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Problems and promises of the ensemble interpretation of quantum mechanics*

Abstract. After a brief outline of the ensemble interpretation, its advantages and promises are described, including both the elimination of the puzzles that beset the Copenhagen interpretation and newer lines of research such as the application of ergodic theory to quantum mechanics. Three problems facing the ensemble interpretation are discussed, of which that of joint probability distributions is seen to require further research; some possible lines are indicated. Certain philosophical problems are discussed, including that of the relation between formalism and interpretation, where it is suggested that the formalism neither implies nor is indifferent to the interpretation. The hidden-variable question is then considered; the von Neumann theorem is seen to be a special case of a very general theorem, and is interpreted to mean that only stochastic hidden-variable theories are acceptable. The outline of a possible such theory is given.

I

The puzzles and paradoxes of quantum mechanics are, as is well known, rather closely associated with the Copenhagen interpretation; this designation will here be taken generically to cover the wide variety of views which has in common the conception that the wave function describes a single system. The list of these difficulties is long and has not yet ceased to grow —witness the recent discovery of a Zeno-type paradox in quantum mechanics (Misra and Sudarshan 1977). They may usefully be classified according to the particular aspect of the Copenhagen view (or views!) to which they relate. Most of them will be found to fall into one of three groups:

a) Those associated with the basic question concerning the nature of the wave function, *e.g.* the problems of the wave-particle duality. This variety has given rise to a great deal of heated discussions but is usually not susceptible to precise statement in mathematical language; this fact does not diminish their importance but does complicate their analysis.

b) Those derivable in one way or another from the work of Einstein, Podolsky and Rosen (1935); these include the well-known puzzle of Schrödinger's cat and its ramification at the hands of Wigner's friend. Here the mathematical formulation was made clear from the very beginning; unhappily it cannot be said that the large number of papers that over the years have attempted to analyse these problems have shed any significant light on them.

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c) Those linked to certain specific points in the Copenhagen interpretation which may be doubted without affecting the main part of that structure. Of this nature are the questions raised by the projection postulate.

The third group is not very relevant to the present purpose, for obvious reasons, and I shall not further discuss them. My main aim here is rather to show, in the first place, that the ensemble interpretation of quantum mechanics successfully deals with the difficulties in groups *a* and *b*; then to ventilate certain philosophical questions concerning the relationship between the mathematical framework and the interpretation of any physical theory; thirdly, in the light of these considerations, to examine certain problems in the ensemble interpretation and the attempts made to solve them; and finally, to consider the outlook for the future work. But before these points are raised I will briefly outline the nature of the ensemble interpretation.

What I call here the ensemble interpretation is essentially what is commonly known as the statistical interpretation; but I mark the difference for two reasons. One is that the term "statistical" may be misleading, since it has sometimes been used to refer to Born's conception of the wave function as a probability amplitude (Heisenberg 1930, Messiah 1959). The other reason is that the statistical interpretation is usually left rather unfinished, while I propose to study the problems involved in rounding it out and so making it the basic interpretation of all quantum mechanics.

The ensemble interpretation was adumbrated by Slater (1929) and further developed by quite a number of authors, including Einstein (1949,1953), Margenau (1958, 1963 a,b) and Blokhintsev (1953). A recent review (Ballantine 1970) provides a good account of the arguments leading to this interpretation, so that here I need only touch on the central points (see also Ross-Bonney 1975).

In the ensemble interpretation the wave function is taken to refer not to one simple system but rather to an ensemble of such systems, in the sense of statistical mechanics: a (possibly infinite) set of theoretical replicas of the system under study; this set is described by a measure function $\mu(d\mathbf{x})$ which yields the relative abundance within the set of systems that lie in the region $d\mathbf{x}$ of the underlying sample space, usually taken to be the phase space of classical mechanics. In such an ensemble the theoretical prediction for a function $f(\mathbf{x})$ is the expectation value,

$$\langle f \rangle = \int_{\Omega} f(\mathbf{x})\mu(d\mathbf{x}) \tag{1}$$

where Ω is the volume of the phase space,[1] and f may, of course, also depend on the time t.

One point must be stressed: the ensemble is a *theoretical* construct, and as always its use does not necessarily imply that only averages over many measurements of the same kind can be compared to its predictions (and even less that it can only deal will systems composed of many particles); I return below to this question.

What the ensemble interpretation maintains is, in other words, that the ensem-

[1]Eq. (1) assumes that the measure μ is normalized. A very clear discussion of ensembles will be found in Balaescu (1975).

ble average (1) coincides with the quantum-mechanical expectation value, provided a suitable ensemble is chosen. But as will be seen below, it is precisely this choice of an ensemble that the quantum formulation does not determine uniquely: it is here that we rediscover the incompleteness of quantum mechanics first pointed out by Einstein, Podolsky and Rosen (1935). As will be seen in section VI, this incompleteness points beyond quantum mechanics and opens up a fruitful field of research. Within the quantum framework the nonexistence of well-defined criteria for choosing an appropriate ensemble creates no problem: the quantum formalism has precisely the aim of allowing us to calculate expectation values without specifying explicitly the ensemble used for this.

But this does not mean that all is plain sailing: there are still certain problems raised by the ensemble interpretation, among which the issue of joint probability distributions occupies pride of pace; accordingly, it will be reviewed in section IV. This, however, requires the clearing up of some philosophical matters, and these, together with some related points, will also by discussed below, together with some possibilities for resolving the joint-probability problem.

II

Now what does the ensemble interpretation achieve?

In the first place, conceptual clarity. As has been stressed, for instance by Penrose (1970), the notion of an ensemble is fundamental not only for statistical mechanics but is a basic concept for the cognitive process we call scientific research. A particular aspect of this situation is the role it can play in adequately explicating the much confused idea of probability (Brody 1980); this explains its relevance to quantum mechanics, where it has been clear for some time that the two chief interpretations are closely linked to the two most prevalent views concerning the nature of probability (Popper 1967; see also Ballentine and Brody *et al.* 1979).

The conceptual clarity I refer to is particularly evident in the elucidation the ensemble interpretation offers of Heisenberg's so-called uncertainty principle: if $\hat{a}$ is the (Hermitian) operator corresponding to an observable a (*i.e.* a quantity for which a measuring process is known) such that the values observed for a state ψ yield a mean that, within experimental error limits, corresponds to $\langle\psi|\hat{a}|\psi\rangle$, then

$$(\Delta a)^2 = \langle\psi|(\hat{a} - \langle\psi|\hat{a}|\psi\rangle)^2|\psi\rangle \tag{2}$$

is the theoretically predicted statistical dispersion of the measured values of a. Similarly for the operator $\hat{b}$. And if a and b do not commute,

$$[\hat{a}, \hat{b}] = i\hbar\hat{1} \tag{3}$$

for instance, then

$$\Delta a \Delta b \geq \tfrac{1}{2}\hbar. \tag{4}$$

Here $\Delta a, \Delta b$ can only be interpreted as standard deviations in the usual statistical sense, and the fact that for (4) to hold they must both be defined with the same ψ only means that we must have a state-preparation procedure which can generate an indefinitely long sequence of systems belonging to the ensemble described by ψ; on some of these systems we measure a, on others we measure b. It is not necessary that a and b should be measured on the same systems. Thus Δa and Δb bear no relation to the experimental errors δa and δb; fortunately so, for otherwise it might prove difficult to provide experimental proof for the validity of (4), as is very clearly explained by Ballentine (1970).

Similar conceptual simplifications arise in the consideration of the measurement problem. This has become a problem only because, in a view that associates the wave function with an individual system, the prediction that different measured values occur with nonzero probabilities is incompatible with the fact that only one of these values is obtained in each measurement while the others do not occur at all. What privileges these values? We do not know; hence the need for von Neumann's projection postulate and all its undesirable consequences. The difficulties are often further compounded by the quite unwarranted assumption that the measurement process does not alter the value of the measured quantity (of course, if ψ is an eigenstate of $\hat{A}$ with eigenvalue α, then $\hat{A}\psi$ is an eigenstate with the same eigenvalue; but no principle underlying quantum mechanics allows us to identify the mathematical effect of $\hat{A}$ with the physical interaction between the systems described by ψ and external systems) and by the neglect of the distinction between state preparation and state measurement. Any physical experiment begins by suitably preparing the system under study; the system is then submitted to whatever interaction is the object of the experiment, and finally suitable measurements are carried out. Therefore a state preparation is a process that leaves the system in a known state; while state measurement determines, at least partially, what state the system is in as it enters the measurement. The system's state before state preparation and after measurement are of no interest, and in the second case may be meaningless when the measurement is destructive. There is thus a symmetry under time reflection between preparation and measurement; but the symmetry is not complete, for not every measurement procedure can be turned into a preparation method. The distinction between state preparation and measurement, established clearly by Margenau (1958, 1963 a, b), is vital. Among other points, it permits defining the proper significance contained in the projection postulate, namely that after state preparation by means of a procedure describable through an operator $\hat{A}$, the system will be in an eigenstate $|\alpha_i\rangle$ belonging to the eigenvalue α_i of $\hat{A}$, if the subensemble i is selected. The last phrase is essential, for without a selection a mixed state containing all eigenstates of $\hat{A}$ is obtained; in a Stern-Gerlach apparatus, for instance, we obtain a beam of particles with all spin projections directed upwards only if we select that part of the split beam which goes through the upper slit. On the other hand, if we replace the slit by a counter, the particles are absorbed: we have turned a preparation procedure into one of measurement, but the projection postulate is now meaningless. It is the confusion between preparation and measurement which generates the many absurdities apparently derived from the projection postulate.

Once these unnecessary confusions are eliminated the discussion of measurements in the ensemble interpretation is very simple (once more follow Ballentine 1970): the expectation value is found from the basic rule

$$\langle \hat{A} \rangle = \langle \psi | \hat{A} | \psi \rangle \tag{5}$$

and corresponds, to within experimental error, to the mean of a sufficiently large set of experimental determinations of A on similarly prepared systems (*i.e.* all belonging to the ensemble described by ψ). Suppose $|S;\alpha_i\rangle$ to be the eigenstate of $\hat{A}$ for the system S to be measured, and $|M;\mu_j\rangle$ to be those states of the measuring apparatus M which are macroscopically distinguishable, $|M;\mu_0\rangle$ being the initial state of M. Then the initial state of the system $S+M$ will be $|S;\alpha_i\rangle|M;\mu_0\rangle$; if we now write V for the evolution operator of the interaction between S and M, the final state will be

$$V|S;\alpha_i\rangle|M;\mu_0\rangle = |S;\phi_i\rangle|M;\mu_i\rangle \tag{6}$$

say. The state ϕ_i of S may but need not coincide with α_i and in general is a functional of it (Araki & Yanese 1960). If instead of an eigenstate of the quantity to be measured we have a state

$$|S;\psi\rangle = \sum_k \langle \alpha_k|\psi\rangle |S;\alpha_k\rangle$$

then

$$V|S;\psi\rangle|M;\mu_0\rangle = \sum_k \langle \alpha_k|\psi\rangle |S;\alpha_k\rangle|M;\mu_k\rangle, \tag{7}$$

and the probability of the macroscopic observation μ_i is $p_i = |\langle \alpha_i|\psi\rangle|^2$, meaning that if we repeat the state preparation yielding ψ and the measurement of A a sufficient number of times, the relative frequency of our finding μ_i is just p_i. If, then, the states $|M;\mu_i>$ are distinguishable at the macroscopic level, the system S must have had the value α_i for A with just that probability p_i. It is this which makes M into an appropriate measuring system. No peculiar phenomena such as the "collapse of the wave function" is involved. Schrödinger's cat presents no problem: there is now a multiplicity of them, half of them being dead and the other half alive; which one of them we look at is not, however, implicit in the wave function.

The only remaining question concerns the simultaneous measurement of more than one quantity. The problem appears, as expected, when the operators for these quantities do not commute. It is suprising that the textbooks make at best confusing reference to this problem, for simultaneous measurement is one of the most widely employed experimental techniques; indeed, in bubble chambers the momentum of a particle is derived from a set of position measurements which yield the curvature in the magnetic field.[2] Surprisingly little work has been done on this question since the pioneering work of Park and Margenau (1968). They showed that the non-commutativity of operators does not imply the incompatibility of the corresponding measurements; they offer a not implausible conjecture that there exists a type of joint measurement (which they term historical because its results depend on earlier states of the system), that is the only one always feasible, and they furnish examples of this type (the curvature measurement on bubble-chamber tracks belongs to it, as do time-of-flight experiments). This conjecture has not to my knowledge been fully validated yet.

It is worth adding, since there exists much confusion about the matter, that there is no connection between non-commutativity and correlation. Thus position and momentum of individual particles are *in some experimental situations* so strongly correlated that we can deduce one from the other; while two perpendicular spin components are quite uncorrelated.

A quite different direction in which the ensemble interpretation has shown notable promise is in the study of the ergodic properties of quantum states. The ensemble represented by the wave function ψ has, in general, a time dependence, though for a stationary state of energy E this takes the relatively trivial form $\exp(-iEt/\hbar)$; it makes sense, therefore, to ask under what circumstances ensemble averages may replace the temporal average for a single system —which is the most fundamental of the properties that ergodic theory (see *e.g.* Arnold and Avez 1968) has shown to be relevant. It is suprising that such questions should never have been asked until quite recently; the first ones to do so were Claverie & Diner (1973, 1975), who defined the correlation function of an operator $\hat{A}(t)$ (in the Heinsenberg picture) as

$$B_A(t,t') = \langle\psi|\tfrac{1}{2}[\hat{A}(t)\hat{A}(t') + \hat{A}(t')\hat{A}(t)]|\psi\rangle \tag{8}$$

and then showed that, firstly, for a stationary quantum state it depends only on the difference $\tau = t - t'$ and so is stationary in the stochastic-process sense also; and secondly that if the quantum state is non-degenerate, then the variance of the quantity $a(t)$ corresponding to $\hat{A}(t)$ in such a way that its ensemble mean is the quantum-mechanical expectation value $\langle\psi|\hat{A}|\psi\rangle$ tends to zero as t^{-2}: thus we have a strong ergodic property. Numerically, it is found that ergodicity is reached in a surprisingly short time; for the average for any operator is within one millionth of its ensemble average when t exceeds 10^{-10} sec. On the other hand, an unconfined system is never ergodic.

These results are highly relevant to the interpretation of quantum mechanics. Among other points, they go a long way to clearing up the source of many confusions;

[2]On more than one occasion, a bright student has interrupted my discussion of bubble-chamber techniques and stated that this was impossible, because the laws of quantum mechanics forbid the simultaneous determination of non-commuting quantities. This is explicitly stated to be the case even by reputable authors, *e.g.* Roman (1965), section 1-2: "the necessary and sufficient condition of the simultaneous measurability of two or more observables on any system is that the corresponding operators commute".

for instance, it is evident that measurements on individual hydrogen atoms (and similar confined non-degenerate systems) are well represented by ensemble averages for they usually take a much longer time than that needed to achieve ergodicity. This is the reason why the single-system interpretation of the Copenhagen type could develop and achieve a persuasive series of successes; it is also the reason why they create so many paradoxes for situations like the famous double-slit experiment, where ergodicity fails and therefore the predictions of the quantum-mechanical ensemble correspond only to averages over many measurements: the passage of a single electron, whichever slit it goes through, never creates a diffraction pattern.

Much further work remains to be done on these and related questions; but it is already evident that such investigations will shed light on some very obscure corners of quantum mechanics, as for instance the unsatisfactory description we have of processes like the separation of H_2^+ into $H^+ + H$; this problem is well known to quantum chemists (see *e.g.* Claverie and Diner 1976), but ignored by physicists.

III

The ensemble interpretation thus offers the advantages of conceptual simplicity and clarity, of freedom from paradox (so far as we know), of a quite natural fit to the experimental situation, and of great possibilities for further research. Why then has it not simply displaced the Copenhagen interpretation? There exists a philosophical bias which I return to below; but there are also difficulties more directly linked to the physics of quantum phenomena, and these may be examined under three headings.

The first one concerns the existence of discrete states. If we accept the ensemble interpretation, quantum mechanics has a basic structure rather like that of statistical mechanics, and one might therefore expect that distribution functions which are Dirac δ's (or sums of them) would appear only as limiting cases; in quantum mechanics, however, such distribution functions appear to be fundamental. The difficulty vanishes once it is noticed that even in quantum situations discrete states (in the mathematical sense) can only be an extrapolation: for such states appear only in systems that are essentially confined to a finite volume (described by a square-integrable wave function) and have no interaction whatsoever with anything outside it, and this is in fact an idealization which real systems at best approximate. From a slightly different point of view, an energy eigenstate, for instance, must have a certain finite width, otherwise its lifetime is infinite, it can never decay and therefore cannot be observed; we should not know about its existence and if the theory predicted it should judge the theory wrong. Here, then, the ensemble interpretation suggests that the basic elements of the quantum formalism be extended so as to consider primarily open systems in interaction with their surroundings, and closed systems only as limiting cases. This point will be touched on again below.

The second difficulty to be looked at here arises from the conclusion many physicists have arrived at that joint distribution functions for non-commutating observables cannot exist in the quantum formalism. The argument may be set out in the following points, which we number for their discussion below; they are largely adapted from Cohen (1966a).

(i) The ensemble of quantum mechanics are characterized by probability distributions over a classical phase space, augmented where necessary by variables that account for spin etc. (In what follows we have no need of this generality and therefore restrict ourselves to a phase space (p, q) with one degree of freedom, where q is the position and p the momentum of a particle.)

(ii) The probability distribution[3] $f(p, q)$ must satisfy the following conditions:

$$f(p,q) \geq 0 \text{ almost every where} \tag{9}$$

$$\int\int f(p,q)dpdq = 1 \tag{10}$$

$$\int f(p,q)dp = |\psi(q)|^2 \tag{11}$$

and

$$\int f(p,q)dq = |\phi(p)|^2 \tag{12}$$

where $\psi(q) = \psi(q,t)$ is the coordinate wave function for the state under consideration and $\phi(p) = (2\pi\hbar)^{-1/2}\int\psi(q)\exp(ipq/\hbar)dq$ is the corresponding momentum wave function. Conditions (9) and (10) are needed so that $f(p, q)$ is a proper density function for a probability distribution, while condition (11) stipulates that it should have the correct marginal distributions for the probabilities of finding values for the position or the momentum. Condition (10) is redundant, being implied by the normalization of ψ or ϕ, but is noted for completeness' sake. All integrals go from $-\infty$ to ∞.

(iii) For each quantum-mechanical operator $\hat{A}$ there should exist a function $a(p, q)$ such that the expectation value

$$\langle\hat{A}\rangle = \langle\psi|\hat{A}|\psi\rangle = \int\int a(p,q)f(p,q)dpdq \tag{13}$$

(iv) Furthermore, if another operator $\hat{B}$ is a function $F(\hat{A})$, then the corresponding function $b(p, q)$ which enters into (13) should satisfy

$$b = f(a). \tag{14}$$

It can now be shown that

[3]For the sake of simplicity, I have written probability densities $f(p, q)$ to discuss distributions here, though the differentiability of the distribution $F(p', q') = Pr(p \leq p', q \leq q')$ is not a requirement

(v) There exists no *unique* rule for deriving a distribution $f(p,q)$ for a given wave function ψ that will satisfy all the conditions of points (ii) and (iii); and moreover,

(vi) There exists no function $f(p,q)$ that satisfies point (iv) for any F as well as (ii) and (iii).

Conclusion (v) was first brought out in a paper by Shewell (1973); both are proved (though differently stated) in Cohen (1966b, 1966c).

These conclusions are all the more surprising in that joint distribution functions have found wide use in various applications, and are particularly useful in quantum optics (Agarwal and Wolf 1970). Indeed, their history goes back to 1932, when Wigner (1932) introduced a distribution

$$f_{\mathrm{W}}(p,q) = \frac{1}{2\pi\hbar}\int \psi^*(q - \tfrac{1}{2}\tau\hbar)e^{i\tau p}\psi(q + \tfrac{1}{2}\tau\hbar)\,dq \tag{15}$$

as a phase-space representation of ψ. This is still the best known of the distribution functions, and it has formed the basis of a serious attempt to restate quantum mechanics as a phase-space theory (Moyal 1949). Yet the Wigner function of Eq. (15) is not a probability function for all of quantum mechanics; it fails in three aspects: it satisfies condition (iia) above at best for the ground state, but is non-positive already for the first excited state of the harmonic oscillator; for functions $a(p,q)$ which cannot be written as $a'(p) + a''(q)$ it can give the wrong expectation value; and –a special but important case– it predicts finite widths for the excited states of systems. Concerning the first point, it has been shown (Urbanik 1967, Hudson 1973, Piquet 1974) that the Wigner function is non-negative for the coherent states first introduced by Glauber (1963); but however useful these have in practice proved to be, it is not possible to reduce quantum mechanics to them, and therefore several authors, among others Mehta (1964) and Margenau and Hill (1961), have proposed alternatives to Eq. (15). That none of these can be satisfactory is the result of Cohen's work, who was able to introduce a general form of which earlier proposals are particular cases:

Suppose, in classifical statistics, we are given a probability density $f(\mathbf{x})$ for a set of variables $\mathbf{x} = (x_1, \ldots, x_n)$ and wish to determine the density for a set of variables $\mathbf{y} = (y_1, \ldots, y_n)$ which are functions $\mathbf{y}(\mathbf{x})$ of the $\mathbf{x}$. A convenient way to carry out the transformation is to find the characteristic function for the $\mathbf{y}$, *i.e.* the Fourier transform of f in the $\mathbf{y}$ space

$$\chi(\boldsymbol{\theta}) = \int f(\mathbf{x})\exp(i\boldsymbol{\theta}\cdot\mathbf{y}(\mathbf{x}))d\mathbf{x} \tag{16}$$

and then to recover the looked-for density $f(\mathbf{y})$ as the inverse Fourier transform

$$f'(\mathbf{y}) = \frac{1}{(2\pi)^n}\int \chi(\boldsymbol{\theta})\exp(-i\boldsymbol{\theta}\cdot\mathbf{y})d\boldsymbol{\theta}. \tag{17}$$

In quantum mechanics $\mathbf{x} = (p,q)$ and the $\mathbf{y}$ we are interested in are the operators $\hat{p}$

and $\hat{q}$. This prescription therefore implies that (writing θ, τ for the two components of $\boldsymbol{\theta}$)

$$\begin{aligned}\chi(\theta,\tau) &= \int \psi^*(q) \exp i(\theta\hat{q} + \tau\hat{p})\psi(q)dq \\ &= \int \psi^*(q) \exp(\tfrac{1}{2}i\theta\tau\hbar) \exp(i\theta\hat{q}) \exp(i\tau\hat{p})\psi(q)dq \\ &= \int \psi^*(q - \tfrac{1}{2}\tau\hbar)e^{i\theta q}\psi(q + \tfrac{1}{2}\tau\hbar)dq. \end{aligned} \tag{18}$$

The Fourier inverse of this is the Wigner function (15); but the notion that we must replace p, q by $\hat{p}, \hat{q}$ in the exponential is, as Cohen (1966b,c) shows, insufficient, and a more general correspondence

$$\exp i(\theta q + \tau p) \longleftrightarrow g(\theta,\tau) \exp i(\theta\hat{q} + \tau\hat{p}) \tag{19}$$

satisfies the requirements (ii) and (iii) provided

$$g(\theta, 0) = g(0, \tau) = 1, \tag{20}$$

and

$$g^*(\theta,\tau) = g(-\theta - \tau). \tag{21}$$

Eq. (20) is needed to ensure the quantum-mechanically prescribed marginal distributions (11) and (12), while Eq. (21) ensures that all operators $\hat{A}(p,q)$ are Hermitean and so have real eigenvalues. Wigner's function now corresponds to the case $g = 1$.

It is not difficult now to show that there exists no one function $g(\theta, \tau)$ such that the conditions (ii) and (iii) are satisfied for all ψ —which proves conclusion (v)— and that there cannot exist any $g(\theta, \tau)$ for which condition (iv) is satisfied —which proves conclusion (vi). These two conclusions not only create foundational difficulties for the extensive applications of distribution functions (and the Wigner function in particular); they are also awkward for the ensemble interpretation. Since their mathematical background is irreproachable, we must critically examine the points (i) to (iv) on which they are based. But this requires the elucidation of certain philosophical points, a matter to which we now turn.

This will at the same time create a basis for discussing the third difficulty of the ensemble interpretation, namely that in it Einstein's theorem (Einstein, Podolsky and Rosen 1935; Einstein 1949) must be taken seriously and the consequence must be faced that the quantum-mechanical description of nature is incomplete.

IV

The origins of quantum mechanics are closely linked with strongly antimaterialist philosophical positions. This is not the place for exploring them, a task which has already been well carried out by others (*e.g.* Jammer 1966, 1974); what is relevant here is that this created an evident bias against the ensemble interpretation because of its open association with the view that the physical world has a reality which is independent of and both logically and chronologically anterior to our ideas about it. Such conceptions have been repeatedly expressed; the first to state them explicitly was, perhaps, Jordan (1936). As a consequence the precise arguments of, for instance, Einstein (1949, 1954) have been either ignored or misunderstood, and the ensemble interpretation has remained underdeveloped, its problems stressed rather that studied, while the peculiarities and paradoxes of the Copenhagen interpretation have been taken, with a naive pride, as signs of its revolutionary character. It is time, I feel, for us to abandon such attitudes and to behave as physicists rather than as blind defenders of our respective Weltanschauungen. If, therefore, I proceed from a materialist point of view (in the sense indicated above), this is to be taken as a postulate whose justification is to be found in its successes, no more —and no less.

This position has significant consequences for the concept of probability used in the ensemble interpretation. It can evidently not correspond to any of the so-called subjective views, whether Keynes or de Finetti, for these can be consistent only with a rejection of objective reality as the starting point for the philosophy (of science as of anything else). Unfortunately the frequentest viewpoint also creates difficulties, due largely to its positivist origins which lead it to ignore the subtle but fundamental distinction between theory and experiment; nevertheless this view (often called objective) should have led the first generations of quantum physicists to something like the ensemble interpretation.

Yet this did not happen. Those founders of quantum mechanics that thought along Copenhagen lines tended towards one or another subjective view of probability; Heisenberg's interpretation of it as some sort of Aristotelian potentia is well known (Heisenberg 1955). Those that followed the ensemble interpretation on the whole accepted von Mises' formulations (von Mises 1931), and then ran into trouble. For some quantum mechanics became a kind of theory for many particles when they interpreted the frequentest conception of probability too strictly as an experimental prescription; it is to avoid such misunderstandings that I have preferred here to speak of the ensemble inpretation instead of the statistical one, as it is traditionally known. For most the smoothing-over of the theory-experiment distinction in the positivist tradition underlying von Mises' work made the agreement of theoretical prediction and experimental result almost automatic, and therefore "automatically" eliminated the whole region of problems that would have led to an ergodic theory of quantum mechanics;[4] we saw above that the first fruits of such a theory already provided useful insights.

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[4] This is particularly striking in the work of J. v. Neumann, who made significant contributions to the development both of quantum mechanics and of ergodic theory, who carefully formulated the postulates of quantum mechanics in terms of statistical ensembles, and who yet did not realize how neatly one part of his work would apply to the other (v. Neumann 1932).

It was necessary, therefore, to develop a conception of probability that could fit better into an interpretation of quantum mechanics based on the idea of an ensemble. In fact, the ensemble concept turned out to provide precisely the required structure. This idea is discussed elsewhere (Brody 1975, 1979). Here we need only say that a full justification can be found for the use of ensembles on the basis that a physical theory must cover a certain range of similar situations and must therefore be an approximation for each particular situation; that averaging over the ensemble provides a way for selecting the common features among all the situations covered by the ensemble; and the probability is then introduced as the ensemble average of one particular kind of property. It will be clear that since the ensemble is a theoretical construct, the agreement of its theoretical predictions with experimental results is not automatic; it must be striven for, by adjusting and improving the ensemble until the fit is adequate. Lastly, if the ensemble is to describe physical situations, it will have a time evolution; thus ergodic concepts can appear naturally in this picture, and they turn out to be very relevant to understanding the role of probability in our descriptions of nature.

On such a basis the ensemble interpretation of quantum mechanics is quite natural, and the conceptual difficulties and misunderstandings I have mentioned above do not develop. But because things did not in fact happen in this way, certain further points require discussion before going on to consider possible solutions to the difficulties of the ensemble interpretation.

The first one concerns the relationship between a theory's structure and its interpretation, where two opposing viewpoints can be found. One is that these two elements of a physical theory have a unique connection, so that the formalism implies the interpretation; this widely held view is clearly stated by Rosenfeld (1957). Such a view raises several problems; even if it were true, it would not be helpful until we actually knew how to deduce the interpretation from the formalism; and since, above all for quantum mechanics, no one has been able to carry out such a programme of deducing the interpretation, we cannot do better than to continue comparing the relative merits of different interpretations. But the view is not even true; this is obvious when we consider that the interpretation of a formalism, *i.e.* the connection we propose to establish between the various concepts of the formalism and elements of physical reality,[5] involves notions that do not at all appear in the formalism. Moreover, historically the first vague ideas of what later becomes the interpretation procedes the construction of the formalism, and it is precisely because the connection between the two is not unique that the business of scientific research requires that element of creativeness all the great scientists have insisted upon. Also, it must not be forgotten that scientific theories are not static; they change and evolve; sometimes it is the interpretation that changes, as when the ideal gas laws are reinterpreted in the light of the microscopic models of statistical mechanics; sometimes the formalism is renewed, as when classical mechanics is rewritten by Hamilton and Jacobi. In

[5]The language here is deliberately borrowed from Einstein (Einstein, Podolsky and Rosen 1935); for the problem is a real one only in his materialistic philosophy. In the phenomenalism of Mach or the conventionalism of Poincaré it reduces to a methodological question of little fundamental interest, and for a subjective idealist it vanishes completely.

quantum mechanics, in particular, the formalism has not remained at all static, and we have a Hilbert-space version, a C*-algebra one, and lattice-theory one —not forgetting the two original formulations of Schrödinger and Heisenberg-Born-Jordan. Do we then have to find a slightly different interpretation for each of these versions?

But if the Rosenfeld view of a unique connection between formalism and interpretation is not tenable, neither is the view apparently held by many critics of the Copenhagen interpretation (I say 'apparently', for its absurdity would be patent if it were fully spelt out, and so it can at best be glimpsed as implicit in their writings) that these two components are largely independent, so that one can remove the interpretation from a theory and simply plug in another one. This view might be termed a 'Meccano' one; household appliances and motorcars can be built on such a principle, but scientific theories lie beyond its pale. We need not labour the point.

Clearly the actual situation lies in between these extremes. Formalism and interpretation do not imply each other, and are not mutually deducible; but neither are they independent, to be changed at the whim of the scientist. They have a strong influence on each other. This fact has an obvious implication here: if the ensemble interpretation of quantum mechanics is to be made fully workable, we may expect that some change in the formalism will be required. Just what changes are in fact needed is still an open question, requiring much further work; in the next sections some relevant ideas are discussed. It might be said, of course, that with such changes in the formalism we no longer have quantum mechanics, we have a new theory. This is to some extent a terminological question to be settled by convention, though only a radical change in basic notions, methods and results could really justify speaking of an entirely new theory; the changes that can at present be foreseen, however, are hardly more important than for instance the introduction of superselection rules. But a more significant answer is that the general acceptance of what we have called the ensemble interpretation would surely mean the equally general relinquishment of the Copenhagen interpretation: this is not what would be expected if it were to constitute a new theory, since new theories (*pace* Kuhn & Co.) do not replace earlier ones except in special, limiting situations.

It is perhaps more important to observe that if we abandon Rosenfeld's view then we must look for a criterion of choice between the alternatives for an interpretation. One should expect, on general grounds, to derive experimental tests; after all a difference in interpretation signifies a difference in the links between formalism and observable fact. Unhappily, in the present case nothing of the kind seems possible. Two factors combine to bring this about: on the one hand, in all interpretations of the probability concept —and we have seen how central this is to the formulation of the two main interpretations of quantum mechanics— the experimental estimate of a probability is derived from a relative frequency; and on the other, it is quite usual to find experimental physicists who in writing adhere to the Copenhagen interpretation but whose experimental practice corresponds to the ensemble one. There is thus no way experimentally to decide between the two interpretations.[6]

[6]It was hoped at one time that Bell's inequality (Bell 1965; see also Clauser and Shimony 1978) might indirectly provide relevant evidence, in that its experimental confirmation would eliminate all but macroscopically non-local hidden-variable theories. As we shall see below, the concept of

Since conceptual simplicity is at best a confused and subjective criterion (Bunge 1963), there remains only one possibility: fruitfulness in suggesting further lines of research. In the last section we shall show that on this criterion the ensemble interpretation wins hands down.

Another point concerns the "return to outmoded classical models" which has been a common reproach directed at critics of the Copenhagen interpretation. Now it is true that the ensemble interpretation makes no such demands of ontological renewal as the Copenhagen viewpoint does —no doubts about the underlying reality, no holistic implication uniting object and measuring device, no renunciations of causality (which, moreover, is generally confused with determinism). But it is not at all true that it reduces quantum theory to a special if elaborate case of classical mechanics, for two good reasons: as the comparison with statistical mechanics makes clear, in a statistical (or better, ensemble) theory, new concepts and qualities appear (*e.g.* temperature or entropy), others disappear (the positions and momenta of the microscopic components) and even new basic principles (irreversibility and ergodicity) can arise which not only have no counterpart in the underlying mechanics but can even contradict it; in the present case, moreover, the ensemble interpretation is not a complete statistical theory, for it lacks the required mechanical theory that it would be based upon.

Precisely this is the last point to need making here: in the ensemble interpretation Einstein's theorem on the incompleteness of quantum mechanics acquires the specific meaning just mentioned; quantum mechanics, then, requires an underlying physical model of which it will be the statistical theory. In a sense we shall have here a hidden-variable theory and we must therefore explore the question to what extent such theories are conceivable, and how far it has been possible to construct them. Here we anticipate the discussions below to underline that the ensemble interpretation provides both the motivation for research in this direction which has proved to be very promising, and the link between the resulting theoretical constructions and quantum mechanics. The Copenhagen interpretation, on the other hand, leads to the conclusion that this line of work is impossible; it has even been used to turn quantum mechanics into the definitive fundamental theory of physics, no longer susceptible of further modification (Born Heisenberg 1928); we need not refer to the many historical precedents of similar predictions that further research has falsified.

V

On the basis of these general considerations it is possible now to examine the problem of the joint probability distributions.

The first observation to be made here concerns point (i) in section III. An ensemble theory needs appropriate probability distributions over a suitable sample space; but that this sample space should be the phase space of classical mechanics —augmented or not— is a matter that all writers on the subject have simply taken

hidden variables is, though not implied, at least suggested by the ensemble interpretation. But recent work (Brody and de la Peña, 1980; Brody 1980) has dashed this hope.

for granted. It is at first sight plausible, particularly in view of an early paper by von Neumann (1931); but it springs from the notion that quantum mechanics should in some sense be derivable from classical mechanics. If this is not so, if classical mechanics appears as a limiting case of quantum mechanics but not also as its basis, then there is no special reason to accept the classical phase space as the sample space relevant to the ensemble interpretation. No work appears to have been done on this question, so that no more can be said here beyond pointing out an open problem.

With this reservation we may accept classical phase space as the basis for the ensemble interpretation, and go on to consider point (iv). The meaning of the stipulation that Eq. (14) should hold becomes clear if we take the simplest case, $b = a^2$ corresponding to $\hat{B} = \hat{A}^2$. The square of an operator will correspond experimentally either to the repetition of a measurement on the same system, or alternatively to a different device that measures the observable A^2. In the second case there seems no conceivable reason to suppose that Eq. (14) should always hold; only the first implies this, under the condition that we accept the projection postulate (without which the two measurements might yield different eigenvalues of $\hat{A}$). But we have already noted that the projection postulate is untenable in the ensemble interpretation; it might be added that it does not correspond to the majority of experimental situations. Thus (iv) is not in general valid in quantum mechanics, and conclusion (vi) need no longer be taken as standing in the way of a consistent ensemble interpretation.

Conclusion (v) presents us with a different situation. It is not in itself a very plausible requirement that there should be a unique rule for deriving the quantum mechanical distribution function, in the sense that Eq. (19) admits only one function $g(\theta,\tau)$; on the one hand, such a requirement has no analogue in classical statistical theories and would therefore need a specific justification which it has never received; and on the other hand, as is well known (see *e.g.* Cohen 1966a-c), each $g(\theta,\tau)$ generates a particular correspondence rule between classical quantities and quantum operators, while no single such rule can have general validity (Shewell 1959). This last point is obvious enough, for the existence of such a privileged correspondence rule would tie quantum mechanics to the apron strings of classical mechanics in a very unacceptable way.

Unfortunately, this does not dispose of the matter. As is already clear from the example of the harmonic oscillator, a joint distribution function would have to be state dependent, as well as problem dependent.

That it should depend on the particular problem what $g(\theta,\tau)$ is appropriate seems eminently reasonable; that g should also be a functional of the quantum state is clearly less so. We must conclude that the conditions given in section III must be reformulated, but it is not yet completely clear in what terms; though condition (iv) can be eliminated, something must be added that will allow an appropriate freedom of choice for $g(\theta\tau)$ while at the same time a physically plausible picture is created.

Research on these questions has been going on now for some time, with useful and interesting results, but no definitive solution as yet. Curiously enough, though various groups have proceeded on the basis of quite dissimilar notions, their conclusions converge. Apart from those authors (*e.g.* Shankara 1967, Leaf 1978) who attempt to remain within the framework of traditional quantum theory, two lines of thought appear.

One is due to Prugovečki (1976, 1979; Ali and Prugovečki 1977; Boisseau and Barrabes 1978) and is based on the concept of a "fuzzy" sample space, made up of "fuzzy" sample points defined as a point in phase space together with a certain "confidence function" giving the certitude of how near an experimental observation actually is to the point. The unsharpness of such points raises a number of problems, among others certain conceptual difficulties with the confidence function. For the present purpose, however, the main awkwardness of the "fuzzy" approach is that fuzziness is taken as a primitive concept, not further explained; thus the approach sidesteps the problems rather than elucidating them, and though on this basis it is possible to define probability functions that are positive semidefinite and fit into the quantum-theoretical picture, they are not ordinary probability distributions in the sense of Kolmogorov, say, and their use would require more fundamental study than they have yet received. We shall not further discuss this approach here.

A second approach is represented by the very different formulations of Bopp (1956), Ruggeri (1971) and Kuryshkin (1972a,b, 1973). Bopp's paper broke new ground and in fact anticipated the later work of Shewell (1959) and Cohen (1966) in many ways. His proposal was –in the terminology used here– to introduce a particular function $g(\theta, \tau; \ell)$ that depends on a parameter which possesses many of the characteristics of a fundamental length. Now $g(0, \tau; \ell) = 1$ only in the limit $\ell \to \infty$; hence we do not here satisfy condition (iic) except approximately; condition (iv) is likewise not satisfied. Bopp's work has been rather unjustly neglected and incorrectly described as wrong (Kuryshkin 1973); it is so only in the sense of breaking with the Copehnagen interpretation. The choice that Bopp made for g was a little too specialized, and the papers by Ruggeri and Kuryshkin attempt to remedy this; their methods are at first sight quite unrelated but work in course by my collaborator J.L. Jiménez establishes the fundamental identity of these three approaches and their connection with related work.

Further possibilities exist that have not yet been worked out. One promising idea is to take explicitly into account the comment made above that eigenstates of zero width are idealizations. This can be done by eliminating pure states from "real" quantum mechanics and only admitting density matrices such that $\operatorname{tr} p^2 \leq \operatorname{tr} p = 1$. Whether this gives rise to a feasible theory is at present under study.

There is, however, one problem with such approaches. We noted that Prugovečki's fuzziness concept is not really satisfactory, essentially because it makes it impossible to reach the underlying physics; the Bopp-Ruggeri-Kuryshkin method does not do this, but as yet it lacks any background model that would make its various assumptions sufficiently plausible and remove their rather indefinite generality. After all, the ensemble interpretation is not looking for a new formalism; it looks for better physics and it necessarily adapts the formalism.

Both these approaches involve functions $g(\theta, \tau)$ that do not satisfy Eq. (20); thus the marginal distributions of q and p will not quite equal $|\psi(q)|^2$ and $|\phi(p)|^2$ respectively; this discrepancy with the prediction of usual quantum theory requires discussion. Quantitatively, the difference can be explained as the effect of experimental uncertainty, as has been shown by Cartwight (1976) and Yoshihuku (1979); if one considers that the experimental errors in the measurement of q and p obey Gaussian distributions $\chi_q(\xi)$ and $\chi_p(\eta)$ with dispersion σ_q^2 and σ_p^2 respectively, then the experimental joint distribution will be given by the joint distribution of $q+\xi$ and $p+\eta$, that is to say the convolution of $f(q,p)$ with χ_q and χ_p; and the resulting distribution function is non-negative provided

$$\sigma_q^2\sigma_p^2 \geq \tfrac{1}{4}\hbar^2. \tag{22}$$

It is evident, then, that a non-negative distribution function $f(q,p)$ can come as near as one whishes to reproducing one or the other of the quantum-mechanical marginal distributions, which are thus seen as the limiting cases of infinite experimental precision; and even when both dispersions have to be taken into account, the uncertainty-like inequality (22) imposes no restriction that we can as yet achieve experimentally. For the fact is that the experimental validity of $|\psi(q)|^2$ and $|\phi(p)|^2$ for the experimental distributions has not received anything like as solid an experimental confirmation as one could wish. The best data available have been obtained from experiments with particle beams, where $\phi(p)$ is well defined but, because the system is not confined, $\psi(q)$ is not normalizable and the relation between the two is not simply that of Fourier transformation as required for the theory that interprets them as marginal-distribution amplitudes;[7] on the other hand, for confined systems measurements of sufficient precision seem to be very difficult. Since so far the exact form the marginal distributions given by a positive joint distribution function have not yet been worked out, the matter must be considered one more open problem; but it might be added that the theory of stochastic electrodynamics, to be discussed should differ slightly from the quantum prediction.

In summary we have more problems calling for future work than answers in this matter of the joint distribution of q and p; yet it may fairly be concluded that our failure so far to find fully satisfactory joint distributions cannot be ascribed to a fundamental weakness of the ensemble interpretation.

VI

We have so far discussed two of the three difficulties mentioned above that arise in the attempt to make the ensemble interpretation complete and consistent; but the third is in a way the most interesting. The EPR theorem (Einstein, Podolsky and Rosen 1935; Einstein 1949) leads to the conclusion that quantum mechanics is incomplete. For the ensemble interpretation this conclusion is inescapable and therefore raises the problem of how to complete the theory. The incompleteness of quantum mechanics takes a specific form in the ensemble interpretation, as we have

[7]The ordinary Fourier transform must be generalized to cover this case, and the function $|\psi(q)|^2$ must be interpreted as a probability distribution in the sense of Rényi. Certain conceptual problems arise that have not yet been elucidated.

mentioned: the formalism provides no guiding lines for the choice of an ensemble on which to base the theory. Any attempt to formulate a suitable physical model which would give rise to an appropriate ensemble must therefore lead us beyond quantum mechanics. We shall outline in the last section a possible theory that does just this; here we have first to face the third difficulty alluded to above: the existence of proofs that a completion of quantum mechanics, of the sort contemplated here, is impossible. In other words, we have to deal with the hidden-variable problem. Much of the extensive literature concerning it seems to obfuscate the matter, and I enter on it here only because I consider it to be much simpler that it is generally thought to be.

The first proof of the impossibility of hidden variables was given by von Neumann (1932) as a straightforward corollary of his derivation of the density-matrix formulation for quantum mechanics. For many years this proof was taken to be conclusive, though many people felt misgivings about its implications (*e.g.* de Broglie 1956); then certain weaknesses in the derivation of the density-matrix results were discovered, and for some time these appeared to justify a search for hidden-variable theories. The question had acquired importance because of the appearance of a fully worked out hidden-variable theory, due to Bohm (1952); this theory has important weaknesses from the physical point of view (in particular with an implausible space dependence), but it provided a counter example to von Neumann's theorem —or so it seemed. But further work, in particular by Kochen and Specker (1967) and by Gleason (1957), then reestablished the validity of von Neumann's density-matrix theorem, and therewith also the hidden-variable corollary. A good summary of these developments may be found in Bell (1966).

Yet, interesting though Gleason's theorem certainly is, it is irrelevant to the issue of hidden variables; for the validity of von Neumann's result was never in doubt, only his methods of proof. This, though obvious once it is pointed out, seems to have been generally overlooked. From the ordinary Hilbert-space formalism, based on pure states, the density-matrix formalism may be derived though an additional postulate which seems unchallengeable; inversely, a pure state takes the form of a special density matrix. Both connections can be found fully worked out in von Neumman's book. As a consequence, if the density-matrix theorem had to be given up in order to allow the introduction of hidden variables, the rest of quantum theory would also have to be given up, which is just what hidden-variable theories are intended to prevent.

Fortunately the way out of this dilemma is not difficult; in fact, it could have been found in von Neumann's book itself, for he carefully specified the hidden variables he proposed to exclude as deterministic, *i.e.* dispersionless. His corollary does not apply to stochastic hidden variables. This becomes clear when it is observed that the corollary is in fact the quantum-theoretical case of a much general result, valid for any statistical theory. The general case is a simple consequence of probability theory: consider the dynamical variable x of statistical theory, with a distribution function [8] $P(x)$. This theory is to be embedded in a broader theory which contains

[8] Here we use distribution functions (which are integrals over probability densities when these exist), to make the argument simple and general.

the hidden variable h as well as x; this means that we must find a joint distribution function $Q(x,h)$ such that $P(x)$ is the marginal distribution for x in it. Let $R(h)$ be the distribution of h and $P_c(x|h)$ the conditional distribution of x given h. We have, from standard probability theory

$$Q(x,h) = \int_{-\infty}^{h} P_c(x|h')dR(h'). \tag{23}$$

If h is to be a hidden variable of deterministic type, with the fixed value $h = h_0$, then

$$R(h) = \begin{cases} 1 & h \geq h_0 \\ 0 & h < h_0 \end{cases} \tag{24}$$

and thus

$$dR(h) = \delta(h - h_0)dh. \tag{25}$$

Substituting (25) in (23) we have

$$Q(x,h) = \begin{cases} P_c(x|h_0) & h \geq h_0, \\ 0 & h < h_0. \end{cases} \tag{26}$$

Therefore

$$P(x) = Q(x,\infty) = P_c(x|h_0). \tag{27}$$

Using (24) and (27) in (26), we have

$$Q(x,h) = P(x)R(h). \tag{28}$$

We conclude from (28) that x and h are statistically independent. Since this argument can obviously be carried through for the set x of the dynamical variables and the set h of "hidden" variables to be added to them, the variables of the original theory are statistically and thus also functionally independent of the h's, which therefore are irrelevant to any explanation of the behaviour of the x's. Nothing is gained by adding the h's. If we allow h to vary over a small interval, from h_0 to h_1, say, then

$$R(h) = \begin{cases} 1 & h > h_1 \\ 0 & h < h_0 \end{cases} \tag{29}$$

and the argument is still valid outside this interval. Only if this interval is large enough to be significant within the original theory can there by any genuine connection, statistical dependence or otherwise, between x and h.

Note that the conclusion depends entirely on the requirement of maintaining the character of the original theory *i.e.* that the introduction of the hidden variable h does not alter $P(x)$; if this requirement is dropped the conclusion that h must also be a stochastic variable no longer follows. Nor does the argument apply to the introduction of h as a new parameter in $P(x)$; it must be a new dynamical variable, in the sense of being at least statistically linked to the other dynamical variables.

Returning to the particular case of quantum mechanics, the situation with regard to von Neumann's corollary is, then, that its validity need no longer be disputed but its meaning has to be reinterpreted. Stating it positively, we draw from it the conclusion, not that hidden-variable theories are impossible, but that they must be stochastic theories. Seen in this way, the corollary provides a useful hint for further research, rather than figuring as an obstacle to it, as seen by the Copenhagen school. The hint is borne out by the Bohm theory and indeed all other hidden-variable theories that have achieved some sort of consistency: without exception their hidden variables are stochastic in nature and are not dispersionless.

It is sometimes said that if hidden variables cannot be dispersionless then they are useless. But it should be clear that this view is inspired more by a desire to return to a fully deterministic, Newtonian kind of physics, and while quantum mechanics (or rather the immense range of experimental results that it accounts for satisfactorily) should not lead us to abandon the ontologically fundamental status of reality, it should convince us of the limitations of a purely mechanistic physics. Moreover, the idea that a stochastic theory cannot provide the basis for completing quantum mechanics and so yield a deeper understanding of it ignores the lesson of statistical mechanics: here we have an entirely stochastic theory that has enormously enriched and broadened our understanding of thermal physics and has gone well beyond the limits of applicability of the thermodynamics it was intended to underpin.

But statistical mechanics, as already noted in section IV, holds the further lesson for us that the underlying classical mechanics differs markedly from it in character, concepts and main quantities. This will be relevant below.

VII

To sum up, we have seen that, from the point of view of the ensemble interpretation, quantum mechanics is incomplete, that any completion should be stochastic in nature, and that the resulting theory will likely be of very different character.

Several theories along such lines have been suggested in the past; the first would appear to have been due to Fényes (1952). They have not, on the whole, been very successful, and for this there are two reasons. One is a technical problem: the best-known stochastic process is, of course, Brownian motion, and this misled many workers into identifying the process underlying quantum mechanics with a Winger process (the mathematical model for Brownian motion); that the two, though related, are different was first shown by de la Peña and Cetto (1977a). There are now reasons, as we shall note, for believing that the process is not even Markovian. The other reason is that without a physical model to serve as starting point, background and touch-stone, a mathematical description of a stochastic process, however ingenious, will be somewhat arbitrary and even *ad hoc*. Of course one has the great advantage of free choice of a nice, simple, tractable mathematical problem if one ignores physical models; but one runs the risk that the nice easy problem can be quite misleading.

I propose therefore to outline here a theory that is far from having a finished form as yet, but does possess a plausible physical conception to base its mathematical structure on; and however provisional the present form of the theory may be, it has already had some significant successes. This theory, stochastic electrodynamics, owes its inception to Braffort and coworkers (Braffort, Spighel and Tzara 1954; Braffort and Tzara 1954; Braffort, Surdin and Taroni 1965; Surdin, Braffort and Taroni 1965) and independently to Marshall (1963; 1965 a,b) who gave it its name; it has been further developed by Santos (1975), Boyer (1975), de la Peña and Cetto (1977b), Claverie and Diner (1976); the last three include a review of the earlier work.

The underlying physical conception is simple: consider a charged particle, such as an electron; in its movements it emits electromagnetic waves described by radiation-reaction terms; if it is considered in isolation, it would therefore lose energy and, in the case of an orbital electron, fall into the nucleus. This is the classical picture, often considered as an argument for quantum mechanics. But —and this is the central point of stochastic electrodynamics— the electron is not isolated; all the other charges in the universe also emit radiations through the same mechanism, and since these radiations are evidently incoherent, the electron being considered is bathed in a stochastic radiation field. The problem to be solved is thus the motion of a classical charged particle under the joint effect of a stochastic electromagnetic field, the radiation reaction and any external force (*e.g.* the Coulomb attraction of the nucleus) that may be present.

The radiation reaction is of course well known; it is given by the Liénard-Wiechert potential, and for non-relativistic speeds is usually well approximated by a term proportional to the third derivative of the particle's position vector. In the same approximation only the electric component of the stochastic electromagnetic field need be taken into account. But because this is a stochastic force, we can at best write a Langevin-type equation, and can derive conclusions from it only if the probability distribution of this force is known. Here we see the significant advantage of stochastic electrodynamics over earlier theories as a physically grounded conception; for not only is the physical model plausible, but the stochastic properties of the background radiation field can be derived quite independently, from considerations of relativistic invariance (Marshall 1963, Santos 1974) and others (Jiménez, de la Peña and Brody 1980). The spectrum so predicted coincides with the quantum-electrodynamical one (ω being the frequency),

$$\rho(\omega) = \frac{\hbar}{2\pi^2 c^3}\omega^3 \tag{30}$$

but $\hbar$ is merely a proportionality constant that yields the amplitude of the fluctuations in the stochastic background field; the use of the symbol $\hbar$ anticipates that eventually ordinary quantum mechanics has to be recovered. This is the one point in stochastic electrodynamics where this constant is introduced.

The equation of Langevin type for a particle of mass m and charge e now becomes

$$m\frac{d^2x}{dt^2} = f(x) + \tau m\frac{d^3x}{dt^3} + eE(t) \tag{31}$$

the so-called Marshall-Braffort equation, in which

$$\tau = \frac{2e^2}{3mc^3} \tag{32}$$

and $E(t)$ is the component along x of the random electric field (for simplicity we only consider the one-dimensional case), for which the expectation value is

$$\langle E(t)\rangle = 0 \tag{33}$$

and the spectrum is given by Eq. (30). We shall not enter here into the details of how phase-space and configuration-space probability distributions for particles obeying Eq. (31) are derived, since they are complex and have already appeared in the papers cited. Certain points, however, are relevant here:

a) The stochastic process considered here is markedly non-Markovian; but as equilibrium is approached, the importance of the memory terms diminishes. Very close to equilibrium, the radiative terms in (31) tend to cancel out, and the configuration-space amplitude then satisfies the Schrödinger equation: quantum mechanics is thus the equilibrium limit for this theory.

b) If we assume that an equilibrium state exists (this has not yet been proved for all relevant cases) then it will be reached rather rapidly; for instance, the relaxation time for an atomic orbital electron is of the order of $10^{-23}s$. Before equilibrium has been reached, the configuration-space distribution and the moment-space one are not necessarily Fourier transforms of each other and so the Heisenberg uncertainty relations may be violated (in the ensemble-interpretation sense, evidently, that the product of the statistical dispersions may be less than $\frac{1}{4}\hbar$). In a particle beam, no equilibrium state in the strict sense can exist, if we take the beam to be infinitely long; but even in a finite beam only quasi-stationary states should be expected, and so the actually observed distributions of q and p could slightly diverge from $|\psi(q)|^2$ and $|\phi(p)|^2$, respectively; this purely qualitative argument was made use of above.

c) As the detailed analysis of the harmonic oscillator (de la Peña and Cetto 1979) in this theory shows, the quantum-mechanical discrete states are recovered. That there exists a ground state of finite energy, at which radiation reaction and absorption from the stochastic field balance each other, and that this state is stable, could already be seen by simple "hand-waving" arguments (Claverie and Diner

1976); that the energies of the excited states should also be correctly reproduced was not so clear; but that these states should be shown to have widths (in the sense that the instantaneous energy fluctuates and is equal to the quantum value only in the mean) was surprising. This last result, it should be noted, fits in well with the ensemble interpretation but not with the orthodox Copenhagen views.

d) A considerable number of detailed predictions have already been derived from stochastic electrodynamics; these range from the Planck black-body spectrum to the non-relativistic Lamb shift, and generally agree very well with the predictions of standard quantum theory. But the theoretical structure is far from complete, largely because no general mathematical formalism has yet been developed, and problems have to be tackled piecemeal. Nevertheless, for a great many questions rough qualitative arguments are available that show that at least in principle it should be possible to answer them satisfactorily within the framework of stochastic electrodynamics.

e) Stochastic electrodynamics does not agree everywhere with quantum mechanics; unlike the ensemble interpretation of the latter, it is a new theory. However, so far it has not proved possible to make predictions from it that differ in an experimentally accessible way from the standard quantum results.

There is one feature of stochastic electrodynamics to which attention should be drawn: it is from its very inception a theory of open systems. In more than one sense this constitutes perhaps a more decisive break with classical physics than the Copenhagen interpretation ever achieved. We are very far from understanding as yet all the implications of this fact; but a highly significant consequence is that in this theory there are no exact conservation laws, —only statistical ones. Hence all symmetries will hold only on the average, and we may guess that conceivably this will provide a mechanism to comprehend "spontaneous" symmetry breaking; but this is speculation. What seems clear is that the ultimate consequences may prove even more fundamental than the appearance of irreversible phenomena and hence of "time's arrow" when thermodynamics first —and as we now see, rather timidly— broke through the restriction of fundamental physical theories to closed systems.

Thus we have here the beginnings of a hidden-variable theory that is intended to underpin but also to go beyond quantum mechanics; in fact, the direction in which such a theory should be sought for was pointed to by that interpretation. The Copenhagen interpretation, on the other hand, never encouraged such a development and in the view of some even forbade it.

VIII

By way of a general conclusion we may say that the ensemble interpretation, so far from being nonexistent as has sometimes been stated (Hanson 1959), forms a consistent body of ideas that removes or at least permits clearing up the peculiar paradoxes arising in connection with the Copenhagen interpretation, that sheds light on certain questions otherwise not even touched, and that rather strongly directs further research along promising new lines. Nevertheless, not all its problems have yet been satisfactorily settled; much work remains to be done, though there is little reason to expect significant conceptual breakthroughs from such investigations: it is rather a matter of adequately completing what has already been outlined.

The future of stochastic electrodynamics is another matter; here even fundamental conceptual problems may have to be solved before it is possible to say that this theory has firm foundations.

But what is to be stressed is that the philosophical problems felt to be peculiar to quantum mechanics in the past simply dissolve in the ensemble interpretation; this leaves room for tackling the genuine problems.

I would like to express my gratitude to J.L. Jiménez to whom many of the results of section V as well as other points are due, and to Luis de la Peña, for numberless useful discussions and thorough reading of the manuscript.

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