Towards a Proper Quantum Theory

(Hints for a recasting)

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Summary

The history of quantum physics has been deeply conditioned by the change in scientific practice as a social activity during the past fifty years. As a result the theory has not been allowed full maturing; both its formal and empirical advances have not resulted in a comparable conceptual progress. The *recasting* of quantum theory thus appears as an epistemological necessity. One of the main aspects of this process is to clear quantum theory from its persisting classical connections in order to endow it with an autonomous and intrinsic status. Problems related to the foundations, description, interpretation and approximations of quantum theory are considered in turn, and various recent works are reviewed which contribute to the proposed endeavour.

Résumé

L'histoire de la théorie quantique a été fortement conditionnée par les changements intervenus au cours des cinquante dernières années dans la pratique scientifique en tant qu'activité sociale. La théorie en conséquence n'a pu atteindre sa pleine maturité et ses développements à la fois formels et empiriques n'ont pas conduit à des progrès conceptuels comparables. La *refonte* de la théorie quantique apparaît ainsi comme une nécessité épistémologique. L'un des principaux aspects de ce processus consiste à débarrasser la théorie quantique de ses persistantes attaches classiques, de façon à lui conférer un statut autonome et intrinsèque. On considère ici tour à tour les problèmes de fondements, de terminologie, d'interprétation et de limites, de la théorie quantique, tout en passant en revue divers travaux récents contribuant à l'entreprise proposée.

Zusammenfassung

Die Geschichte der Quantenphysik ist in entscheidender Weise durch die Veränderungen, die in den letzten fünfzig Jahren in der wissenschaftlichen Praxis als einer sozialen Tätigkeit eingetreten sind, bestimmt worden. Die Theorie hat deshalb ihre volle Reife nicht erlangen können; ihre zugleich formalen und empirischen Entfaltungen haben nicht zu einem entsprechenden begrifflichen Fortschritt geführt. Eine Umgestaltung der Quantentheorie muss daher als ein erkenntnistheoretisches Desiderat betrachtet werden. Einer der wichtigsten Aspekte eines derartigen Unterfangens wird darin bestehen, die Theorie von ihren hartnäckigen Bindungen an klassische Begriffe zu befreien, um ihr einen eigenständigen und autonomen Status zu verleihen. Es werden die Probleme der Begründung, der Terminologie, der Interpretation und der Grenzen der Quantentheorie diskutiert, indem auch verschiedene kürzlich erschienene Arbeiten, die sich auf das vorgeschlagene Unternehmen beziehen, berücksichtigt werden.

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> " Up to now, philosophers have only interpreted quantum theory. The point, however, is to transform it ".
> (after the " Theses on Feuerbach ").

TABLE

Introduction:

Fifty years old, and not yet grown-up!

Despite the festive character of our gathering today *, aimed at celebrating " half a century of quantum mechanics ", let me take the risk of asking a few indecorous questions. The chronological reference of this Colloquium, to start with, might be worth some considering. Could'nt we imagine that analogous Colloquiums, with similar titles, were or will be held in Berlin in 1950, Zürich in 1955, Manchester in 1963, Göttingen in 1975, Cambridge in 1975 too, Vienna in 1976, etc., celebrating various possible birthdays of quantum mechanics (1), comparable in importance to today's one (2)? In fact, we know well that *none* of these dates by itself could fully symbolize the breaking forth of a new physics. It would not be sufficient either to list the succession of these dates, would it be in detail, to account for this emergence. History cannot be reduced to chronology. Far from being a sequential enumeration of events, a cumulative description of linear processes, it requires a retroactive analysis, a critical point of view. The history of sciences, as any history, cannot but be written in the present tense. In other terms, the history of sciences itself has an history, as may be proven by the title of our Colloquium; celebrating the jubilee of the foundation of

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a "*wave* mechanics", we take into account, rightly but implicitly, the practice of these past fifty years, in modifying a limited and inadequate terminology to replace it by a more generally valid one, so that we now speak of "*quantum* mechanics". This sensible unfaithfulness to the very work which motivated celebration, may be understood from a general point of view. Indeed, the history of a scientific field does not close with the end of its springing up period. Quantum mechanics was established during the first quarter of this century, through a scientific activity sometimes considered as "revolutionary"; one should not conclude, despite some appearances, that the following half-century, leading us at the present day, only saw a "normal" activity, consisting of merely applying a "paradigm" set up by the great masters[1]. By the very fact that any new physical theory is born in a difficult breaking off with the preceding ones, it still bears their stamp: as ever, the new for a long time shows the mark of the old. Well after the emergence and development of a new theory, there remains various contradictions between, on the one hand, its intrinsic structure and conceptualization (such as they keep appearing with an increasing clarity), and, on the other hand, the temporary forms it could not but borrow. I will describe below several examples of this phenomenon for the case of quantum mechanics. It is the effect of the experimental and theoretical practices within the new field to "transform it into itself", by progressive elimination of its archaic and irrelevant notions and formulations. This *recasting* process by no means is less important, historically speaking, than the more spectacular breaking off which precedes and allows it (4).

The importance here of these general considerations comes from the rather paradoxical situation of quantum theory in that respect. The most recent of the great theoretical syntheses of physics, this last-born child is a backward one. It looks as if the recasting process, as described above, had not really taken place for quantum physics, or, at least, had remained in a mostly implicit stage. That our Colloquium will spend much of its time debating some of the same basic epistemological problems that were already discussed fifty years ago, may be taken as evidence that very little recasting indeed has been achieved. A detailed study of most text-books in quantum physics could yield another proof; the deeply repetitive character of these books with respect to one another, the absence of any modernizing in the terminology as well as in the description of the theoretical structure or in the discussion of the fundamental concepts, express, it seems to me, a state of sclerosis without precedent in the history of physics.

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[1] I take issue here with T. Kuhn's ideas on the history of science (3).

For I doubt that, if a Colloquium was held in 1915 to celebrate " half a century of electromagnetism "[2] (5), it included discussions about the properties of the ether, the physical reality of the " displacement current " (even though this execrable terminology has been maintained), or the interpretation of Hertz's experiments. Through these fifty years, a thorough recasting of electromagnetism had been achieved; the field concept had emerged, the spatio-temporal framework of the theory had been brought to light (if I dare say so) by einsteinian relativity, the formulation of Maxwell's equations had been deepened and tightened. A comparison between Maxwell's first papers and textbooks of the twenties bears a clear testimony in that respect. Similar statements could be made for the good old " classical " mechanics, or thermodynamics, etc. Of course, it must be emphasized that none of these recasting processes yet should be considered as closed; even though the domains of validity of such ancient physical theories may be well defined by now, their internal structure keeps modifying under the the influence of the new theoretical syntheses which overtake and extend them. As an example, the role of symmetry principles and invariance considerations, come to the foreground of quantum theory (see below), has also taken a great importance in the re-formulation of more " classical " theories. At least, it can be said that, up to a recent past, these recasting processes in physics had not met with major obstacles.

What, then, is the nature of these obstacles which have maintained the recasting of quantum theory to a late, superficial and insufficient development?

They derive, I believe, from the particular historical situation in which quantum physics was born. Two related features are of importance here: 1) the upsetting of scientific *practice* as a social activity, 2) the change in the philosophical (not to say ideological) conditions for the elaboration of *theory*. As to the first of these two topics, it is to be emphasized that the end of the first quarter of this century marks precisely enough the boundary between two modes of production of scientific knowledge. The ancient mode was essentially one of craftmanship. It was based on the individual skill of scholars, working either in isolation or surrounded with a few pupils and students; hierarchical relationship were of the patronizing type, the values were devotion to progress, scientific integrity, humanitarian ascesis and ethics of knowledge (I am dealing here with " values ", that is with the latent or

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[2] The date was not that convenient, obviously, and, as thirty years later, science was drafted on the battlefield rather than celebrated in Colloquiums...

patent ideology of the scientific milieu, not with its real functioning, sometimes mean enough and in any case rather trite). After the First World War begins a change which has kept deepening to this day. Increasing weight of the state through the funding and organizing of fundamental research (not to speak of its seizure by the Army), industrialization of the management and administration of science, hierarchizing, division of work, parcellisation of tasks, and, in particular, partitioning of fields, separation of theory and experiment; these are the main features of science today, specially pronounced in the case of contemporary physics (6). These socio-political phenomena have deep consequences on scientific activity at its most " internal " level, although some persist in thinking of it as neutral and pure. The division of scientific work, with the ensuing separation between tasks of 1) fundamental research (study of new concepts and phenomena), 2) " fundamental development " (exploration and exploitation of the theoretical and experimental domains opened by fundamental research), and 3) teaching (in the broad sense, that is, spreading of scientific knowledge, including popularization) for instance, has impeded the recasting processes of modern physics. For it is usually through development and teaching that new theories are faced with practices which may first dissolve their archaic attle and then restructure them on a specific basis, under the condition, however, that these practices may act through a suitable theoretical feedback. The separation of the various scientific activities hinders the dialectics of such a process. While it was natural and implicit in the former mode of scientific research, recasting today can be but a specific activity, explicit and determined; it cannot escape from the very division of tasks that it criticizes. To state this contradiction rather than to ignore it, to use it as a tool rather than to be victim of it, such is my intention here.

The second feature of the particular conditions surrounding the springing up of quantum physics is its philosophical context. It is not uncommon that during periods of breakthrough in the history of science, philosophy comes to play an important role (4); the criticism of old concepts, the elaboration of new theories cannot proceed in a purely deductive way from experimental " data ". Such or such philosophical trend can play a role as a motor — or brake. The founding fathers of quantum mechanics thus relied explicitly onto a philosophical point of view which, through its numerous variants, can be said to be a *positivist* one. The major and seminal role played in its time by this philosophical current may be understood easily. By rejecting the intrinsic a priori validity of any previous theoretical concept and subordinating it to empirical investigation, the holders of this point of view could get rid of an apparently compulsory reference to the concepts of classical physics, as these exhibited their limitations. An operationalist approach enabled them to use as much as possible of the available experimental results, in a " phenomenological " way, as to-day high-energy theorists would say. Such a methodology is quite clear in the building by Heisenberg of its " matrix mechanics " from the frequencies of atomic spectra (7). In other words, by relying on a positivist philosophical standpoint as a fulcrum, physicists could break with the iron-collar of another philosophical domination, that of a narrow mechanicist rationalism, which had reigned for several decades. Indeed the first attempts to a working positivism in physics, the ideas of Mach for instance, or of Ostwald, had been concluded by a relative failure; atomism had largely overcome energetism, and the cartesian description of the physical world " par figures et mouvements " appeared unchallenged by the end of the XIXth century. The " crisis " of relativity was but a false alarm and, far from endangering the building of classical physics, Einstein strengthened it by ensuring the consistency of electromagnetism with mechanics, within a reformed space-time. The quantum riddle was somewhat more serious... Indeed, and offering a proof *a contrario* of the fecund importance of the positivist standpoint, those of the founders of quantum physics which stuck to the mechanicist rationalism of classical physics, such as Planck, Einstein, De Broglie, would not lead nor follow the major developments of quantum theory. They would not even accept them, or only to reconsider their opinion later on.

But — and this is my main point — the very same positivist current which had been so efficient to promote the breakthrough leading to the birth of quantum mechanics, rapidly turned into an obstacle, both epistemologically and pedagogically, for its recasting; the cornerstone had become a stumbling block. It will be my purpose in the following pages to try proving this statement. Let me already note here that the philosophical dogmas of the leading school were contradicted by their own supporters in their practice as physicists. For instance, it is Heisenberg himself who, after having emphasized the elimination of " unobservables " elements from theoretical arguments as an epistemological golden rule of quantum physics[3], some years later introduced the S-matrix notion with consider-

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[3] The abstract of his 1925's seminal paper reads. " The present paper seeks to establish a basis for theoretical quantum mechanics founded exclusively upon relationships between quantities which in principle are observable " (7). Ironically enough, Heisenberg used this argument in an entirely mistaken, although most fecund way, to exclude from theoretical considerations " unobservables in principle " quantities such as... the position of an electron! The later development of quantum theory proved this property to be perfectly observable by itself.

ations upon the analyticity of its elements. How could one ever " observe ", or better " measure " directly, an operator in an infinite-dimensional space, and analytic functions (that is, in particular, infinitely differentiable)? In fact, no physical *concept* can be directly measured or observed; as Feynman[4] writes sensibly (note that I do not refer here to philosophers' opinions):

> " It is not true that we can pursue science completely by using only those concepts which are directly subject to experiment. In quantum mechanics itself, there is a probability amplitude, there is a potential, and there are many constructs that we cannot measure directly (. . .) It is absolutely necessary to make constructs " (the whole paragraph is worth reading) (8).

To add one more argument yet for the necessity of the recasting that I advocate, I could propose a careful comparison of the ways a physicist thinks and talks according to whether, on the other hand, he *does* some quantum physics, with colleagues, dealing with his paper block or his apparatus, or, on the other hand, he *teaches* it, to students, in front of a blackboard. It is very rare that he uses, or simply mentions, in the first situation, the general philosophical statements that he steadily repeats in the second one. In other words, within this orthodoxy, as for most, there are many church-goers and few believers. Then, could say some people, the problem is not that serious. It is really worth-while fighting against ideas which are falling into abeyance and which are just paid lip service to? But it is precisely the most vulgar of the positivistic conceptions to consider philosophical and epistemological issues as deprived of interest, or of relevance, for the practicing of physics itself. Some praying mills are not as harmless as windmills, and it is not necessarily quixotic to tilt at them. Without any more preliminary justification, let me now try to sketch some directions for the recasting of quantum physics. For convenience, I will distinguish four types of problems, dealing respectively with the foundations, the description (terminology), the (so-called) interpretation and the (classical) approximations of quantum theory.

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[4] Let me seize this opportunity to stress the importance for the recasting of quantum physics of the two introductory textbooks by Feynman (8) and Wichmann (9). They are the first ones to break on some decisive points with an antiquated tradition and to contain some bold, although often implicit, new points of view. I owe to them much of my personal understanding of quantum physics — which came much later than my learning it.

I. On the Foundations of Quantum Theory: Down with the Correspondence Principle!

It is convenient to distinguish two different aspects in the foundations of quantum theories:

1) *the Universal Framework,* that is, the set of general assertions, postulates and corollaries, which hold true for any quantum theory, irrespective the particular physical situation to which it applies. As any theoretical structure of physics, it is not uniquely determined and obeys several formulations, with scopes of various extents. There exist old and narrow formalisms, such as the ones of the initial " wave mechanics " or " matrix mechanics ", as well as modern and very general ones, such as the C*-algebra formalism. In between, and at the present stage, the Hilbert space formalism perhaps is the one with the wider use. In that formalism, a state of a physical system is represented by a vector (or, more precisely, by a ray) in a Hilbert space, the inner product of two such vectors yields a probability amplitude, the physical properties are represented by self-adjoint operators, etc. It is the collection of these rules, common to all quantum theories (within this formalism), which I call here the Universal Framework. In short, it is the part of the quantum theory which may be thought of as relying on the PRINCIPLE OF SUPERPOSITION as its cornerstone, or more generally, as corresponding to the linear structure of the theory. The generalization of the initial wave mechanics closely associated with classical wave theories through heuristic analogies, to the more general Hilbert space formalism is typical of a recasting process in the foundations of quantum theory, one among the few to have taken place. This aspect of the foundations has been considerably renewed in the last period by the work done on the so-called " quantum logics ". These provide a new and deeper basis, although not completely stabilized yet, for the Universal Framework of quantum theory. Since these questions are dealt with at length in other contributions at this Colloquium, I will not insist any further, and will rather consider:

2) the *specific structure* of particular quantum theories, describing restricted classes of physical systems, such as, for instance, " nonrelativistic " (galilean) quantum mechanics of a particle (Schrödinger theory), many-body nonrelativistic theory, quantum electrodynamics, etc. For any such theory, the Universal Framework must be supplemented with specific assertions on the choice of the operators associated to the relevant physical properties,

their algebraic relationships, the dynamical law of evolution for the system, etc. Chronologically speaking, the initial approach to these specific structures has been through the PRINCIPLE OF CORRESPONDENCE with the " classical " theories. This is how nonrelativistic quantum mechanics was built upon classical Hamiltonian mechanics, quantum electrodynamics upon Maxwell electromagnetism, etc. This approach was justified, indeed it was almost a necessary one in historical terms; some criterion of consistency with the old theoretical framework in effect is one of the strongest conditions to be imposed to any new, emerging theory, and can be followed as a trustworthy guide. But, despite its role as an Ariadne's clew, this umbilical chord should be cut some day, for the correspondence principle meets with several difficulties, theoretical and (epistemo)logical. For instance, either it is considered as a heuristic guide, such as it was used with fecundity by Bohr, but the scope and validity of which cannot be systematically assessed, or it meets with logical contradictions when given a precise theoretical formulation (10). Much more serious is the fact that the correspondence principle gives us some knowledge of these quantum properties only which do possess a classical analog; specific quantum effects, vanishing in the classical limit, thus are outside of its scope. The quantized spin of " elementary " quantum objects here is a conspicuous example. Another one is the concept of parity[5]. Finally, since we know a classical theory to have only approximate validity, in a much narrower domain that the corresponding quantum one, there seems to be some logical inconsistency in using the first one as foundations for the second. It is truly paradoxical to assert, as do Landau and Lifshitz, that:

> "A more general theory can usually be formulated in a logically complete manner, independently of a less general theory which forms a limiting case of it. (...) It is in principle impossible, however, to formulate the basic concepts of quantum mechanics without using classical mechanics. The fact that an electron has no definite path means that it has also, in itself, no other dynamical characteristics (*sic.*). Etc. " (11).

This vicious circle (see fig. 1) is directly linked to our lack of knowledge of the conditions of validity for the " classical limit ", about which some comments may be found in the last section.

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[5] Observe that parity cannot have a classical limit as a conventional mechanical property; for a system such as the hydrogen atom, its value for consecutive levels is alternatively + 1 and — 1, so that no well-behaved limit exist for large quantum numbers, when the levels crowd together.

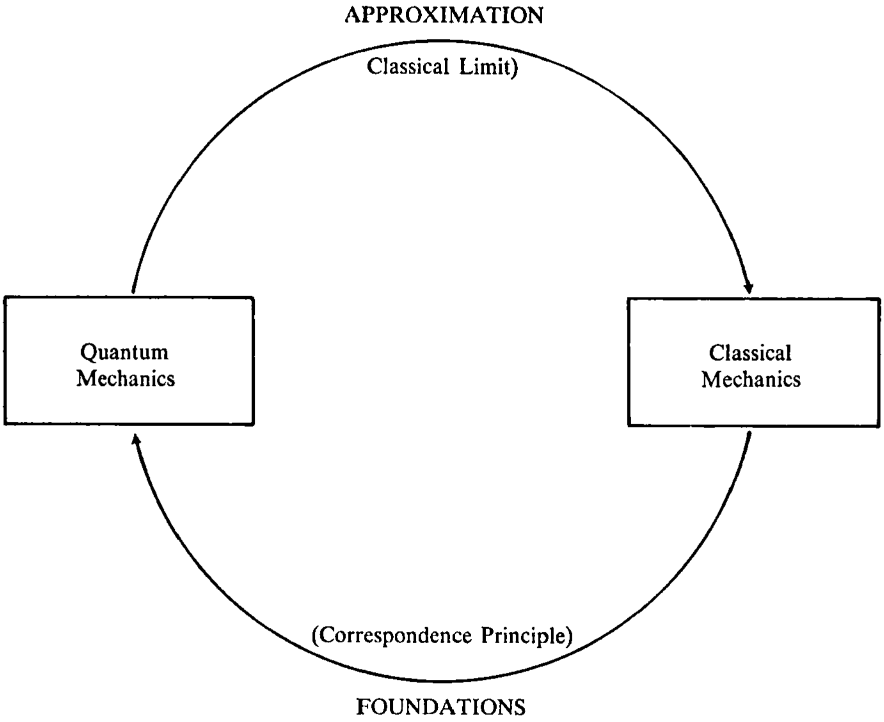

Fig. 1: The vicious circle

Fortunately, the correspondence principle may be replaced, and advantageously so, by the use of the INVARIANCE PRINCIPLES. The Universal Framework of quantum theories, through the linearity properties, indeed endows the invariance groups of physical symmetries with a great importance (12). It requires the existence of a unitary projective representation of the invariance group in the Hilbert space of any physical system with the relevant symmetry properties. A classification of these representations thus yield a classification of the possible quantum systems. Further, for Lie groups, through the SNAG theorem, any one-parameter subgroup is represented by a unitary subgroup derived through exponentiation from a self-adjoint operator, its " infinitesimal generator ". These operators usually qualify for describing the most important physical properties, such as energy, momentum, etc. Other properties may then be found in the enveloping algebra of the Lie algebra by simple considerations of invariance (or, rather, group " variance "). The position operator yields a simple and important example of this procedure (13). From such a point of view, the concept of spin, for instance, appears in a very natural way, from a simple analysis of the space-time properties of an " elementary " quantum object, whether it be in einsteinian (14) or galilean (15, 16) relativity. Besides deepening the foundations of the specific structure for quantum theories, the consideration of Invariance Principles enables one to clear-up some old misunderstandings, as well as to shape open problems. The emphasis they deserve, it is true, is not a new discovery and has already been advocated in detail in the literature (16, 17), so that I will pass on to more controversial issues.

II. On the Description of Quantum Theory:
In other words . . .

It is quite clear that in the actual practice of physics, no one can be content with the use of the sheer mathematical formalism, even though this formalism is a necessary and fundamental constituent of the considered theoretical domain (18). A metalanguage is necessary as well, so that the names given to the mathematical objects and formal concepts of the theory enable its statements to fit in the general discourse. The choice of the terminology thus is a very delicate affair, with deep epistemological implications. If adequate, it may greatly help the understandability of the crucial points as it may hinder it in the contrary case. Now, the difficulty is that such a choice necessarily relies on abuses of language or metaphors. Indeed one has to choose old names for new things: a completely invented name, created from scratch, would add nothing to the mathematical expression of a physical concept. One usually looks for a convenient name either in the ordinary language, or in the already specialized language of a previous theory. In the first case, we deal with a metaphorical analogy, for example when calling " spin " the intrinsic angular momentum of a quantum particle. In the second case, it is an abuse of language to extend the name of a physical concept belonging to a certain theory, to a more or less homologous concept in another theory, for example when calling " energy " the operator generating time-translations in quantum mechanics. Note that in that case case, due to the Einstein-Planck relationship $E = \hbar\omega$, one could have used as well the classical term " pulsation " for the same concept up to the numerically arbitrary constant $\hbar$). In fact, we deal here with a new, specifically quantum concept, which may be given a name borrowed from classical mechanics, under the condition however that this abuse of language be explicitly recognized. In these two examples (" spin " and " energy "), the choices of terminology may be considered as fortunate ones, giving in the first case a concrete picture (however approximate it is known to be), and in the second case a partially sound reference to a familiar limit theory with a large domain of validity. However, I contend here that, if for most of such specific concepts of quantum theory the common terminology can be considered as adequate, the situation is much worse for the general concepts, belonging, one might say, to the Universal Framework. There, the weight is heavily felt of the philosophical prejudices which permeated the initial theoretical work in quantum physics. The recasting of quantum theory should incorporate at least a critique of the conventional terminology: to hope for its modification probably is unrealistic in the present state of sociological inertia of our profession.

Here is a partial list of the usual terms in quantum theory, with some of my reasons for rejecting them and possible alternate proposals (despite my skepticism on their implementability).

" Observables " The word is a direct imprint on quantum physics of the positivism advocated by its founders. To call " observable " any self-adjoint operator associated to a physical property of a system is a multiple nonsense. To start with, as I have already pointed out, no one will ever actually observe or measure such a highly complex mathematical being . . . As a matter of fact, the only physical quantities we do measure directly (with a few exceptions) are lengths: displacements of needles on measuring apparatus, tracks on photographs, etc. Already in classical physics, very few physical properties are " observed "; think only of velocity for instance[6]. The very

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[6] The birth and life in physics of the concept of (instantaneous) velocity offers a simple and convincing example of the recasting process. It can be said that this concept is the crucial point of the galilean breakthrough which brought physics from a prescientific stage to the state of a true scientific theory. However, since Galileo could use but the euclidean geometrical theory of proportions as a mathematical tool, it is no surprise that he had to struggle for many years to master the concept (19). The development of mathematical analysis in the following century, and the rigorisation of limiting processes, would later on endow the concept with a much more convenient formal expression. But the ultimate stage in this recasting process was only reached in the present century, when the theoretical concept was materialized, so to speak, in common solid apparatus, such as the speedometer which is to be found on hundreds of millions of cars. Thanks to this *real*ization, any six-years old kid (well at least in that small fraction of the humanity where cars are a usual commodity) does know, in empirical terms, that a speed of 60 m. p. h. does *not* mean that the car will run for 60 miles in an hour. The instantaneous nature of the velocity is visibly conveyed by the motions of the needle under a quick acceleration or a brutal braking. A practical grasp of the concept thus builds on, at a collective level, easing the way for a later theoretical study. My contention is that a similar evolution is now taking place for quantum mechanical concepts, although, of course, on a socially much narrower scale.

indirect link between physical measurements, or observations, on the one hand and the essential ideas in the analysis and understanding of the situation, precisely is the reason for there being a *theory* with *concepts,* that cannot be immediately (i. e., without mediation) expressed in empirical terms. Of course, the terminology (" observables ") was introduced in an effort to overpass the limitations imposed by the formalism of classical mechanism. Since an analysis of the measurement process showed that one could not " observe " simultaneously, for instance, the momentum and the position of a particle, one could forget about this classical requirement and concentrate upon the real... observables of the system, rather than imposing *a priori* theoretical notions. But the error has been in the ensuing confusion between the experimental description and the theoretical statements. Finally, we know so little, as I will emphasize in the following section, about an actual quantum theory of measurement, that very few such " observations " of quantum properties can be theoretically analyzed. And, after all, we do care, experimentally speaking, for a handful only of such properties, those precisely which hold specific names (energy, momentum, spin, position, etc.). I would then rather use a general terminology such as *" physical properties "* in place of " observables ". " Dynamical variables " is acceptable also, though I find it a rather awkward expression, for instance when used to describe a . . . " kinematical constant ", such as energy for instance. Of course, it may be necessary to insist on the specific nature of these physical properties in a quantum theory, as opposed to classical ones. But, as I mentioned, " observables " does not qualify for stressing the difference. Simply call them " quantum properties ", or " q-properties ", distinguished from classical ones or c-properties. Consider finally this terminological monster: " commuting observables "; it associates a mathematical epithet with an empirical substantive — a true positivist chimera. Why not speak rather of " compatible q-properties ", or, on the formal side, of " commuting operators "?

" Observer " In most cases, this term simply is without any real theoretical function. It may be suppressed and, along with it, the whole sentence that contains it, without damage. In the few instances where it plays a role, it should be replaced, depending on the case, either by " experimenter " (in general metaphysical discussions), or by " measuring apparatus " (in epistemological or theoretical statements). These remarks will perhaps become clearer after the discussion below of the quantum theory of measurement.

" Uncertainties ". Here is a case of mistaken borrowing from the vocabulary of experimental physics. When it was realized that, in quantum physics, a physical property of a system in general cannot be characterized

by a sharp numerical value, the spreading of the possible values was assimilated to the experimental uncertainties on the classical physical properties. These c-properties indeed do have a sharp value in any physical state, but this value usually is only known up to some uncertainty defined by the experimental conditions (resolution of the apparatus, knowledge of auxiliary parameters). It should be realized to-day, however, that the essential formal difference between c-properties and q-properties is that the first ones " are " numerical functions (at least in particle mechanics), while the second ones " are " operators; that is to say, a q-property usually associates to a given physical state not a single numerical value but a whole spectrum. One does not deal with an empirical uncertainty, but with an intrinsic *" spread "*, which could (and should) also be called *" spectrum width "*, or *" dispersion "* or *" extension "* for instance. Of course, this is precisely what practicing physicists do: a Breit-Wigner curve is characterized by its width in any sensible laboratory talk; one only speaks of an energy uncertainty in classrooms. What is specific of quantum physics of course is that *all* physical properties may have such a spreading. But already in classical physics some properties may not always be sharp. In classical wave theory, we know that a wave in general has a whole spectrum of frequencies, and, except for harmonic waves, not a single value. Nobody would think of calling the width of the pulsation spectrum an " uncertainty " on the pulsation. How comes, then, that in quantum physics, the analogous energy width, $\Delta E = \hbar \Delta \omega$, becomes an " uncertainty "? It is clearly seen here how the failure comes from not taking the quantum theory seriously enough, by keeping stuck to classical ideas irrelevant in the quantum domain. It has been argued frequently, by the Copenhagen school in particular, that the difference between classical and quantum physics is that the first one only is consistent with everyday intuition and common sense, so that our mental pictures and the words we by pointing to the situation a few centuries ago, when aristotelian physics use should necessarily be based on this classical realm. I can only answer indeed was closer to common experience than the new, galilean one; after all, arrows do not have an indefinite uniform motion, up and down are not physically equivalent, the Sun is observed to go round the Earth, etc. What happened since, is that, due to this new physics, our " common " sense has been enriched and our " intuition " has evolved. (See for instance the note 6 above). I contend that the same is true today and that half a century of practice in quantum physics should allow us to drop our classical prejudices.

" Uncertainty principle ", " uncertainty relations ". Clearly there is no " principle " here; the " relations " between " uncertainties " are but ine-

qualities linking the dispersions in two non-compatible physical properties. These " Heisenberg inequalities ", as I propose to call them simply, are consequences of the true basic principles of quantum physics. As such they play a theoretically subordinate part. This is not to underestimate their importance. Quite on the contrary, I hold them for a major pedagogical and epistemological result (20). Not only do they exhibit the essential difference between q-properties and c-properties, but they are a very effective heuristic tool. Precisely because they express the limits of validity of the classical concepts, they enable one to use classical expressions for approximating quantum derivations by imposing additional constraints which simulate the full quantum treatment. The Heisenberg inequalities, far from expressing intrinsic and final limitations of our physical knowledge, as many philosophers have commented upon them, quite on the contrary greatly help us to enrich and refine our understanding. A terminology relying on the idea of " uncertainties " clearly cannot do full justice to this deep positive role, which is an additional reason to advocate a change. Finally, and to use once more a classical example for an argument *a fortiori*, has any one ever called the classical spectral inequality $\Delta k \cdot \Delta x \gtrsim 1$ an " uncertainty relation "? Why then, should the closely related quantum inequality $\Delta p \cdot \Delta x \gtrsim \hbar$ receives this dubious privilege? This might be the place also to get rid of the so-called " uncontrollable quantum perturbation of the observed system by the observer " (or the measuring apparatus), which are sometimes invoked as a source of the " uncertainties ". It may be asserted simply that no such perturbations exist. Quantum theory does not imply a necessary (and unknown) change in the state of the system subjected to a measurement. In fact, most analyses of the measurement processes (see the next section), including the conventional and orthodox ones, use a simple model, going back to Von Neuman, where the state of the measured system does not change.

" Complementarity ". We deal here with the typical example of a parasitical philosophical notion in physics. Not that it has been without utility: to physicists educated in classical mechanics, some general prescription was necessary to relieve them from the anxiety of not being able to apply any more classical ideas, such as the existence of simultaneously sharp numerical values for any two physical properties. When it became clear that quantum position and quantum momentum, for instance, decidedly did not fit into this scheme, they were interpreted as a pair of " complementary " properties, the observation of one with arbitrary precision precluding that of the other. It is not a matter of observation of course, but rather a question of the fundamental nature of the quantum concepts. Only by insisting on their

supposedly sharp numerical definition, do we need to introduce so vague a notion as the one of complementarity. Complementarity becomes a totally irrelevant idea for physics, as soon as one accepts the specifically quantum, i. e. qualitatively non-classical, nature of quantum theory. Also, the ever extended use to which complementarity was put to (by Bohr especially) should cast some doubt on the notion as a scientific one: from the complementarity between position and momentum, to that between particle and wave (see below), then to physics and biology, and even worse to society and individual, the ideological role of the idea becomes clear at last. Far from being an example of the philosophical impact of modern physics, a new way of thinking brought about by contemporary science, it is quite on the contrary a philosophical trojan horse inside physics and a witness of the real exploitation to which physics has been subjected by some philosophical currents (4, 6). The same type of critique will apply to my next target.

" Wave-particle duality ". Classical physics is built upon two key concepts, enabling it to describe all situations within its domain of validity: the concept of particle (discrete, localized) and the concept of wave (continuous, extended). But the objects the behaviour of which is described by quantum physics cannot be consistently analyzed in terms of these two concepts, although they share some of their characteristics. It was natural enough at the beginning of quantum physics to rely as far as possible on the known classical concepts, while using some criterion to avoid any situation where their contradictory properties would come into conflict. The point however is, as we should come to realize today, that the basic quantum objects are not either waves, or particles, but neither waves, nor particles. They must be described by some new concept, which, furthermore, turns out to be a unique one; several names have been proposed for such a concept, for instance " wavicle " or " quanton "[7]. In actual practice however, we still speak of " particles ", although we know well that they are not classical ones. Perhaps could we, at least in the beginnings of introductory courses, emphasize the point by writing " partiqles "? It still remains to be said that quantons have *something* to do with waves and particles. But it is not that they appear either as waves, or as particles: as a matter of fact, most of the times they just appear for what they are, deserving a full quantum treatment, and not lending themselves to a classical wave or particle description. It is true, yet, that in some specific circumstances, there are valid wave or particle approximations (necessarily exclusive). The conditions

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[7] As advocated by M. Bunge, whose careful discussion of several issues contemplated here is very close in spirit to mine (21).

for the validity of these approximations, although empirically more or less well-known, are not, to my knowledge, theoretically under control. It is an interesting problem, I think, to be solved, and one which should deepen our understanding of the quantum concepts as such (see also Section 4, below). Let me only note that bosons may obey either a wave, or a particle approximation; electromagnetic fields propagate as waves, and photons sometimes may be treated as classical particles. But fermions do not seem to have a classical wave description with any physical domain of approximate validity. And it is not clear whether the wave approximation for bosons does require or not a zero invariant mass (compare the cases of photons and pions). These last brief remarks are just intended to show how inadequate is the idea of a universal wave-particle duality, and how its very generality prevents one from dealing with concrete physical problems, namely studying the validity of *the classical wave and particle approximations.* To sum up, the " wave-particle duality " is no more correct a way to analyze a quanton, than a " rectangle-circle duality " would be to analyze a cylinder[8]; it is even worse, since the two partial aspects may hardly fit in the same picture with some consistency (fig. 2).

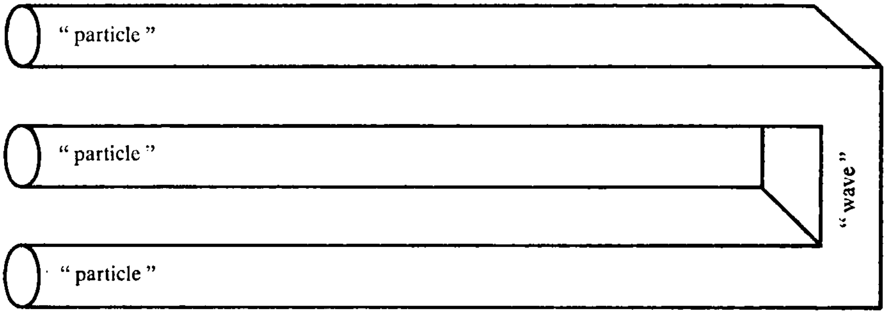

Fig. 2: The wave-particle " duality ". Although partial views of this figure may be interpreted as two-dimensional projections of three-dimensional bodies, the full figure is but a two-dimensional one, without such an interpretation.

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[8] I could also use another metaphor: the activities of several among our eminent colleagues might be analyzed by some people in terms of a " scientific-military duality " according to whether they give (or rather sell) talks at Colloquiums such as this one, or advices to the Pentagon in the Jason division for instance (22). On the contrary, I hold that these are but two aspects of a single and consistent sociopolitical situation, which, beyond the individual cases, is that of our whole professional community (6).

I will deal in the following section with the terminology of the so-called quantum theory of measurement: " reduction of the wave-packet ", " perturbation of the observed system by the observer (or measuring apparatus) ", " undeterminism ", etc., because of the need to discuss the whole question with some more details. I will not insist either on the very common incorrect use of some specific terms; for instance, " wave-function ", with the full weight of the classical metaphor it carries, should be exclusively restricted to denoting the state vector in the x-representation $< x \mid \varphi > = \varphi(x)$, and the same remark holds true for " wave equations ". In more general situations, one should speak simply of the " state vector ", or the " state ", obeying " dynamical " or " evolution " equations. Many such examples can be found; while they do not need a thorough nor controversial analysis, I see no reason why we should tolerate a systematic sloppiness in our language. But let me end this section by calling into question, without any will to change it however, the " quantum " label of quantum physics. It goes back, as we know well, to the discovery by Planck and Einstein of the discrete aspects of electromagnetism radiation, and was reinforced by the analysis of the quantized energy spectra of atoms, molecules, nuclei. The old saying " Natura saltus non fecit " seemed to be contradicted, and these discrete, quantal, aspects of the new physics came to be thought of as its characteristics. However, if it is true that in quantum physics, some continuous aspects of classical physics reveal their essential discontinuity, the converse is true as well. Instead of point-like particles, we deal with continuously extended quantons. Due to the tunnel effect, the transmission by a potential barreer which classically obeyed a simple yes-or-no law, is characterized by a coefficient with a value in the continuous range between 0 and 1. The quantum world is not " more discontinuous " than the classical one[9]. Instead, while the classical world could neatly enough be divided into a continuous part (waves) and a discrete one (particles), the quantum world is a single one, where this opposition is a rather irrelevant one[10].

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[9] A detailed study of the real pecularities shown by quantum physics with respect to classical physics, with a healthy criticism of many commonly accepted ideas, has been worked out by M. Bunge and A. Kalnay (23).

[10] Conversely, one might as well call into question the name of " classical physics " customarily given to pre-relativistic and pre-quantum physics. Have not " relativity " theory and " quantum " theory become " classical "as well, after more than half-a-century of active development? It is not true that most physicists today are much better educated in these sectors of so-called " modern " physics, than in several important fields of " classical " physics, such as hydrodynamics, for instance?

III. On the " Interpretation " of Quantum Theory: The late hatching of a Columbus' egg

Most philosophical exegeses, commentaries and discussions about quantum theory and its interpretation up to now have been centered on the so-called " measurement problem ". Stripped down to its essentials, the problem comes from the apparent contradiction between the two kinds of time-evolution followed by a quantum system:

1) when isolated, a quantum system is described by a state vector driven in the Hilbert state space by a linear unitary evolution operator: $\Psi(t) = U(t, t_0)\,\Psi(t_0)$. This operator U in turn is linked to the Hamiltonian which acts as the time-evolution generator. Such a behaviour, closely related to the validity of the superposition principle, is continuous and perfectly deterministic.

2) when subjected to a measurement, however, the state vector is said to " collapse " onto one of the eigenvectors of the operator associated to the physical property under measurement, and the measured value of the property is given by the corresponding eigenvalue. This so-called " reduction of the wave-packet " is not a linear process in the state space of the combined system consisting of the measured system and the measuring apparatus (or . . . " observer ") and cannot follow a deterministic unitary evolution of the preceding type. It is at this stage that the alleged " undeterminism " of quantum theory enters: the projection of the state-vector (as I will say instead of " reduction ") onto one of the eigenvectors obeys a stochastic process, with probabilities given by the squared modulus of the inner product between the initial state vector and the final eigenvector.

There is no need to stress that this probabilistic rule up to now has been supported by all the available evidence. It thus seem as if quantum theory was self-contradictory, since the behaviour it requires for isolated systems (type 1) could not apply to such a system when composed of a measured subsystem and a measuring one, for it has to obey a type 2 — evolution. Several solutions have been proposed out of this dilemma.

The conceptually simplest ones, apparently the most radical, but — to me, at least — in fact the most conservative, do not question the existence of the conflict, and explain it by a fundamental incompleteness of quantum theory. One may then look for a fundamental change, and investigate " deeper " theories. Such is the goal of the various " hidden variables " theories. Their strong classical flavour nevertheless makes it hard to believe that the difficulties of quantum physics might be solved in such a backwards way. However, there is no need anymore for lengthy philosophical discussions on this point; due to the work of Bell and others (24, 25), we now know that there are experimentally checkable differences between the predictions of quantum theory on the one hand, and hidden-variables theories on the other hand, unless these exhibit rather weird nonlocal features, which would plague them with conceptual problems even worse than the ones they are supposed to solve.

Other possibilities derive from accepting quantum theory, but supplementing it with various external devices which would explain, through some more or less natural physical mechanism, the projection of the state vector: one may invoke specific " physical " (!) laws obeyed by the mind of the living observer, as Wigner proposes (26), or, more soberly, macroscopic ergodicity (27), or still, gravitational fluctuations. But most of these attempts suffer from their rather " ad hoc " character. Indeed it is difficult, if the measurement process is considered as an interaction process between a (measured) system and an apparatus (or even an observer), to put such processes entirely apart from all other physical interaction processes, and to understand how they could obey specific laws without there being testable consequences outside of measurement theory as such.

The dominant conception, at least, is a fully consistent one. The " Copenhagen school " answer, or rather the way I understand it (for, as in any orthodox chuch, the fundamental dogmas may be interpreted in thousands of ways), consists in eluding the physical problem by giving it a philosophical solution. The state-vector receives a purely subjective interpretation, as a mere recording of the known informations on the system. Any new data, such as given by a measurement, then obviously change this catalogue. This change is not ruled by the laws of physics proper, it is a truly meta-physical process. This positivist standpoint is free from contradictions, especially since, as Von Neuman showed, the same results obtain whether the projection is supposed to take place during the direct measurement (of the system by the apparatus), or a following one (recording of the apparatus state by another apparatus), or the final observation (by a " conscious " experimenter). I only wish to stress that one cannot speak here of a " measurement theory ", since measurement is precisely put apart of the physical processes to which quantum theory applies. One should rather consider the projection rule as a supplementary postulate of the theory, of an empirical nature, and which can be shown to be consistent with the rest of the theoretical structure, provided we accept a particular philosophical interpretation. It has been repeatedly emphasized by the founding fathers of the theory that this interpretation implies rather drastic consequences for our world-view. For instance, because of the subjective interpretation of the state vector, no objective properties can be attributed to quantum systems as such. In other words, no quantum ontology is possible (see above the quotation by Landau and Lifshitz, p. 9). My previous proposals for changes in the terminology (" physical properties " instead of " observables ", for example) would then meet with a strong reluctance from the custodians of the orthodoxy; indeed, these changes need a consistent re-interpretation of the " measurement problem ", which I sketch below. Another inescapable feature of the Copenhagen interpretation is its dualism: there must exist two separate physical worlds, a quantum one and a classical one. All measuring apparatus and observers necessary follow the laws of classical physics, and the theoretical predictions as well as the experimental results must be formulated in classical terms, as Bohr specially pointed out. This is not a question of convenience due to the macroscopic nature of most experimental devices which would implie an approximately classical behaviour (see below a discussion of this " approximation "). Rather it is a question of principle; there is no fixed place for the classical/quantum borderline and its location may be arbitrarily moved provided it separates the measured object from the ultimate observing device. By reading the original works of the old masters, one cannot but admire the consistency and depth of their views. It must be said that much of their thoroughness was gradually lost by the following generations and that the customary statements of this epistemology, in most textbooks for instance, usually fail, by and large, to reach the original standards of rigour, clarity and coherence. I claim here that this philosophical decay does not have its only cause in the exceptional genius of the great masters, as compared to our present average level of understanding.

Instead, I would argue that we do not need any more to rely upon these philosophical principles in order to further our work in quantum physics. As I have already pointed out, the main problem of the first generation of quantum physicists was to get rid of the epistemological prejudices linked to classical physics, while at the same time relying on the approximately valid aspects of the very same classical physics as far as possible in the quantum domain[11]. The work of Bohr himself is a splendid illustration of

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[11] Let me quote here the apt words of d'Espagnat for characterizing Bohr's views: " along with many satisfactory aspects, such a view has the well-known but nevertheless surprising feature of expressing the laws of the microworld by using approximate classical concepts referring essentially to *our* experience of the macroworld " (25). I would only add that this experience of the macroworld was the only experience of the world available in Bohr's time, while we may rely today on a thorough experience of the microworld as well, which should entail the possibility of expressing it laws in a specific way.

this point; for most of its great physical contributions, he never used the fully developed and consistent quantum formalism, but he worked out with the utmost cleverness and an admirable insight semi-classical approximations, for instance, in building derivations based on the correspondence principle. The prescriptions of the Copenhagen School thus played a seminal role by ensuring the necessary philosophical security to the first explorers of the quantum domain, keeping them from falling back into classical preconceptions as well as from asking premature questions in quantum theory.

Things have changed today, however, and — this is my leitmotiv, indeed — we should try to draw the lesson of half a century of quantum practice. For if most physicists only pay lip service to the orthodox dogmas, it is that in fact they hold opposite beliefs, although implicitly only. Most of us, in our daily laboratory work, do act as if quantum systems in fact had objective existence and properties, as if quantum physics was universally valid and classical physics only a convenient approximation. For, raised in a quantum context (many of us know much more of quantum than of classical physics), we do not need to fight all day long against classical prejudices. Our physicists' common sense, so to speak, is no longer contradictory with quantum theory, the apparent "paradoxes" of which do not trouble us any more. In other words, besides the dominant explicit neo-positivist interpretation of quantum physics, there is a no less widely shared implicit realist point of view. The recasting which I advocate here should consist in expliciting, strengthening and developing this point of view — in other words, transforming the general silent indifference regarding the orthodox position into a voiced difference.

A decisive step into that direction was accomplished by Everett (28), more than fifteen years ago, with the efficient support of Wheeler (29), later followed by several authors (30). His solution to the vexed question of the state-vector projection in a measurement process is a very simple one indeed, namely that this projection does *not* occur... The projection postulate, he showed, is not needed to obtain the usual results of quantum theory. I will sketch the idea on the usual simple example. Let S be a quantum system with two basis states φ_+ and φ_- (spin up and down, to follow the tradition). Let A be a measuring apparatus with initial state Φ_0; upon interacting with S, A goes into a final state Φ_+ (resp. Φ_-), if S is in the state φ_+ (resp. φ_-). Φ_+ and Φ_- may be thought of as macroscopic pointer states, indicating at the end of the measurement the initial state of the system S. In other words, the evolution operator U of the combined interacting system S & A, is defined by:

$$U(\varphi_\pm \otimes \Phi_0) = \varphi_\pm \otimes \Phi_\pm$$

Consider now an arbitrary state of S, that is, a linear superposition, $\varphi = c_+ \varphi_+ + c_- \varphi_-$. The combined system $S \oplus A$, if in the initial state $\varphi \otimes \Phi_0$, will end in the final state:

$$U(\varphi \otimes \Phi_0) = c_+ \varphi_+ \otimes \Phi_+ + c_- \varphi_- \otimes \Phi_- \overset{\mathrm{df}}{=} \Psi$$

according to the linearity of U (" type 1 " evolution process). The projection postulate then asserts that the very act of measurement (or observation) somehow will cut off one of the two components of this state, and leave the combined system in one of the states $\varphi_+ \otimes \Phi_+$ or $\varphi_- \otimes \Phi_-$, with respective probabilities $|c_+|^2$ and $|c_-|^2$ (" type 2 " process). Now, Everett points out, independently of this projection postulate, if A is to be a good measuring apparatus for the " spin " of S, its pointer states Φ_+ and Φ_- certainly must be orthogonal, in order that the two possible states φ_+ and φ_- may be discriminated without ambiguity. If such is the case, it is well known, in the most orthodox tradition, that, *as far as the subsystem S is concerned,* the pure state Ψ is completely equivalent to the density matrix:

$$\varrho = |c_+|^2 p_+ + |c_-|^2 p_-$$

where p_+ are the projectors onto the states $\varphi_\pm$. It is now a purely subjective choice to interpret the state of S within the compound system $S \oplus A$ with state vector Ψ as being rather described either by φ_+ with a probability $|c_+|^2$ or by φ_- with a probability $|c_-|^2$. This interpretation, which is the one associated with the projection postulate, gives exactly the same theoretical results *for S* (for instance, the average values of any physical property) that the plain use of Ψ itself. Everett thus simply denies the need for the projection postulate, and gives a solution to the difficulties of the quantum theory of measurement which is really in the spirit of the Columbus' egg problem. One may now see the epistemological root of the projection postulate; it lies in the difficulty of fully accepting the superposition principle. Indeed, the intrinsic linearity of quantum theory, cannot be interpreted in classical terms. The quantum " plus " which relates two superposed states, cannot be thought of as a classical " or ". If one wants to fall back on this classical disjunction, then a supplementary assumption is necessary, extraneous to quantum theory as such. This precisely is the role played by the projection postulate, which allows a quasi-classical interpretation of the measurement process as yielding either such a result, *or* that one. Another way of saying this, is that, only by using the projection postulate, can we attribute a definite state to the system S after the measurement. The rejection of the postulate does not allow such a characterization, and we must deal with the non-separable state vector describing the compound system $S \oplus A$. It is precisely this specific quantum non-separability, upon which Everett rightly insists (28), which the projection postulate tries to bypass.

But how can all this be reconciled with our daily experience? After all we see either a down spot, or an up one in a Stern-Gerlach apparatus! The answer is simple — just treat an observer O as an apparatus; let χ_0 be its initial state and $\chi_\pm$ the final ones corresponding to the observation of the apparatus states $\Phi_\pm$. The combined system $S \oplus A \oplus O$ will now go from the initial state $(c_+\varphi_+ + c_-\varphi_-) \otimes \Phi_0 \otimes \chi_0$ to the final one $c_+\varphi_+ \otimes \Phi_+ \otimes \chi_+ + c_-\varphi_- \otimes \Phi_- \otimes \chi_-$. As long as χ_+ and χ_- are orthogonal, as they should be if the observation is to be a reliable one, no interferences can take place, that is, no mixing of the " consciousness states " χ_+ and χ_-, which remain disjoint, each one being correlated to the correct system states φ_+ and φ_-. The very linearity of the evolution process entails the consistency of this scheme as Everett shows by giving examples of multiple measurements on a given system, as well as chains of measurements, each successive apparatus measuring the state of the preceding one.

Of course Everett's suggestion has far-reaching consequences, which he carries consistently and which are exactly opposite to those of the Copenhagen interpretation. There is now but a unique world, a purely quantum one. It is described by a universal state-vector (the title of Everett's big paper (28) is " The Theory of the Universal Wave-Function "), which evolves according a single unitary deterministic process (" type 1 "). A measurement, in this scheme, is but a specific type of interaction, the effect of which is to produce " correlated " final states, namely, states of the form $\sum_k c_k\varphi_k \otimes \Phi_k$, where $\{\varphi_k\}$ and $\{\Phi_k\}$ are basis of the two subsystems, instead of the most general state $\sum_k \sum_l \gamma_{kl}\,\varphi_k \otimes \Phi_l$. Everett's analysis also sheds new lights on the probabilistic interpretation of the $|c_k|^2$, although it does not go as far as to have " the formalism dictate its own interpretation ", as somewhat too enthusiastic supporters would make us believe (30) . . . I will not comment on that point however, and would rather insist on what I believe to be a more serious misunderstanding of Everett's thesis by many of his followers. Once more, under a question of terminology lies a deep conceptual problem. The above interpretation in effect has been called by several people, especially De Witt, one of his main propagandists, the " many-worlds (or many-universes) interpretation of quantum theory " (30). The rejection of the postulate projection leaves us with the " universal " state vector. Since, with each successive measurement, this state-vector " splits " into a superposition of several " branches ", it is said to describe " many universes ", one for each of these branches. Where the Copenhagen interpretation would arbitrarily choose " one world " by cutting off all

" branches " of the state-vector except one (presumably the one we think we sit upon), one should accept the simultaneous existence of the " many worlds " corresponding to all possible outcomes of the measurement. Now, my criticism here is exactly symmetrical of the one I directed again the orthodox position: the " many worlds " idea again is a left-over of classical conceptions. The coexisting branches here, as the unique surviving one in the Copenhagen point of view, can only be related to " worlds " described by classical physics. The difference is that, instead of interpreting the quantum " plus " as a classical " or ", De Witt and al. interpret it as a classical " and ". To me, the deep meaning of Everett's ideas is not the coexistence of many worlds, but on the contrary, the existence of a single quantum one. The main drawback of the " many-worlds " terminology is that it leads one to ask the question of " what branch we are on ", since it certainly looks as if our consciousness definitely belonged to only one world at a time. But this question only makes sense from a classical point of view, once more. It becomes entirely irrelevant as soon as one commits oneself to a consistent quantum view, exactly as the question of the existence of the ether was deprived of meaning, rather than answered, by a consistent interpretation of relativity theory. In the words of Everett:

> " Arguments that the world picture presented by this theory is contradicted by experience, because we are unaware of any branching process, are like the criticism of the copernican theory that the mobility of the earth as a real physical fact is incompatible with the common sense interpretation of nature because we feel no such motion. In both cases, the argument fails when it is shown that the theory itself predicts that our experience will be what in fact it was " (28).

Of course, the very same analysis which shows that the projection postulate is unnecessary as a fundamental part of quantum theory, also shows that it is a convenient recipe in practical work. It allows one to deal with states characterizing the considered system alone, instead of the global state of the system - and - apparatus, not to say of the whole universe. Rather than a separate postulate, we should view it as a theorem, and a most useful one, the importance of which I do not intend to minimize. In other terms, a consistent treatment of quantum theory does not require the projection postulate, but everything works " as if " it did hold[12]. Perhaps *not* everything, after all,

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[12] To pursue the analogy used in Everett's quotation above, where his interpretation of quantum theory is compared to the copernican system, I would compare the common Copenhagen interpretation to the clever system devised by Tycho-Brahé,

since one of the reasons why people like Wheeler and De Witt support the Everett interpretation, is their belief that it may allow a conceptual welding of quantum theory with general relativity which seems difficult within the conventional treatment; this is another problem, upon which I do not want to comment here.

Yet this cannot be the end of the story. Indeed, Everett's reinterpretation breaks an important " epistemological obstacle " (according to Bachelard's expression) on the way to a better quantum theoretical understanding. But it opens for us now the task of building concrete analyses of quantum measurements. As we have seen, the Copenhagen interpretation cannot but consider this question as a metaphysical one. On the contrary, from the new point of view, as I have already stressed, a measurement is a specific type of interaction process between two physical systems, which is to be fully described within quantum theory. It is to be proved, in such cases, that the system called " apparatus " in its interaction with the " measured " system, indeed possesses the characteristics necessary to its performance as a measurement device.

It is not sufficient, in that respect, that particular macroscopic pointer positions be described by orthogonal states of the apparatus, as the too sketchy analysis above might lead one to conclude. In fact, all non-diagonal matrix elements should vanish for every operator describing a reasonable physical property of the apparatus which could serve as a pointer for the considered measurement. In particular, one would like to understand, from that point of view, the role of the macroscopic nature of the measuring apparatus, which — contrarily to the Copenhagen orthodoxy — we do not consider as obeying classical mechanics. The challenge has been successfully met by Hepp who, for the first time, gave specific models of measurement processes, completely analyzed in quantum theoretical terms (31). He showed, in his most realistic example, the so-called Coleman-Hepp model[13], that the necessary orthogonality of the pointer states of the measuring apparatus for an adequate class of its macroscopic physical properties, results from a superselection rule in the relevant state space, obtained in the double limit where i) the apparatus becomes infinitely extended (with an infinite number

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who, by the way, was a Dane as well. His system was a compromise between the ptolemaic system (here to be likened to classical physics), and the copernican one; it had the earth fixed at the centre of the universe, with the sun circling around it and all other planets then circling around the sun. It is clear that this system is consistent with the more general copernican one as it only supplements it with a choice of a particular privileged reference frame. The choice is unnecessary but convenient from the observer's viewpoint. The same exactly may be said for the projection postulate with respect to general quantum theory.

[13] Bell has given an elementary version of the model (33).

of particles), ii) the interaction time (that is, the duration of the measurement) becomes infinite as well[14]. Of course, neither of these conditions ever is met rigorously in practice. However, Hepp could estimate the corrections due to the finite size and time of the measurement process; they are quite negligible. Hepp's analyses may and should be refined and extended to more realistic situations — as well as to simpler ones, perhaps, for educational purposes. But we know now that theoretical analyses of measurement processes can be developed in fully quantum terms — rather than a general theory of measurement, for which there is no place as a separated entity, if a measurement is but a specific type of physical interaction[15].

IV. Approximations of Quantum Theory: Back to Classical Physics.

My main theme in the preceding sections has been the assertion that, to the present day, much of quantum physics foundations, terminology and interpretation, unduly relies on classical physics. I have also tried to explain the reasons for such a situation. As long as classical physics is used as a starting point towards quantum physics, their relationship hardly can be analyzed but in abstract philosophical terms, as in the Copenhagen view. On the contrary, as soon as quantum physics may stand on its own, its connection with classical physics may be subjected to theoretical analyses, rather than to meta-theoretical ones. And, indeed, it is a vast domain to investigate, in which deep and important physical problems too long have been obscured by epistemological prejudices. Surprising as it may seem, we do not have today a

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[14] Let me stress the need for the second condition (infinite duration of the measurement), perhaps more unexpected than the first one. Although models are possible where it is not required, they seem to be much too crude and physically irrelevant. Rather, a moment of reflexion will convince oneself that this condition indeed closely corresponds, as the first one, to the usual experimental situations. Also its importance comes from its contradicting the very general assumptions under which d'Espagnat has derived " anti-quantum " Bell's type inequalities (25).

[15] It has been argued by Bell(33), against Hepp's point of view, that the limits (in size and time) necessary for the validity of the analysis are purely formal ones and that no " rigorous " projection of the state vector actually occurs. Bell exhibited a physical property of the measuring apparatus in the Coleman-Hepp's model for which the non-diagonal matrix elements do not vanish in the above limit. It is, Bell admits, a complicated object, with a strange time dependence, but its very existence, he maintains, prevents one from speaking about a " wave-packet reduction ". Of course, I do agree with him, since I hold that there is *no* such " reduction "! But what Bell holds for a drawback of Hepp's analysis to me is one of its assets, since it shows that not every macroscopic property of a given apparatus, but only a specific, though large, class of such properties may be used as efficient measuring pointers for a given property of the measured system. Indeed there are no universal measuring apparatus and experimenters usually stick to rather stable (rather than weirdly time-dependent) properties of their devices as reliable pointers!

serious inderstanding of the classical approximation to quantum mechanics. There are, of course, formal derivations of the mathematical structure of classical mechanics from the one of quantum mechanics. They generally consist in studying mathematical limit processes in which Planck's constant vanishes. It is apparent that such processes are purely formal and, at most, give us a proof of the theoretical possibility that classical mechanics be a valid approximation to quantum mechanics [16]. But they tell us nothing about its physical conditions of validity. Besides the philosophical veto for such investigations as expressed by the conventional view of quantum physics, there may be another cause to this gap. It is commonly, although perhaps implicitly, thought that the problem is a simple one and that the classical/quantum dichotomy merely corresponds to the macroscopic/microscopic one. Once more, this idea relfects a past historical situation. For a long time, indeed, all known macroscopic systems could be analyzed by classical physics, while quantum physics was restricted to atoms and molecules, nuclei and fundamental particles. We know today, however, as a result of our long experimental and theoretical work in quantum physics, how to observe macroscopic quantum effects, in well-specified conditions. Physical systems such as lasers, superconductors and superfluids, indeed exhibit clear quantum effects on a macroscopic scale [17]. Thus the large size (or, rather, number of particles) cannot be a sufficient condition for the validity of classical concepts. Neither is it a sufficient one, since classical (or semi-classical) approximations, today find an extended use, in fundamental particle physics for instance.

In fact, besides these spectacular but rather particular quantum effects, quantum physics plays an all-pervasive role in our everyday macroscopic world. For, not only is classical physics unable to ensure the stability of an isolated atom, a difficulty which was one of the sources of quantum physics, but it cannot explain the stability of their grouping into ordinary bodies, such

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[16] Hepp has given one of the most interesting analyses of this type, by studying the relationship between the limits $\hbar \to 0$ and $N \to \infty$ for quantum mechanical correlation functions (34). He himself has emphasized that "the classical limit is not unique" and that, even in the simple cases he considers, one may obtain the classical mechanics of N point particles as well as a classical field theory, depending on formal assumptions (see also the discussion of the so-called "wave-particle duality" in Section 2 above).

[17] A nice example is given by the quantization of the vorticity in rotating superfluid helium. The circulation of the velocity vector around the vortex strings is quantized in units of value h/m, where m is the mass of the helium atom. Numerically, $h/m \simeq 10^{-3}\ \mathrm{cm}^2\,\mathrm{s}^{-1}$, which means that this quantum effect takes place on a scale of a tenth of a millimeter in space and a tenth of a second in time. Indeed they may be observed almost with the naked eye ... (35).

as crystals for instance. In that respect, the relationship between quantum and classical (non-quantum) theory, is a much more complicated one than the relationship between " relativistic " (einsteinian) and classical (galilean) physics. The galilean theory of relativity has a wide scope of approximate validity, even extending far enough to include surprisingly many electromagnetic phenomena (36). Above all, it provides a consistent (although erroneous) view of the world, to be contradicted only by rather elaborate experimental tests. On the contrary, classical mechanics loses its inner consistency as soon as it hits upon the atomic hypothesis; the extended bodies of our common experience, in classical terms, can only be thought of as perfectly homogeneous and continuous lumps of matter. Since the atomic hypothesis itself is grounded in well-known and very elementary chemistry and thermodynamics, it is seen that classical mechanics is contradictory with other parts of the classical picture of the universe. After all, its incapacity to provide an explanation of the black-body radiation, as a macroscopic failure, probably was a much more serious cause of concern than the puzzles associated to atomic spectra or the photoelectric effect.

It may be surprising, then, that the stability of ordinary matter has stood for so long before being proven on the basis of quantum theory. Only in the recent years has the problem been solved through the efforts of Dyson and Lenard (37), and Lieb and Lebovitz (38). The first of these authors were able to show that in a system consisting of massive charged particles interacting via Coulomb forces, the binding energy per particle is bounded independently of the number of particles (saturation of forces), under the condition that at least the particles with one sign of their electric charges belong to a finite number of species of fermions. In other words, it is the Pauli principle ruling the electrons which ensures the stability of the world. As a counter-example, Dyson also has shown that boson systems interacting via Coulomb forces are not saturated (39). The specific quantum nature of the Pauli principle thus is a proof of the need for a quantum explanation of the most fundamental aspects of the physical world, namely its consisting of separate pieces of matter with roughly constant density. Pursuing this work, Lieb and Lebovitz were able to prove rigorously the existence of thermodynamic limits for the physical properties of interest in such Coulomb interacting bodies (38). I hold these results for some of the most important ones in theoretical physics during the past years: they can be said to provide a real and deep explanation of very general physical phenomena, right from first principles. It is ironical enough that they do not use any recent knowledge, neither of empirical data, nor of mathematical techniques, and " could " have been established a long time ago, were it not for epistemological obstacles. True, the analysis of Dyson and Lenard is a monument of subtlety, proceeding through a very long chain of clever inequalities. It is highly desirable that a new, shorter proof be given to provide an easier access to the result and to bring down the estimate on the bound of the energy per particle to a more plausible value; due to the cumulative multiplying of the successive estimates, it is actually some 10^{14} times higher than the empirical value [18]. To still emphasize the highly non-trivial nature of these analyses, let it be said that the Coulomb potential precisely is a critical one: for potentials decreasing faster than r^{-1} at infinity and slower at the origin, saturation may be proved much more easily (41). Or still, for purely attractive forces, such as those responsible for gravitation, saturation is trivially shown not to hold (42) [19], so that it is the delicate balance of attractions and repulsions in Coulomb systems which endow them with their very special properties. One may see here how, as I asserted earlier, the " classical " behaviour does not result in a simple and universal way from some formal approximation to quantum theory, but requires, on the contrary, a thorough analysis of the specific physical situation.

One could also quote here other studies of the behaviour exhibited by various specific models of quantum systems in the macroscopic limit. Simple models of the collective and cooperative interactions of radiation and matter (such as based on the " Dicke Hamiltonian "), have led to a better understanding of quantum optics and laser physics (44) [20]; they provide an active and fruitful field of investigation.

The above-mentioned works deal with the possibly classical behaviour of macroscopic bodies for various specific physical situations. A more general approach to " the connection between macrophysics and microphysics " has been proposed by Fröhlich (47). Starting right from the microscopic Schrödinger equation obeyed by the density matrix of an N-body quantum systems, he studies how various approximations, depending on the concrete situation, may lead to macroscopic physical laws. In his own words:

> " The method to use is to formulate the relevant macroconcepts, say hydrodynamic velocity field, mass density, etc. in terms of microproperties and then to employ the exact microequations of motion (without attempting to solve them) for the derivation of dynamical laws

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[18] On the very day when I was finishing the present paper, such a proof appeared in print (40).

[19] Non trivial results come out of a direct quantum study of macroscopic bodies consisting of gravitationally bound particles (43).

[20] Rigorous studies of the macroscopic thermodynamics (equilibrium and non-equilibrium) of such models have been achieved with exciting (!) results, such as the appearance of phase transitions (superradiance?) (45). Unfortunately, it appears that these features were due to unphysical drastic simplifications in the original model (46).

> between the macroconcepts, e.g. the equations of hydrodynamics. Such derivations of macroscopic equations nearly always require imposition of certain assumptions which specify the particular situation ".

The macroscopic physical quantities in fact are related to reduced density matrices, the linked equations of motion of which obey an ascending hierarchy, to be suitably cut off, depending on the approximation used. His methods enable Fröhlich to derive classical laws when valid, such as the Navier-Stokes equation for fluids, as well as macroscopic-quantum approximations, such as are necessary to understand superconductivity or superfluidity. Further applications appear to be possible, leading to new results, rather than to the recovery of old ones, for instance in biological systems (48). Indeed, in the apt words of Fröhlich:

> " It might be thought that all interesting macroscopic properties had been found long ago and that the derivation of their dynamical laws would be a matter of time, but not of very great interest. In contrast, however, it will be noted that the concept of macroscopic wave functions which dominates the properties of superfluids and superconductors had been discovered in recent had been discovered in recent years only. (. . .) It is quite obvious that a very large number of undiscovered macroconcepts does exist in situations which are removed from thermal equilibrium. For otherwise one should be able to derive by systematic methods the properties of all machines made of metal, say, since one has been able to formulate the basic laws referring to the atoms of metals " (47).

Since the starting point is a set of exact microscopic equations of motion, not containing any statistical assumptions, an appropriate treatment should finally permit the introduction of thermodynamic quantities and yield all relations that hold between them. Such an ambitious program, implying a new justification for statistical mechanics in general, has yet to be carried out; Fröhlich still has shown that the expectation was fulfilled for very weak interactions. Finally, it is fitting to conclude this too brief description of a major work by quoting its last paragraph, in which Fröhlich, apparently unaware of Everett's work, comes close to the position advocated in the present paper concerning the interpretation of quantum mechanics (see the preceeding Section).

> ". . . This article should not be closed without emphasizing the exclusive status of the density matrix Ω of " the whole world ". In

standard use of quantum mechanics, the interpretation of a state vector, or of the corresponding density matrix rests on the introduction of an observer who interferes with it. If Ω refers to the whole world, then no such observer can exist. Hence Ω develops causally with time, containing all possible quantum-mechanical possibilities[21]. (. . .) It would be fascinating to speculate on the consequences of an Ω attributed to " the whole world " " (47).

Conclusion

Rather than to close this paper by a trite summarizing of the precedent considerations, I prefer to leave it open-ended, by trusting further developments onto the readers. As a neat example, the practical importance of which cannot be denied, an urging task in recasting quantum theory could consist in the rewriting of Zipkin's theoretical zipperdynamics (49). Since this fundamental work has too long been ignored, I think it useful to have it partly reprinted below. It will be seen how the author bravely deals with a problem in macroscopic quantum physics[22], thus defying the orthodox tradition, while keeping attached to this very same tradition in his use of a worn-out terminology. No doubt many further progresses in such a crucial area might be achieved through a consistent recasting, such as the understanding of the (epistemological) obstacles which too often block zippers at mid-course.

[21] Let us note here that Fröhlich, in stressing, as he does, the quantum nature of the various " possibilities ", does not fall into the classical trap of the " many-universes " terminology.

[22] One might also mention here the penetrating quantum-theoretical analysis of ordinary ghost phenomena by Wright, as a further proof of the importance of quantum effects in everyday life (50).

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Theoretical zipperdynamics

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Introduction

The fundamental principles of zipper operation were never well understood before the discovery of the quantum theory [1]. Now that the role of quantum effects in zippers has been convincingly demonstrated [2], it can be concluded that the present state of our knowledge of zipper opera-