obtained, then the system evolves again and another measurement is performed and the corresponding outcome is obtained, and so on. But in spite of this relation to the standard formalism here one does not consider any measurement or WPR to take place, the theory pretends simply to attach probabilities to histories of the type: at 12 my particle had its spin pointing along the positive direction of the z-axis, at 12:05 it was in the space region Δ and so on. The statements refer to an objective situation and not to the one in which observers decide to perform measurements of the considered observables.

A fundamental fact has to be stressed: in order that the so-defined probabilities have a meaning, one must confine strictly his considerations to a decoherent family. If one puts together histories corresponding to different families and they are not part of a larger decoherent family, then the whole procedure collapses since the basic rules of probability calculus are violated[12].

The general philosophy of the approach can now be made clear: one must always limit his statements to the histories of a decoherent family and, within such a framework, the probabilities of the theory define a classical probability measure on the set of histories and classical logic can be used. As shown by Omnés and Griffiths [35, 36] one can directly define the conjunction and the disjunction of two histories as well as the negation of a history, i.e., all connectives necessary to build a Boolean algebra. Correspondingly the associated statements fit perfectly well in a classical Boolean logic concerning the probabilities of 'the objective events' summarized by any history. Actually one can also define in a straightforward way the logical implication between two histories of the same family, which we will express as $H_{\text{his}}^{\alpha} \Rightarrow H_{\text{his}}^{\beta}$. The proponents and the supporters of the decoherent histories approach maintain that one can consider completely in general, i.e. with reference both to micro and macroscopic systems, or even to the whole universe, all conceivable sets of histories, provided they belong to a single decoherent family. Within the scheme decoherence replaces the notion of measurement. There is no need for conscious observers or for the postulate of WPR; the statements refer to objective facts obeying classical logic.

Let us now come to various criticisms to the above proposal. A first criticism derives from the fact that one can very well consider decoherent families whose histories are physically senseless. Typically, what meaning whatsoever can one attach to the histories of a decoherent family claiming that the celebrated Scrödinger's cat is not *either* alive *or* dead, but alive+dead? In particular, Dowker and Kent [47] have proved[13] that within the proposed interpretation, taking the past and present for granted, one can identify infinitely many decoherent families of histories which imply, in general, a future nonclassical (and meaningless) behaviour of macrosystems.

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[12] A trivial but illuminating example can be mentioned. One can prove that a one time history $H_{\text{his}}^{\alpha} = (Q_1^{\alpha_1}, t)$ belongs always to a decoherent family, trivially to the one whose fine-grained histories are those denoted by $(P_i^{\alpha_1}, t)$, with the index i running on the whole set of fine-grained projection operators. So, if we limit our considerations to the spin degrees of freedom of a spin 1/2 particle we can consider the family with 2 elements (P_1^{z+}, t) and (P_1^{z-}, t) in which P^{z+} and P^{z-} project on the one-dimensional manifolds associated with 'spin up' and 'spin down' along the z-axis. It is obvious that such a family is decoherent because $\text{Tr}\, P^{z+} W P^{z-} = 0$ and it is also obvious that $\text{Tr}\, P^{z+} W P^{z+}$ and $\text{Tr}\, P^{z-} W P^{z-}$ can be consistently interpreted, within the scheme under consideration, as *the probabilities that the particle has, at the considered time, the spin up or down in the z-direction* without any need to measure it. If we try to include in the family also the history asserting that at the considered time the particle has spin up along the x-direction, we would be in trouble with the probability calculus because the two previous probabilities already sum up to 1. But actually, since at least one of the expressions $\text{Tr}\, P^{z+} W P^{x+}$ and $\text{Tr}\, P^{z-} W P^{x-}$ does not vanish, this last history cannot be accommodated in the same family as the previous one while preserving decoherence.

[13] See also [48].

Another serious problem derives from the fact that the same history can very well belong to two different families each of which is coherent but that cannot be combined in a larger decoherent family. The most clear cut example of this situation has been identified by A Kent [49] and makes direct reference to what has been considered by Griffiths [35] and Omnés [36] among others, as the most interesting aspect of the approach, i.e., that within a decoherent family, classical logic, including the implication, holds and can be systematically used. Kent considers two decoherent families, Fam_1 and Fam_2 having an history, $H^{\alpha}_{\rm his}$ in common and containing two other histories $H^{\beta}_{\rm his} \in Fam_1$ and $H^{\gamma}_{\rm his} \in Fam_2$ which are chosen in such a way that, in Fam_1, $H^{\alpha}_{\rm his} \Rightarrow H^{\beta}_{\rm his}$ while in Fam_2, $H^{\alpha}_{\rm his} \Rightarrow H^{\gamma}_{\rm his}$, and, moreover $H^{\beta}_{\rm his}$ turns out to contradict $H^{\gamma}_{\rm his}$. This gives rise to an obviously quite embarrassing situation which must be faced.

The reader who has followed us will immediately guess the answer of the proponents of the interpretation we are considering: the fundamental fact about the approach is that histories which cannot be accommodated within the same family, i.e. which do not decohere, can never be considered together; accordingly to compare $H^{\beta}_{\rm his}$ and $H^{\gamma}_{\rm his}$, violates the fundamental *single family rule* of the theory which imposes to deal always and exclusively with a decoherent family. When confronted with this situation we have been inclined to agree with the position of B d'Espagnat [50] who has claimed that the just outlined fact represents a real logical paradox. In spite of the above remarks, however, we were perfectly aware of the fact that, at the purely formal level, the decoherent histories approach, when enriched by the strict adoption of the single family rule, could not be claimed to be inconsistent. Accordingly, in a series of papers, we have tried a different approach [51] to really question the tenability of the proposal, an approach which—differently from the one of A Kent—puts into evidence a contradiction without ever resorting to compare different histories belonging to different families.

The idea goes as follows: let us start by considering a specific family of decoherent histories and let us take into account the diagonal elements $D(\alpha, \alpha)$ of the decoherence functional which, according to the assumptions of the scheme, represent the probability distribution over the histories of our family. One can then raise a fundamental question, one which has been formulated many times by J S Bell in connection with the orthodox interpretation: these probabilities are *probabilities of what?* Of course, not the probabilities of *finding* the system in the situation described by the history when a measurement is performed. The only possible answer in order that the proposal can be taken seriously is that the probabilities be, in a sense, classical, i.e., that they refer to objective properties of the system under consideration. Only if this is the case one can claim to have worked out a consistent realistic interpretation of Quantum Mechanics, as often advocated by the supporters of the decoherent histories approach.

To clarify this point we can consider the probabilities of classical statistical mechanics, a theory which gives, in general, only probability distributions over the set of the subsets of the phase space, implying, however, that any individual physical system be uniquely associated with a precise point in phase space. This association renders automatically true or false any statement concerning the properties of the system. Actually it is precisely this feature which makes classical statistical mechanics compatible with an objective attitude towards physical reality. In particular any statement such as 'there is a molecule whose position belongs to a certain region of space' has a precise truth value (in general unknown to us). According to this point of view, and as repeatedly claimed by the proponents of the approach, the probabilities of the decoherent histories must have a conceptual status which is analogous to the one of the classical probabilities; in particular, it must be possible to assign to every decoherent history a precise truth value, even though in general we do not know which one is the right one.

At this point we have taken into account four assumptions which are explicitly made by the proponents of this interpretation or which are simple translations of their claims and which are, in our opinion, absolutely necessary in order that the proposal can be legitimately asserted to have achieved the purposes for which it has been presented. The assumptions are

- Any family of decoherent histories can be equipped with an algebraic Boolean structure allowing us to use classical logic.
- In spite of the fact that within a family we know only the probability $D(\alpha, \alpha)$ of a given history, this probability refers to objectively possessed properties, and as such the history must have a truth value.
- All conceivable families of decoherent histories must be taken into account.
- If a *unique* decoherent history belongs to different and incompatible families, its truth value must be independent of the family we consider it to belong. It is obvious that if this is not the case one has to face an extremely embarrassing situation: if he looks (this is a purely mental act) to the history from the perspective of a family then the history is actually true (false) but if he looks at it from the one of another family, then the history is false (true). If this is the case, which advantage would the theory present with respect to the standard theory with the orthodox interpretation?

It is then easy to prove [51] that the four preceding assumptions imply that there must be a homomorphic mapping of all the projection operators of the Hilbert space of the system on the Boolean set $\{0, 1\}$. We have then simply to recall that Kochen and Specker, in their celebrated paper [52] have proved precisely that such a homomorphism cannot exist. This shows that the decoherent histories approach is contradictory unless one gives up one of the previous requests[14].

A remark might clarify the situation. We recall that the Kochen and Specker theorem has been formulated to call attention on a quite peculiar aspect of any conceivable deterministic hidden variable theory predictively equivalent to quantum mechanics: its unavoidably contextual character. This means that, given the maximally precise specification of the state of a physical system, the certain predictions that the theory makes about possessed properties turn out to be contextual, i.e., the certain results which the theory attributes to any measurement process depend not only on the just mentioned most accurate specification of the state, but on the whole context, in particular, on the specific measurement procedure which one chooses to ascertain the value of the considered property. Somebody has considered this aspect of Hidden Variable Theories as a serious drawback, but we share the position of Daumer, Dürr, Goldstein and Zanghí [54] who have stressed that:

> Properties which are merely contextual are not properties at all; they do not exist, and their failure to do so is in the strongest sense possible.

Therefore, while contextuality can be fully accepted within the hidden variable approach since different truth values assigned to precise statements are always associated to different and incompatible practical ways of ascertaining the corresponding ‘contextual properties’, the dependence of the truth value of an history within the decoherent history approach is unacceptable, since within it the fact that an history belongs to one or another decoherent family does not correspond to different physical situations, but simply to the perspective one chooses to look at the statements of the theory.

These remarks conclude the presentation of our criticisms to the decoherent histories approach to the conceptual problems of the standard formalism. It is time to pass to more relevant results of our research activity.

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[14] It has to be mentioned that in [53] a brief sentence appears, suggesting that the decoherent histories approach is affected by problems of the kind we have put into evidence in our work.

6. The dynamical reduction program

In this section I will briefly recall the investigations [38, 55] I have carried on, with the fundamental cooperation of my colleagues and collaborators, proving that a different approach to the solution of the macro-objectification problem is in fact viable. I am referring to the elaboration of the so called Dynamical Reduction Program (DRP) which contemplates the possibility of modifying the dynamical equation of the theory by the addition of nonlinear and stochastic terms. The perspective inspiring our researches can be simply summarized by recalling a deep remark by A Leggett [56]:

> One might imagine that there are corrections to Schrödinger's equation which are totally negligible at the level of one, two, or even one hundred particles but play a major role when the number of particles involved becomes macroscopic (say, of the order of 10^{23}).

or the lucid observation by J S Bell [39]:

> Either the wavefunction, as given by the Schrödinger equation is not everything, or it is not right.

6.1. *The original theory*

The idea underlying the DRP is to try to derive, within a Hilbert space scenario, the two basic and incompatible evolution principles of the standard theory, i.e. the linear and deterministic Schrödinger evolution and the nonlinear and stochastic process of WPR from a unique, universal, dynamical equation. Accordingly, such an equation must lead to the spontaneous suppression of the superpositions of macroscopically different states of any macrosystem and at the same time it must not change in any appreciable way all the known properties of microscopic systems. As already stated, we tried to achieve this goal, at the nonrelativistic level, by introducing nonlinear and stochastic modifications of the standard Hamiltonian dynamics.

We consider it appropriate to make immediately two points:

- The proposal does not represent a reinterpretation of the standard formalism, but qualifies itself as a rival theory of quantum mechanics which, in principle, can be tested against it in appropriate experiments,
- The interest of the proposal derives from the fact that it represents (just as Bohmian mechanics) an example of *a quantum theory without observers*, a consistent theoretical scheme which can accommodate all what we know about natural processes at the micro as well as at the macro-level.

It goes without saying that if one wants, within a genuinely Hilbert space formalism, to contemplate the idea of a universal dynamical mechanism leading to reduction processes one has first of all to face what has been denoted as the *problem of the preferred basis*: within which linear manifolds should the reduction mechanism drive the statevector? The second problem is the one of the so-called *trigger mechanism*: the modifications should have a negligible impact for small systems but nevertheless they should suppress in a very efficient way the macroscopic superpositions. That this is actually possible is not at all obvious. For instance A Einstein, when contemplating, in his famous *Reply to critics* [57], the possibility that macroscopic physical systems have objective properties at all times, has stated:

> the 'real' in physics has to be taken as a type of program, to which we are, however, not forced to cling *a priori*. [Then he added]: No one is likely to be inclined to

> attempt to give up this program within the realm of the 'macroscopic'. [And he concluded]: but the 'macroscopic' and the 'microscopic' are so inter-related that it appears impracticable to give up this program in the microscopic alone.

We see how, in a sense, Einstein considered practically impossible to follow the line of thought characterizing the DRP[15].

Let us come to our proposal [38]. A clear indication about the choice of the preferred basis comes from the fact that the most embarrassing superpositions, at the macrolevel, are those involving different spatial locations of macroscopic objects. Actually, to quote him once more, A Einstein stated [59]:

> A macro-body must always have a quasi-sharply defined position in the objective description of reality.

In [38] we have contemplated the possibility of appropriate spontaneous processes, which occur instantaneously, affecting any elementary constituent of any system. The required trigger mechanism should follow from this basic assumption. The key idea is then the following: each elementary constituent of any system is subjected, at random times with an appropriate average frequency, to random and spontaneous localizations processes (hittings) around appropriate positions. To have a precise mathematical model one must be precise about the above assumptions, specifying HOW the process works, i.e., which modifications of the wave function are induced by the localizations, WHERE they occur, i.e., what governs the distribution in space of the localizations and WHEN, i.e., at what times, they occur. To answer these questions let us consider a system of N distinguishable particles and let us denote by $\Psi(\mathbf{q}_1, \mathbf{q}_2, \ldots, \mathbf{q}_N)$ its wavefunction[16]. Then, if a hitting occurs for the ith particle at point $\mathbf{x}$, the wavefunction changes instantaneously in the following way:

$$\begin{aligned} &\Psi(\mathbf{q}_1, \mathbf{q}_2, \ldots, \mathbf{q}_N) \quad \Rightarrow \quad \Psi_{\mathbf{x}}(\mathbf{q}_1, \mathbf{q}_2, \ldots, \mathbf{q}_N) = \frac{\Phi_{\mathbf{x}}(\mathbf{q}_1, \mathbf{q}_2, \ldots, \mathbf{q}_N)}{\|\Phi_{\mathbf{x}}(\mathbf{q}_1, \mathbf{q}_2, \ldots, \mathbf{q}_N)\|^2}, \\ &\Phi_{\mathbf{x}}(\mathbf{q}_1, \mathbf{q}_2, \ldots, \mathbf{q}_N) = \left[\frac{\alpha}{\pi}\right]^{3/4} \exp\left[-\frac{\alpha}{2}(\mathbf{q}_i - \mathbf{x})^2\right] \Psi(\mathbf{q}_1, \mathbf{q}_2, \ldots, \mathbf{q}_N). \end{aligned} \tag{9}$$

For what concerns where the localizations occur, the probability density $P(\mathbf{x})$ of their taking place at the point $\mathbf{x}$ is assumed to be:

$$P(\mathbf{x}) = \|\Phi_{\mathbf{x}}(\mathbf{q}_1, \mathbf{q}_2, \ldots, \mathbf{q}_N)\|^2, \tag{10}$$

so that hittings occur with a higher probability at those places where, in the standard description, there is a higher probability of finding the particle in a measurement.

Finally, one assumes that the hittings occur at randomly distributed times, according to a Poisson distribution, with mean frequency λ.

To understand intuitively that the above assumptions imply the trigger mechanism, one can consider, for simplicity, the linear superposition $\Psi = \frac{1}{\sqrt{2}}[\Psi_1 + \Psi_2]$ of two states corresponding to different locations of a macroscopic object. In such a case there is a macroscopic number N of particles which are located in macroscopically different positions (this means, see below, at a distance from each other which is much larger than the characteristic localization distance $1/\sqrt{\alpha}$ of the model) when the state is Ψ_1 or Ψ_2. If a localization of any one of the constituents takes place such a constituent is constrained to be either in the spatial region which it occupies when the state is Ψ_1, or in the one corresponding to Ψ_2: accordingly, the wavefunction is

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[15] Also A Shimony [58] has taken a similar attitude. When contemplating the very possibility of dynamical reduction mechanisms he wrote: *Reasonable desiderata for such a theory pull in opposing directions.*

[16] We disregard the spin variables since the hittings do not involve them.

reduced to one of the two states. Obviously, this means that the reduction rate for a macro-object is amplified by a factor N with respect to the one, λ, characterizing the elementary constituents of matter.

Formally one can prove rigorously this result by resorting to the statistical operator formalism. The dynamics which has been described above, when written in this language is, as easily checked:

$$\frac{\mathrm{d}}{\mathrm{d}t}\rho(t) = -\frac{\mathrm{i}}{\hbar}[H, \rho(t)] - \sum_i \lambda(\rho(t) - T_i[\rho(t)]), \tag{11}$$

where:

$$T_i[\rho(t)] = \left(\frac{\alpha}{\pi}\right)^{3/2} \int_{-\infty}^{+\infty} \mathrm{d}\mathbf{x}\, \mathrm{e}^{-(\alpha/2)(\mathbf{q}_i-\mathbf{x})^2} \rho\, \mathrm{e}^{-(\alpha/2)(\mathbf{q}_i-\mathbf{x})^2}. \tag{12}$$

If one is interested in the dynamics of the centre of mass and takes the partial trace on the internal degrees of freedom, one discovers that, in the case in which the Hamiltonian is the sum of the centre of mass and of the internal motion Hamiltonian, respectively, the reduced statistical operator ρ_{CM} of the centre of mass obeys precisely an equation like equation (11) for a single particle (i.e. without the sum over i), with H replaced by the c.o.m. Hamiltonian and the rate λ replaced by a rate $\Lambda = N\lambda$. Moreover, due to the fact that the characteristic localization distance is chosen (see below) much larger than the typical atomic dimensions one can prove [38] that the internal motion is practically unaffected by the localizations.

Taking into account the formal properties we have just discussed, we can now specify the values which we have chosen for the two parameters α and λ of the model[17]:

$$\alpha = 10^{10}\ \mathrm{cm}^{-2} \quad \Rightarrow \quad \frac{1}{\sqrt{\alpha}} = 10^{-5}\ \mathrm{cm}, \qquad \lambda = 10^{-16}\ \mathrm{s}^{-1}. \tag{13}$$

According to these equations, an elementary particle suffers a localization (reducing the extension of the wavefunction to an interval of about 10^{-5} cm) about every 10^{9} years (and this is the reason for which the quantum predictions concerning microsystems are respected) while a macro-object, containing about 10^{23} constituents, ends up in a definite location in 10^{-7} s. J S Bell has commented [39]:

> in the GRW theory Schrödinger's cat is not both dead and alive for more than a split second.

These are the guiding ideas of the DRP. I will not spend time in analysing all interesting features of the model, I will simply recall that it has been proved [61] to yield a perfectly consistent and satisfactory solution of all problems affecting the standard theory[18]. However there is an extremely important point I cannot avoid mentioning. The model, as originally formulated, deals with systems of distinguishable particles and it does not respect the symmetry properties required by the identity of the constituents of a composite system. The generalization to cover such a case was obviously necessary. Rimini and myself discussed this problem for the first time with J S Bell at the conference celebrating Scrödinger's centenary and all together we have outlined a solution (on a paper towel) in which the hitting processes induced reductions to states containing a randomly chosen number of (identical) particles in a volume of about 10^{-15} cm^3 around a randomly chosen position $\mathbf{r}$. This approach

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[17] Quite recently [60], S Adler, in order to guarantee that the very formation of a latent image in a photographic process leads to reduction, has contemplated the possibility of changing radically the values of the parameters and has investigated in great detail the practical implications of this choice, showing that no contradiction with established experimental facts arises.

[18] See, in particular, the Physics Reports review paper by A Bassi and myself.

required the introduction of a new constant characterizing the rate of suppression of states containing a different number of particles in the considered volume element, and this feature was worrying Alberto and myself, while it seemed not to disturb John. Actually, I remember that subsequently, when I met Bell in Padua where he was delivering a seminar, he told me: *either you publish the model for identical particles or I will do it!*. Meanwhile, our group had proposed an alternative approach to the problem [62] which however had various unsatisfactory features, A Kent [63] worked out a model which, under the request of Bell, I have studied and I proved to present faster than light effects (private correspondence between me and John) and, finally, C Dove and E J Squires [64] have advanced a proposal with the same limitation. Quite recently, R Tumulka [65] has exhibited an extremely simple and fully satisfactory way of building up a hitting theory for systems of identical particles. For what concerns our group, we have stopped considering this problem when P Pearle [66], has made the important step of working out a continuous model with spontaneous localizations (CSL) which turned out to allow [67] a simple and natural treatment of system containing identical constituents[19].

6.2. Continuous dynamical reduction models

In the years following the publication of our paper [38] I had the opportunity of interacting repeatedly with J S Bell. Besides the many lessons, human and scientific, I got from him, he has triggered a quite important process. Philip Pearle had always been extremely interested in the macro-objectification problem and had suggested [68], since 1976, that a way to solve it might be to choose a stochastic dynamical equation as the fundamental equation replacing the standard one. He had obtained many interesting results but he had not identified the appropriate preferred basis for the reducing dynamics. We were not aware of his important contributions and he had not paid the necessary attention to our work, believing that it was simply based on decoherence instead of proposing a new evolution mechanism. He intended to spend one sabbatical year in Geneva and wrote to John who suggested him and asked us whether he might spend his time in Trieste. John used extremely nice expressions of appreciation for his work and we, Rimini and myself, arranged that Philip might spend one year partly in Trieste and partly in Pavia (where Rimini had his position). Pearle was certainly one of the greatest experts of the world on stochastic differential equations and he had a great experience in stochastic attempts to solve the measurement problem. So, in the period he spent in Italy he realized that our choice for the preferred basis fitted perfectly in his line of research, and he worked out [66] an extremely elegant differential stochastic equation with continuous spatial localizations as the basic ingredients, an equation which from a physical point of view had just the same nice features of our model. This has been an extremely important step for the DRP because the CSL model became the starting point for many later developments, mainly due to Pearle himself [69], but gave also rise to an extremely fruitful collaboration between him and our group. I recall the already mentioned nice solution it yielded for the problem of systems with identical constituents, the proof [70] that empirical evidence required to make

[19] R Tumulka has stated various times that I seem to believe that to deal with identical constituents a model contemplating continuous spontaneous localizations in place of discrete hittings is essential. I agree that some phrasing in my writings might have given this impression, but I have absolutely clear that this is not the case, as proved by what I have just said. Actually in the paper on CSL with P Pearle and A Rimini [67] we have stressed in general the physical equivalence between discrete and continuous models. Probably, due to the formal elegance of Pearle's approach I have, in a sense, associated his CSL model with the problem of identical particles and used misleading expressions on this point. Actually I must say that in that period I have discussed the discrete versus continuous option various times with J S Bell, and I recall he was making systematic reference to the discrete one since he wanted to base on it the 'flash ontology' (see below) he had taken about the DRP.

the time reduction rate of the model proportional to the mass of the involved particles, and the fact that Pearle himself has taken [71] the CSL model as the starting point to present the first interesting attempt of a relativistic generalization of dynamical equations inducing reductions.

Before proceeding there is a point that I must mention since it will be relevant for my future analysis. Pearle himself, Rimini and myself have been able to prove [67] that, for any model like the one proposed by Pearle based on a stochastic differential equation describing continuous spontaneous localizations, there is a corresponding discrete model, á la hittings, just as our original model, which induces an evolution as close as one wants to the one of the continuous model.

6.3. The interpretation of dynamical reduction models and the search for an appropriate ontology

The 'rules of the game' presented in the two previous subsections represent, according to many theorists, all there is to say about the so-called GRW theory and CSL. However, both J S Bell and myself believed that this cannot be the whole story, in agreement with our shared opinion that more strict and precise requirements than the purely formal ones must be imposed to any theory to be taken seriously as a fundamental description of natural processes. This necessity of going beyond the pure formal aspects has been denoted as the adoption of the Primitive Ontology (PO) in a nice recent paper [72]. The fundamental request for the PO is that of being absolutely precise concerning what the theory is fundamentally about.

Bell had always maintained such a position and to stress it he had introduced the expression *be*-ables (as opposed to *observ*-ables), a term, as he stated, derived from the verb 'to be', 'to exist', which he made more precise by writing [73]:

> In your theory you must identify some things as being really there, as distinct from the many mathematical concepts you can easily devise . . . We must decide that some things are really there and that you are going to take them seriously.

We could rephrase this request by stating that the elements of the PO are the stuff that things are made of. Accordingly, it should be clear that while the wavefunction also belongs to the ontology of the Dynamical Reduction Models, it does not to its PO: objects are not made of wavefunctions. In GRW-like theories, as well as in Bohmian mechanics, the role of the wavefunction is simply that of 'telling the matter how to move'.

Probably I went too far in this analysis, so, let me step back to reconsider the evolution of the DRP in order to see which POs have been proposed for it. In the first paper he wrote [39] about our model J S Bell has indicated clearly what he considered the associates beables, making in this way a precise choice concerning the PO of the theory. In GRW the spontaneous localizations of the particles take place at precise places and at precise times (governed by probabilistic laws). Thus one can take these spacetime events as the fundamental quantities described by the theory. Let us resort to John Bell's words:

> There is nothing in this theory but the wavefunction. It is in the wavefunction that we must find an image of the physical world, and, in particular, of the arrangement of things in ordinary 3-dimensional space. But the wavefunction lives in a much bigger space, of 3N-dimensions. . . . However, the GRW-jumps (which are part of the wavefunction, not something else) are well localized in ordinary space. . . . So we can propose these events as the basis of the 'local beables' of the theory. . . . A piece of matter then is a galaxy of such events. As a schematic psychophysical parallelism we can suppose that our personal experience is more or less directly of events in

> particular pieces of matter, our brains, which events are in turn correlated with events in our body as a whole, and they in turn with events in the outer world.

In [72] this PO has been denoted as 'the flash ontology'.

Subsequently, Bell himself seems to have made a step back, taking a rather vague position which has seriously worried me and which I did never understand. This position has been considered by the authors of [72] simply as leaving the theory without any ontology. At the Erice meeting in 1989, Bell stated [2]:

> The GRW-type theories have nothing in their kinematics but the wavefunction. It gives the density (in a multidimensional configuration space!) of stuff. To account for the narrowness of that stuff in macroscopic dimensions, the linear Schrödinger equation has to be modified, in the GRW picture by a mathematically prescribed spontaneous collapse mechanism.

We have discussed this point repeatedly. On October 3, 1989 he wrote to me:

> As regards Ψ and the density of stuff, I think it is important that this density is in the 3N-dimensional configuration space. So I have not thought of relating it to ordinary matter or charge density in 3-space. Even for one particle I think one would have problems with the latter. So I am inclined to the view you mention: 'as it is sufficient for an objective interpretation . . . ' And it has to be stressed that the 'stuff' is in 3N-space—or whatever corresponds in field theory.

The repeated exchanges of view I had with him and my dissatisfaction about what seemed to me a position at odds with his previous firm requests for any 'serious theory', stimulated me, together with my collaborators R Grassi and F Benatti [74], to work out a proposal of an interpretation of the theory which in [72] has been denoted as 'the matter density ontology'. It amounts to claim that the PO is given by a field, i.e. a variable $m(\mathbf{x}, t)$ for every point $\mathbf{x} \in \mathbb{R}^3$ in ordinary space and any time t, which is defined as[20]:

$$m(\mathbf{x}, t) = \sum_{i=1}^{N} m_i \int_{\mathbb{R}^{3N}} \mathrm{d}\mathbf{q}_1 \mathbf{q}_2 \cdots \mathrm{d}\mathbf{q}_N \delta^3(\mathbf{q}_i - \mathbf{x}) |\Psi(\mathbf{q}_1, \mathbf{q}_2, \ldots, \mathbf{q}_N, t)|^2. \quad (14)$$

Obviously, the field $m(\mathbf{x}, t)$ which represents the density of matter in space at time t, is a functional of the wavefunction and it is completely determined by it; nonetheless it represents an additional element which need to be posited in order to have a complete and consistent description of the world. The microscopic description provided by the matter density field $m(\cdot, t)$ is not particle like as in Bohmian mechanics; it is reminiscent of Schrödinger's early view of the wavefunction as representing a continuous matter field. With reference to this important point I would like to stress that Bell's claim 'even for one particle I think one would have problems with the latter' seems to ignore the real physical implications of the GRW theory and an important remark that he had done precisely in the same paper in which he proposed the density of stuff interpretation. In fact he had stated [2]:

> In the beginning, Schrödinger tried to interpret his wavefunction as giving somehow the density of stuff of which the world is made. He tried to think of an electron as represented by a wavepacket . . . a wavefunction appreciably different form zero only owe a small region in space. The extension of that region he thought of as the actual size of the electron . . . his electron was a little bit fuzzy. At first he thought that small wavepacket, evolving according to the Schrödinger equation, would remain

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[20] The original definition, given in a more complicated context was slightly different from this one, but the difference is totally irrelevant for our purposes.

> small. But that was wrong. Wavepacket diffuse, and with the passage of time become indefinitely extended, according to Schrödinger equation. But however far the wavefunction has extended, the reaction of a detector to an electron remains spotty. So Schrödinger's 'realistic' interpretation of his wavefunction did not survive.

This is a perfectly correct statement. But what this great thinker seems not having fully appreciated is that, in spite of the fact that also within GRW 'wave packet diffuse' and, accordingly, the field $m(\mathbf{x}, t)$ for an elementary particle such as an electron might be appreciably different from zero over a quite spread region, the very dynamics of the GRW model implies that the *'reaction of a detector to an electron remains spotty'*, so that there is no reason to abandon the matter density interpretation within such a theory, while such an interpretation cannot be maintained within standard QM due to the fundamentally different dynamics for macroscopic objects (like detectors).

Actually, we have discussed [59] in great detail all implications of adopting the suggested mass density PO, and we have shown that it allows, actually it imposes on us, a perfectly consistent view of natural processes matching perfectly our definite perceptions at the macroscopic level. So, there is no reason to abandon this ontology. Moreover, in the lucid and deep paper by V Allori *et al* [72], it is shown that the mass density ontology presents some advantages with respect to the flash ontology and it also allows us to identify interesting strict similarities between Bohmian mechanics and the GRW theory.

Before concluding this section I must mention another important fact. D Albert and L Vaidman [75] have challenged the DRP by considering a situation in which the extreme sensitivity of our perceptual visual apparatus yields a definite perception in spite of the fact that the processes which are involved do not satisfy the conditions for which the dynamical reduction mechanism can become effective. They consider a situation in which our eye is stimulated by a state which is the superposition of two microstates which, by themselves, would trigger different perceptual responses. In spite of that, they argue, a conscious being will have in any case a definite perception, showing that in specific situations even the GRW theory must invoke consciousness to account for our perceptions.

We have taken this challenge and we have shown [76] that if one takes into account the transmission of the nervous signal from the retina to the lateral geniculate body and to the higher visual cortex, the very process taking place in the brain implies the occurrence of a superposition of two states corresponding to such a number of particles (sodium and potassium ions going through the Ranvier nodes to transmit the nervous stimulus) in different positions that the fundamental dynamical features of the model imply that the superposition is dynamically suppressed within the perceptual times.

I do not want to commit myself to this conclusion which requires to have such a confidence in the appropriateness of the theory to feel comfortable in applying it to a situation of which so little is known as the perceptual process, but I cannot avoid remarking that if one is ready to adventure himself in such a controversial issue, the theory accounts for our definite perceptions on the basis of the suppression of superpositions implied by it, even when one chooses to deal with the brain as a physical system governed by the new dynamical laws.

6.4. The relativistic challenge

As soon as our proposal appeared and attracted the attention of J S Bell it also stimulated him to look at it from the point of view of relativity theory. As he stated subsequently [73]:

> when I saw this theory first, I thought that I could blow it out of the water, by showing that it was *grossly* in violation of Lorentz invariance. That's connected with the problem of 'quantum entanglement', the EPR paradox.

Accordingly, he started investigating this point by studying the effect on the theory of a transformation mimicking a non-relativistic approximation of a Lorentz transformation and he came out with a surprising conclusion [39]:

> . . . the model is as Lorentz invariant as it could be in its nonrelativistic version. It takes away the ground of my fears that any exact formulation of quantum mechanics must conflict with fundamental Lorentz invariance.

What Bell has actually proved in a rather complicated way by resorting to a two-times formulation of the Schrödinger equation is that the model violates locality by violating Outcome Dependence and not, as deterministic Hidden Variable Theories do, Parameter Dependence. We have proved rigorously this fact in subsequent papers [77]. But even though there is no reason of principle which forbids the relativistic generalization of the model, this does not mean that one can get such a result easily. Actually, since 1989 there have been many unsuccessful attempts to get this interesting result. I will briefly summarize some of the steps in this direction.

P Pearle [71] was the first to suggest a relativistic generalization to the case of a quantum field theory describing a fermion field coupled to a meson scalar field enriched with the introduction of stochastic and nonlinear terms. A quite detailed discussion of this proposal has been presented in [78]. In it it has been shown that the theory enjoys formally of all properties which are necessary in order to meet the relativistic requirements[21]. The procedure has required the precise formulation of the idea of stochastic Lorentz invariance. The essential ingredients of the approach are the following: one considers a Tomonaga–Schwinger equation for the statevector with a coupling between the fermion and meson fields. As we all know this implies that the fermions become 'dressed' and that the virtual mesonic cloud associated with a fermion at one space point differs from the one associated with the same fermion at a different point. One then introduces an appropriate reduction mechanism which tends to suppress the superpositions of different mesonic clouds, inducing in this way an indirect space-localization of the fermion. The game works perfectly at the formal level but it meets an unsurmountable difficulty related to the appearance of untractable divergences which find their origin in the stochastic process which has been added to the standard field theory. There are various reasons for this, some of them are lucidly discussed in the paper [20] by Bell included in this special issue, to which we refer the interested reader.

The years which have followed these first attempts have seen a flourishing of researches aimed to get the desired result, most of them having been proposed by P Pearle himself. These attempts make reference to the Tomonaga–Schwinger approach because it appears as the most natural candidate for getting the desired result. In following such an approach one must be extremely careful in order to guarantee that the Tomonaga–Schwinger equation turns out to be integrable, an absolutely necessary condition. There are indications that probably the crucial element giving rise to divergences is the adoption of a noise which is white in character and for this reason P Pearle [81], L Diósi [82] and A Bassi and myself [83] have reconsidered the problem from the beginning by investigating nonrelativistic theories with nonwhite Gaussian noises.

It is interesting to remark that precisely in the same years similar attempts at a relativistic generalization of the other existing 'exact' theory, i.e., Bohmian mechanics were going on and

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[21] In various papers [79] I have discussed, with reference to some toys models mimicking the DR-ones, the features of a relativistic theory inducing reductions, I have made plausible that, in principle, there should be no difficulty in building such a theory and I have analysed its implications for micro and macrosystems, by making reference, among other things, to the deep contributions of Aharonov and Albert [80] to the problem of relativistic dynamical reduction. I refer the reader to the original papers for a detailed discussion of some of the basic aspects of the problem.

that they too have encountered serious difficulties. Important steps are represented by a paper [84] resorting to a preferred spacelike slicing, by the investigations of Goldstein and Tumulka [85] and by other scientists [86]. For comments on these researches we refer the reader to [87]. However, we have to mention that no one of these attempts has led to a fully satisfactory solution of the problem of having a theory without observers, like Bohmian mechanics, which is perfectly satisfactory from the point of view of relativity.

Let us come back to the relativistic DRP. With reference to it I will mention an attempt by Dove and Squires [88] based on discrete processes and one by Dewdney and Horton [89] formulated on a discrete spacetime. All other attempts towards a relativistic collapse model are based on a continuous spontaneous localization description of the localization processes. Among them mention must be made of the serious attempts by Pearle [90] and by Nicrosini and Rimini [91], which represent interesting steps towards a solution.

An important change took place quite recently due to the systematic and serious investigations by R Tumulka. He first was able to write down [87] a relativistic version of the GRW model for N non-interacting distinguishable particles based on the consideration of a multi-time wavefunction whose evolution is governed by the Dirac equation and whose PO is the flashes ontology. To my knowledge this is the first proposal of a reduction model which is fully compatible with relativity. His second step has been to present [92] a quantum field theoretical dynamical reduction model, and presently he is trying to combine the two approaches to have a fully relativistic field theoretical theory without observers. The conclusion that this author reaches in [87] is extremely interesting:

> A somewhat surprising feature of the present situation is that we seem to arrive at the following alternative: Bohmian mechanics shows that one can explain quantum mechanics, exactly and completely, if one is willing to pay with using a preferred slicing of spacetime; our model suggests that one should be able to avoid a preferred slicing if one is willing to pay with a certain deviation from quantum mechanics.

a conclusion which he has rephrased and reinforced in [65]:

> Thus, with the presently available models we have the alternative: either the conventional understanding of relativity is not right, or quantum mechanics is not exact

To conclude let me come back to the most recent positions of the scientific community about the foundational problems of quantum mechanics.

7. The present status of the debate on foundations

In this paper I have outlined some crucial problems of quantum mechanics and, by making reference to my personal experience, I have pointed out some important changes concerning the position of the scientific community about such problems. As I have stated, when I entered the field there was a shared skepticism about foundational investigations. The important commitment of various brilliant scientists and the fundamental contributions by J S Bell have changed in a remarkable manner the situation. In the subsequent period, 1970–1995, the (unappropriate) idea that the basic problems of the theory are due to philosophical prejudices was gradually abandoned and replaced by the shared conviction that they represent precise physical challenges which must be faced and require serious work by theoretical physicists rather than debates by philosophers.

Starting from the 1980s, but particularly after 1995, an important new line of research emerged and gave rise to a renewed interest in some precise aspects of quantum mechanics, in particular its linear structure, entanglement and nonlocality. Various brilliant physicists contemplated the possibility of fully exploiting these peculiar aspects of the theory in order to achieve important technological advances. A relevant boost in this direction had been given by R P Feynmann who had raised the question of whether the quantum aspects of natural processes might play some important role for computer technology. It was also suggested that one might exploit the genuine probabilistic nature of natural processes to generate truly random sequences to be used as the basis for implementing unbreakable cryptographic protocols.

With these motivations, a group of high level scientists, including D Deutsch, C H Bennet, G Brassard, A Ekert, P W Shor and others, has launched an extremely interesting line of investigation which has led to the practical implementation of quantum cryptographic methods and to the elaboration of quantum algorithms which seem to make attainable unexpected and absolutely innovative results in the field of quantum information and quantum computation. I will not comment on this point: I refer the reader to the exhaustive book by M A Nielsen and I L Chung [93].

This promising line of research covering the areas of quantum cryptography, quantum teleportation, quantum information and quantum computation has also triggered a series of researches by experimental physicists aiming to the practical realization of the quantum devices (q-bits) which should replace the classical components (bits) of classical circuits. These researches are flourishing and had a remarkable impact even in fields lying outside the one which has motivated them.

Concerning these important new lines of research it has to be stressed that most of them make a systematic use of EPR-like set-ups and take full advantage of quantum entanglement. This is extremely interesting, particularly from the perspective of somebody who is reconsidering the evolution of the field over a period covering more than 70 years (those which have elapsed since the EPR paper appeared). In fact one cannot forget that this paper has been considered for quite a long time as a useless paper, motivated mainly by the epistemological prejudices of Albert Einstein; it is therefore quite comforting, for people seriously interested in the conceptual foundations of physical theories to see that investigations in this field, besides having triggered important conceptual advances are also leading to unexpected and quite innovative technological developments.

Our group which, as discussed in this paper, had been seriously involved in researches about entanglement and nonlocality has been stimulated to reconsider these aspects of QM. In particular, we have discussed and clarified [94] the subtle implications of entanglement in the case of systems with identical constituents (an argument about which gross misunderstandings appear in the literature) and we have paid a certain attention to new approaches to the problem of nonlocality [95]. I will not discuss our achievements and I will come back now to my main subject, the present status and the position of the scientific community about the foundations of the theory.

Concerning this point, it seems to me that one must recognize that, in spite of the many positive and exciting aspects and achievements of the just mentioned investigations, they had a quite negative effect for the investigations on the foundational problems of QM. I would dare to say that, in a sense, the debate on such issues is back to the level of the years in which I was graduated, years characterized by a certain hostility concerning epistemological investigations, by a great superficiality in dealing with serious problems, and by a complacency which has led to an uncritical acceptance of ‘easy solutions’ to the problems which affect the theory.

To make reference once more to my personal experience I want to mention that on the occasion of the tenth anniversary of Bell’s death a meeting was organized in Vienna to celebrate this great thinker. In that meeting the convinced and sincere appreciation for the derivation of his celebrated inequality has been accompanied, with few exceptions, by extremely severe criticisms about his positions concerning the conceptual status of the theory. This is perfectly in line with the new positions which are emerging as a consequence of the enormous interest in quantum computation.

Obviously, the main objectives of computational techniques are the storage, manipulation and transmission of information. This fact has given rise to a quite peculiar position with respect to the meaning and value of quantum mechanics, a position which can be summarized in the assertion that the very object of the theory, its referent, is not something existing objectively and independent from the observer, but simply information itself[22].

To make clear the new 'philosophy' which characterizes the community working in quantum information I will make explicitly reference to some statements which appear in the proceedings of the just mentioned Vienna meeting, proceedings which have been published [96] under the significative title of *Quantum [Un]speakable: from Bell to Quantum Information*. Probably the paper which, in the intentions of the author, should make absolutely clear and precise the innovative position concerning QM implied by the new discoveries is the one by A Zeilinger: *Bell's theorem, information and quantum theory*. In the conclusive section of the article the author makes clear that he does not agree with John Bell about the meaning and the interpretation of quantum mechanics and he makes clear his point of view:

> . . . it is suggested that *information* is the most basic notion of quantum mechanics, and it is *information* about possible *measurement* results that is represented in the quantum states. *Measurement* results are nothing more than states of the classical *apparatus* used by the experimentalist. The quantum *system* then is nothing other than the consistently constructed referent of the *information* represented in the quantum state.

The first thing which comes to the mind when reading such a sentence is its strict relations with the old statements by Bohr and Heisenberg concerning the fact that classical mechanics must be taken as a necessary logical prerequisite of quantum mechanics, and that it is wrong to attribute to quantum objects any degree of direct physical reality. But today one cannot ignore that the whole community of great scientists who have been involved in the debate about the interpretation of the theory for more than 70 years, has almost unanimously reached the conclusion that the orthodox way out is basically inconsistent and unsatisfactory. The natural and obvious remark that any serious scientist would make when reading the above statement is that assertions like those reported above are of no value unless one makes absolutely precise, in scientific terms and not resorting to empty verbal assertions, what is classical and what is quantum.

Actually, Zeilinger seems to have contemplated this aspect of the problem, since he continues:

> The point where I agree with John Bell is that *microscopic* and *macroscopic* should not command any fundamental place in any physical theory. Experimental progress will certainly make it possible to push the regime where quantum phenomena have been demonstrated very far into what we would consider *macroscopic*. Thus, while the dichotomy *microscopic–macroscopic* should not have any place in a physical theory, the dichotomy *quantum-classical* is a most fundamental one.

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[22] We recall that some of the supporters of the decoherent histories approach, in particular Gell–Mann and Hartle, had already suggested to take what they have admitted to be a subjective point of view, i.e., to assume that the referents of the decoherent histories approach are the IGUSes (Information Gathering and Utilizing Systems), i.e. complex adaptive systems such as, e.g., human beings, which are able to interact with the surrounding environment, gather and elaborate the information coming from it. We also recall the sharp reaction of J S Bell to this idea summarized in his pretensions that before considering such solutions one should make precise his position concerning the fundamental questions: *information about what?* and *information by whom?*

At this point I must confess that I am extremely puzzled. Having played the game (naturally imposed by the notorious fact that there are macroscopic systems which exhibit a genuine quantum behaviour) of shifting the boundary line from *micro-macro* to *quantum-classical* does not help in any way in locating such a boundary in a scientifically acceptable manner. But something more must be said. The above sentence shows clearly that the author believes that this boundary is fundamentally shifty and that it depends crucially on technological developments. A question naturally raises: if one would succeed to *push the regime where quantum phenomena have been demonstrated* up to the point of encompassing all natural processes (or at least measuring apparatuses), what will remain of the proposed interpretation? At any rate to ground the interpretation of a theory on the technological devices one is (or will be) able to implement seems to me an untenable position.

This attitude of the adherents to what I will call the *quantum information interpretation* of quantum mechanics emerges again and again in the recent literature on the subject. An interesting example is the recent paper by the same author [97] on a prestigious journal. I refer the reader to this paper and to the sharp criticisms [98] by Daumer *et al* which have been recently published to grasp the precise terms of the present debate on QM.

I also feel the duty to call the attention of the reader on the fact that the now outlined change of attitude about the interpretation of QM has been accompanied by the strong critical position, typical of the old times of the debate about the theory, towards the foundational and epistemological investigations on QM. For example, in *Quantum [Unspeakable]* there is an article by D Mermin (a scientist who I have seen many times—and he honestly admits that—on the same positions of John Bell) in which he claims:

> Until quite recently I was entirely on Bell's side on the matter of knowledge-information. But then I fell into bad company. I started hanging out with the quantum computation crowd, for many of whom quantum mechanics is self-evidently and unproblematically all about information. After having raised the question of what would have been Bell's reaction to the recent developments in quantum computation he goes on: partly from my association with quantum computer scientists and partly from endless debates with constructivist sociologists of science, I have come to feel that 'Information about *what*?' is a fundamentally metaphysical question that ought not to distract tough-minded physicists. There is no way to settle a dispute over whether the information is about something objective, or is merely information about other information. Ultimately it is a matter of taste, and, like many matter of taste, capable of arousing strong emotions, but in the end not really very interesting.

My natural reaction to such statements can be expressed by recalling a recent sentence taken from the paper by Daumer *et al* which I have mentioned few lines above:

> The very concepts of knowledge and information imply a special kind of relationship between different things, appropriate correlations between a knower and what is known. Thus the distinction between reality and our knowledge of reality not only can be made; *it must be made if the notions of knowledge and information are to have any meaning in the first place*.

These remarks conclude my reflections on the debate concerning our best theory, quantum mechanics. This debate has raised my passionate interest in the many years I have been involved in researches on this subject. As I have mentioned the foundational investigations on QM have been looked at with a variable interest by the scientific community. But even though I have the impression that presently old prejudices and a superficial attitude are emerging once more, I am convinced that this essential (in my opinion) debate will involve many of the brilliant young scientists who, fortunately, are taking the place of people who (like in my case) have reached an age in which they can at best follow rather than contributing to the new developments.

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