Quantum theory and the mind brain relation

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QUANTUM THEORY AND THE MIND BRAIN RELATION

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ABSTRACT

In reductionist biology, mental states are brain states and the brain obeys the laws of a physical world existing independently of and prior to minds. This account is invalidated if the physical laws themselves involve essential reference to mental states.. The quantum theory has usually been presented in a form making such reference. .

To remove the need for this, the first step is to accept that quantum theory applies only to fields and to entities embedded in fields. Ehrenfest´s theorem then shows how systems obeying Newtonian mechanics, including objects of everyday life, appear as persistent patterns showing none of the indeterminacy associated with features of the underlying field.

The theorems of Gleason, Kocken and Specker demand that the quantum theory should leave a degree of indeterminacy in the pattern of the fields it describes. Any interaction of a quantum system with its environment therefore requires a definite selection of a unique pattern of behaviour within the range of indeterminacy.Such interaction is continuous, and there is no rôle for a mental state in this selection.

It would be consistent with the formalism of quantum theory if a localised interaction in a system caused an instantaneous removal of indeterminacy over an arbitrarily large volume, in apparent conflict with the special theory of relativity. This conflict is not removed by any appeal to the effects of mental states.. However, a consistent interpretation of quantum systems as fields throws doubt on the claim that the event correlations in the experiments of Aspect and his colllaborators are evidence of causal propagation at speeds greater than that of light.

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A physical world, independent of mental states and pre-existing them, is a presupposition in many evolutionary account of experience. In some of these,states of the mind are taken to be reducible to states of the brain, and processes in the brain are taken to obey the same laws as those of the physical world. This reductionist approach is barred if the laws of the physical world cannot be formulated without reference to mental entities. It is of great significance therefore outside physics that, throughout its history, quantum theory has commonly been presented as making essential reference to observers, conscious states or measurements, and hence, directly or indirectly to mental states. The aim here is to show that such reference can be eliminated without affecting the way quantum theory is actually used.

In general, quantum theory assigns probabilities to a range of outcomes, rather than a unique outcome, to a quantum process. It gives no account of the mechanism by which the actual outcome is selected from the range of possibilities. Moreover, if the quantum theory is supposed to apply to all possible systems, including for instance systems of free particles or of macroscopic bodies, it is difficult to suggest any plausible mechanism for this selection. It is in this connection that some have seen a rôle for mental states. In a different approach, the obviously permissible assumption is made in this work that the theory applies only to those kinds of physical systems for which physicists actually use it. It then appears that quantum theory is primarily the mechanics of the microstructure of the electromagnetic field, although also adaptable to fields associated with the weak and strong forces of physics. Quantum particles are then aspects of the behaviour of fields, the structure of which are determined by all boundaries, internal and external, of the region containing them.

Ehrenfest's theorem, derivable within quantum theory, then shows how the quantum substructure underlies and is compatible with systems which obey classical mechanics and lack the currently irremovable indeterminacy of some of the properties of their microconstituents such as electrons. This solves the so-called "measurement problem" and eliminates necessary reference to observer-related measurement systems.

Indeterminacy must be retained for the quantum microstructure to explain the single outcome ,from the range of possibilities permitted by quantum theory, without recourse to interaction with observers (or other entities not supposed to be governed by quantum laws).. This then leaves quantum theory entirely independent of any essential reference to mental entities. The posibility need not be excluded that indeterminacy at the quantum level may be based upon determinacy at some finer level of analysis, e.g. in some "hidden variable" formulation, but this would not affect the present arguments, and it is certainly not required to justify them.

It would be consistent with the quantum theory to see the range of outcomes permitted by the theory as accessible to a single quantum system, and hence to take the probabilities as propensities of a single system, rather than as frequencies in a population of similarly prepared systems. This would, however,leave problems in our account of macroscopic bodies, and it is therefore necessary to suppose that both varieties of probability are

involved, with a limited range of propensities open to each system of a population and a wider range of possibilities arising from a frequency distribution in a population of systems.

Interpreted in this way, the quantum theory does not require action at a distance, or causal effects transmitted at speeds greater than that of light, to account for correlations it predicts between separated parts of quantum systems. In the present account, the existence of such effects is an empirical matter, at present undecided. Experiments, such as those of Aspect and his co-workers on photon pair correlations, do not unambiguously exhibit action at a distance. This is because radiation emission, polariser reflection and photomultiplier excitation are all field-related or field induced. The field structure connects the choice between detection and non-detection at a photomultiplier to a certain consonance betwen polariser setting and emission parameters. No causal effect is therefore transmitted : it is inherent in the set-up. Aspect's second experiment switched beams between pairs of polarisers at fixed angles, but low detection rates at the photomultipliers leaves open the possibility that only photons with emission parameters consonant with polariser angles actually met would actuate the photomultipliers.

This presentation not only eliminates mental entities, it is also consistent with conceptual schemes used to interpret the formalism of quantum theory by working physicists. It is very close to the ideas of the so-called Copenhagen school, despite the support the latter is supposed to give to subjectivist interpretations. The main departure here from Bohr and Heisenberg is the rejection of their static view of human powers of representation of physical entities in mental pictures. With this proviso the account given here is the metaphysics of the practising physicist.

QUANTUM THEORY AND THE MIND-BRAIN RELATION

BACKGROUND

Since the 1920's, the quantum theory in physics has frequently (and perhaps usually) been presented in a form which would have profound implications in philosophy.More narrowly, keeping within the boundaries of science, the quantum theory in this form would appear to be incompatible with that school of neurology which proceeds from a reductionist standpoint, taking mental states to be brain states, with the ultimate ambition of explaining psychology in terms of processes in neuron networks.

I am here concerned to show that the quantum theory need not be presented in this form. In doing so, I believe I also rid quantum theory of what some would consider its more puzzling philosophical implications, such as its apparent incompatibility with an objective physical world preceding and existing independently of subjective entities such as minds or observers. However, I emphasise the implications for neurobiology because these have had less attention than those for philosophy, and also because I find it particularly odd that the implications of one widely practised branch of science on another should be so neglected.

As an illustration of this neglect, a comprehensive review of the mind-brain problem (Churchland, 1986) gives a very full account of the reductionist controversy in biology in general and in mind-brain studies in particular. However, it contains no reference at all to the support which the quantum theory in a widely accepted form gives to dualistic and non-reductionist theories of the mind-brain.

This is not because practitioners in the field of neurobiology are totally unaware of the general character of quantum theory. Indeed, Churchland's review gives due attention to matters in quantum theory which appear to her to have relevance to her field, for example when she quotes the use of vectors in Hilbert spaces in quantum theory as a precedent for their application to neural networks. She also discusses the explanation of chemical bonding in quantum theory as an example of reductionism in a non-biological science.

Nor is it because the reductionist school in neurology holds such sway that the difficulties it may engender in physics appear of little consequence: as Churchland's review shows,reductionism in biology is currently a highly controversial matter. One might have supposed therefore that opponents of reductionism would have quoted the subjectivist trend in quantum theory in support of their views. This, it appears, is hardly ever done.

My concern here is to show that the supposed implications of quantum theory for reductionism in biology are not sustainable, rather than to explain the lack of inter-disciplinary discussion of them. My own interest in this matter arises of course from a reluctance to accept that the quantum theory conflicts with accounts of the mind-brain relation which I find acceptable.

REDUCTIONISM IN NEUROBIOLOGY

No detailed account is needed here of the various interpretations of the mind-brain relation which underlie investigation in a range of biological, psychological, neurological and artificial intelligence topics. Churchland's review gives a full coverage of these matters, with extensive references. All that is required are illustrations of the kind of thinking in these fields which appear incompatible with subjectivist accounts of the quantum theory.

Starting from relatively uncontroversial matters, the brain is a collection of cells organised to control bodily activity, and to conduct this control in relation to representations of the world. The brain also processes the sensory signals used to modify these representations or add to them. Associated with the brain are mental states such as perceptions, thoughts, feelings and sensations. Some mental states are conscious states. Mental states influence and are influenced by the processes and properties of the brain with which they are associated. The mind is the locus of mental states.

To turn to controversial matters, accounts of the mind-brain relation may for present purposes be classified as dualist or as reductionist. According to the dualists, processes in the mind cannot be interpreted completely in terms of processes taking place within the brain and obeying the same laws as non-mental processes. Some dualists view the mind as a substance separate from the brain, even capable of existence independently of it. Other dualists accept that all mind states are brain states, but still maintain that mental processes are not, and never will be, explicable in terms of physical processes in collections of brain cells.

As opposed to the dualists, reductionists see mental states as brain states and hold that the processes and properties of the brain follow the same physical laws as those of non-mental processes.

The reductionist does not of course argue that neuroscience is yet sufficiently advanced to give a full account of mental processes in terms of neural networks. He would point out here to analogies with, say, chemistry or thermodynamics. The laws of chemistry can now be interpreted in terms of quantum theory applied to electron systems in atoms and molecules, but until the 2nd or 3rd decade of this century the reduction of chemistry to physics, athough widely expected, was not in fact achievable. Similarly, thermodynamics can now be based on statistical mechanics but the development of classical thermodynamics preceded its statistical interpretation.

Looking at the problem in this way, it is tempting to suggest that we are dealing with an empirical question, in the sense that we shall know whether psychology, perception and memory can be reduced to processes in neural networks when the reduction has in fact been achieved. Until then, it may be said, we can leave the question in suspense. This view is unappealing to either side in the controversy. Many dualists believe they can show here and now that a complete reduction of mind states to brain states is impossible. They say, for instance, that the structure of language itself forbids it, in that there are propositions about the mind that cannot be propositions about the brain and vice versa (A neuronal network obeying physical laws cannot harbour a desire for revenge, and I cannot have a tumour in my mind).

The reductionist in his turn might be unwilling to leave the matter in suspense because the supposition of a separate mental substance or of separate laws governing mental processes gives rise to grave difficulties for an understanding of the evolution of living organisms. At what stage did this mental substance appear, or at what stage did non-physical laws begin to operate. Has a bacterial cell got its quota of mental substance ? Is it likely we shall ever have to appeal to non-physical laws to account for processes in the bacterial cell ?

However this may be, it is clear that the reductionist position becomes untenable if the physical laws themselves imply the existence of mental systems not governed by physical laws. Mental states cannot be interpreted in terms of neural networks obeying physical laws if the physical laws themselves cannot be understood without appealing to mental processes.

Later I will discuss in more detail the reasons for supposing that quantum theory has these implications (and of course why I consider these reasons insufficient). For the present the matter can be put briefly as follows. In general, quantum theory gives probabilities to a range of possible outcomes of a given initial state. The unique outcome presented in any particullar case is supposed in some interpretations to arise in the course of interaction of the physical system with a mental system (for example, a state of consciousness). In less extreme interpretations, the interaction giving the unique outcome is between the physical system and a measurement system. Since a measurement system can be identified as such only by reference to mental states (i.e.those of the observer using the measurement system), this less extreme interpretation is still fatal to the reductionist argument, which to avoid circularity requires physical laws which make no reference, implied or explicit, to mental states.

It is my intention then to argue that quantum mechanics, as it was developed in the 20's and 30's of this century, can be rid of these subjectivist interpretations without affecting in any way what working physicists so successfully do with quantum mechanics. To do this, I must first give some consideration to aspects of classical dynamics and classical electromagnetic theory, since quantum theory makes essential reference to concepts such as energy and the electromagnetic field which are characterised in the classical theories rather than in quantum theory.

CLASSICAL DYNAMICS

For the present purpose, the central feature of classical dynamics is that of determining the evolution in time, from a given initial state, of any physical system for which a Hamiltonian function can be defined. The Hamiltonian is a function of coordinates specifying completely the configuration of the system, and also of a set of quantities, equal in number to the coordinates, known as "momenta". Corresponding pairs of coordinates and momenta obey a set of Hamilton's equations

$$\frac{dp_r}{dt} = -\frac{\partial H}{\partial q_r} \qquad \frac{dq_r}{dt} = \frac{\partial H}{\partial p_r}$$

where t represents time, H is the Hamiltonian function and q_r, p_r are a coordinate and its corresponding momentum respectively.

We are concerned with what Whittaker (1944) called "natural systems", for which H is numerically equal to the total energy of the system, i.e. the sum of its kinetic and potential energy. Whether suitable kinetic and potential energy functions can be formulated for a particular physical system is an empirical question, but such functions are well known for a large variety of physical systems.

For the present purpose, the important point is that the existence of a Hamiltonian and, if it exists, the coordinates required for its expression, are matters for investigation for each particular class of system. The relevance of this to electromagnetic systems will be considered below.

Another feature of classical dynamics will also be relevant. During the 17th and 18th centuries the development of the concept of energy was slow and difficult (Mach 1960). It was not (and may be still not now)easy to get a mental image of a common entity to be associated with the kinetic and potential energies of a system, and still less easy to generalise any such image to the wide variety of systems to which the concept of energy is applicable.

The concept of energy would not have been explicable in terms of human experience before the 16th century. The widening range of human experience requires a widening range of concepts. I shall argue below that a failure to recognise this led Bohr and the Copenhagen school to make an unjustifiable distinction between entities of quantum mechanics and those of classical mechanics. The entities appearing in quantum mechanics have features not readily modelled in terms of those familiar in early physics, just as energy in classical mechanics was not readily modelled in terms of the ideas of 16th century science. In due course quantum systems will lose their strangeness, just as kinetic and potential energy have long ago lost their strangeness. With this proviso, the views of Bohr and the Copenhagen school will appear close to those which I am advocating here.

CLASSICAL ELECTROMAGNETISM

I shall argue below that the philosophical problems of quantum theory arose in part from an unjustified assumption that quantum mechanics, in the form considered here, had the same wide range of application as classical mechanics, but extending beyond the latter to micro-phenomena. For macroscopic processes, classical mechanics was to be regarded as a sufficiently accurate approximation, but becoming increasingly invalid at the atomic scale and below. In principle, however, quantum mechanics was supposed to embrace all processes covered by classical mechanics, although giving results indistinguishable for practical purposes from those of classical mechanics on the macroscopic scale.

In fact, quantum mechanics in the form considered here was applied almost exclusively to electromagnetic phenomena on the small scale (including such things as atoms and molecules or electrons in crystalline solids,for which the energy is expressed in electromagnetic terms). At this point then it is appropriate to consider some important features of electromagnetic phenomena.

Unlike, say, systems of massive bodies interacting by gravitational forces, electromagnetic systems cannot, except in special cases, be represented by Hamiltonian functions of interacting particles, forces between pairs of which act along the lines joining them. For a pair of charged particles in relative motion, the electrostatic part of the force between them does indeed act along the line joining them, but the magnetic part of the force does not.

This feature of electromagnetic systems has for the present purpose an important consequence, namely that the Hamiltonian function cannot be expressed in terms of the sum of the kinetic energies of point particles and of a potential energy dependent only on the instantaneous coordinates of point particles. (Goldstein 1959, p9). For the simplest case of a charged particle in an electromagnetic field, the Hamiltonian is (Goldstein,1959, p.222)

$$H = 1/2m(p-qA/c)^2 + qV$$

where m is the mass associated with the charged particle, q its charge

p is the canonical momentum vector

A is the vector potential and
V the scalar potential

c is the velocity of light

I take this formulation of H to imply that in an electromagnetic system the charged particles are embedded in a field, and the mechanics of the system cannot in general be treated without reference to the field. The field quantities A and V may of course be regarded as determined by the distribution of charges and their velocities, but it is equally valid to regard the charges and their velocities as determined by the configuration of the field quantities A and V. Whichever point of view is taken, allowance is made for macroscopic boundaries to the field (screens, apertures, equipotential surfaces etc), the effects of which are supposed to be known without detailed analysis of their fine structures.

The charged particles of classical electromagnetics are therefore embedded in fields, the structures of which reflect not only the distributions and velocities of the charged particles but also the macroscopic boundaries of the field. In what follows I take this generalisation to apply to all systems to which quantum mechanics can be applied.

I am claiming therefore that neither classical electromagnetism, nor quantum theory, which is a fine structure refinement of it, have any place for the isolated, or even intermittently isolated, particle. An isolated particle may travel along a definite trajectory defined by apertures in screens. The screens themselves, and any apertures in them not characterising the trajectory, may be supposed to have no influence on the behaviour of the particle. The presence of the screens merely assures the absence of particles not belonging to a particular trajectory.

Particles, screens and apertures of this kind are not the subject of classical electromagnetism or of quantum theory. For these theories, particles are aspects of the behaviour of fields, and the structure of a field is determined among other things by the distribution of the boundaries (including screens and apertures) relevant to any particular case. This property of a field is of course an essential characteristic of our understanding of what a field is.

For our present purpose, quantum mechanics may be regarded as a theory of the electromagnetic field, since its applications to non-electromagnetic systems were at first few and received little attention in discussions of the philosophical problems of quantum theory.Systems involving the weak and strong forces have since been treated by developments of quantum mechanics but I shall not deal with these,except to say that here too we appear to have fields as primary entities. It has long been recognised that many problems are solved if quantum systems are always primarily field systems, and never systems of autonomous particles. However, early attempts to develop this idea were frustrated by the choice of the Schrödinger function as the field function. The Schrödinger function has representation in a space of as many dimensions as the system in question has degrees of freedom, and cannot therefore serve as a field function in real 3-dimensional space.

For my present purpose it is not necessary to suppose that much or all of quantum theory can be derived by relatively minor adjustments to classical electromagnetic theory. It is no doubnt of great significance that so much of the broad character of quantum systems is given by the use of Maxwell's equations along with the assumption of a universal randomness in the electromagnetic field, as is done in so-called stochastic electromagnetic theories (see for instance T. Marshall. E. Santos 1988).For my purpose,it is enough that the quantum theory is itself that modification required to classical electromagnetic theory on the small scale when the effects of the universal zero point field can no longer be neglected.

It is in any case irrelevant to our present purpose that the quantum theory will in due course be embedded in some more general theory. It is fundamental to the philosophical stance underlying this paper that any scientific theory which has broad practical application does so in virtue of an isomorphism between elements of the structure of the theory on the one hand and elements of the structure of an objective reality on the other hand. It must follow that any valid scientific theory must permit presentation in a form which is free from subjective aspects such as observers, deities, consciousness etc. Ptolemy's geocentric astronomy was successfully applied by ancient navigators, and Ptolemy's astronomy is easily freed of reference to deities embodied in the stars and planets. Egyptian astronomy gave rise to a calendar and to useable predictions of the annual Nile floods, and again Egyptian astronomy is easily detachable from the pantheon associated with it by an Egyptian priestly caste.

Quantum theory too has had wide practical application in the form with which I am here concerned, and it too must be presentable without reference to subjective entities. This is the purpose of the present paper, and it is apparent that this purpose would not be served merely by showing that the quantum theory with its subjective trappings will in due course be replaced by some more general theory without them. It is the quantum theory in the form used to interpret spectra and solid state physics which must be freed of subjective reference, and to do this I must now look at this theory more closely.

THE SCHRÖDINGER EQUATION

I shall discuss quantum thoery as developed on the basis of the Schrödinger equation. It appears generally accepted that the alternative development using matrix mechanics and commutivity rules is formally equivalent to the form I shall discuss, and I therefore assume that any conclusions I reach about the latter apply to the former also.

The Schrödinger equation (1) for any quantum system is a second order partial differential equation relating the Schrödinger function U as dependent variable to time and to variables specifying the configuration of the system

$$HU = i\hbar \frac{\partial U}{\partial t} \qquad (1)$$

Here H is a linear differential operator derivable from the Hamiltonian function of the system, considered classically.

A solution to this equation, for given initial and boundary conditions, is an expression for the Schrödinger function in terms of time and the configuration variables.

Unlike the solutions to Hamilton's equations in classical dynamics, the solution to the Schrödinger equation does not give a unique configuration for the system for a particular value of the time variable. Instead, it allows the calculation of a probability that at a particular time the system will react as if it had a particular set of values for the coordinates specifying its configuration and for the corresponding momenta. (I also leave for later discussion the question whether the system may be supposed to possess these values before it reacts).

The procedure is as follows. For each coordinate and momentum characterising a quantum system there can be found a corresponding linear operator. The identification of the linear operator appropriate to a particular case is not a straightforward matter, and appears to depend upon intuition based upon classical mechanics rather than quantum mechanics. However, for my purpose I need not pursue this matter. I will simply assume that the possibility of identifying such linear operators is a characteristic of quantum systems. This may imply a restrictive view of the scope of quantum mechanics, but it will be recalled that I start out with doubts about its universality. It is sufficient for me that for systems represented in Cartesian coordinates the appropriate linear operator is obtained from the classical expression for the corresponding dynamical variable by treating each coordinate x as a multiplicative operator and its paired momentum by the operator $-i\hbar\partial/\partial x$.

Such an operator, denoted by L, is used to define a set of eigenvalues a_n and eigenfunctions v_n by the equation

$$Lv_n = a_n v_n \qquad (2)$$

If certain mathematical conditions are satisfied by L, such eigenfunctions exist and have a property known as "completeness" which implies that arbitrary functions of quite broad types can be expressed as sums of series of the functions. The operators occurring in the applications of quantum theory satisfy the conditions, so that it is possible to express the Schrödinger function U for an arbitrary quantum system in the form

$$U = Sr_n v_n \qquad (3)$$

where S represents a summation which may become an integration in a region where the distribution of eigenvalues becomes continuous.

Now it is supposed to be known how the possession of a set of values for its dynamical variables governs the interaction of a quantum system with other systems. The nature of this knowledge is again not a straightforward matter, and appears to involve in varying degrees classical mechanics, quantum mechanics, and adaptations of procedures to fit the facts in well-investigated analogous situations. It is no part of the present task to consider to what extent the use of quantum mechanics depends upon ad hoc appeals to classical mechanics and to special rules not deducible from the postulates of quantum mechanics (as when Schrödinger functions for electrons in molecules are in part derived from the known geometries of molecules). It is sufficient here that the working physicist can relate the behaviour of a quantum system to values of its dynamical variables.

The values of dynamical variables of a quantum system are derived from its Schrödinger functions in the following way. According to quantum theory, the possible values of a dynamical variable are the eigenvalues a_n of its corresponding operator L as given by equation (2). The probability that in any particular situation the quantum system exhibits behaviour appropriate to the value a_n of the dynamical variable is proportional to the square of the modulus of the coefficient of the corresponding v_n in the expansion of its Schrödinger function in terms of the eigenfunctions of the operator corresponding to the dynamical variable. The set of eigenfunctions can be normalised so that

$$P_n = |r_n|^2 \qquad (4)$$

where P_n is the probability that the system would exhibit behaviour appropriate to the value a_n of the dynamical variable and r_n is the coefficient of v_n in equation (3).

The above follows closely the usual treatment of the Schrödinger equation in textbooks (e.g. chapter 3 of L.I.Schiff, 1968) but differs from the majority of them in one respect. For me, the P_n in (4) measures the probability that the system should exhibit behaviour appropriate to the value of a_n, whereas most textbooks take the probability as that of getting a_n as the result of a measurement of the dynamical variable. The former assumption is more general, since any measurement on a system is an exhibition of a particular behaviour by the system, but the converse is of course not true.

An example may clarify the distinction I am making. A track in a bubble chamber is a measurement of the positions of a particle at a succession of times.A trace in a rock of a Devonian radioactive decay is a similar physical process but it is not a measurement.

The modification to the common textbook approach is necessary to the objective of ridding quantum theory of all reference to subjective entities, since measurements can be distinguished from interactions in general only by reference to a measurer. It leaves the subsequent development of quantum mechanics unaffected.However, the view taken on what happens to a quantum system in the absence of measurement (in particular in the absence of an interaction between the system and a mental state) has profound implications for any account of the relation between physical states and mental states.

. It is worth noting that the denial of any special distinction between measurements and other interactions has always had support among some physicists and philosophers. Among these are Born (Jammer 1974, pages 162,163) Jordan (ibid, page 488) Schrödinger (1935- "States of a microscopic system which could be told apart by a macroscopic observation are distinct from each other whether they are observed or not")Blokhintsev (1964), Fock (1965), Groenewald (Bastin 1971 page 43), Bunge (Ibid page 163), Landé(1965) Krips (1987), Bub (1974), Cartwright (1983 page 195), and many others.

In this connection one should note the very widespread tendency among physicists to apply the term "measurement" in such a broad sense that it becomes practically synonymous with "interaction". Sudbery (1984) noted that it is "universally assumed in textbooks and research papers alike that decaying systems decay at some definite time even when not subject to measurement"

It appears therefore that the elimination of any reference to measurement in the postulates of quantum theory gives rise to no problems.

TO WHAT DOES THE SCHRÖDINGER EQUATION REFER ?

Clearly the Schrödinger equation governs the time evolution of the statistical properties of quantum systems, in some sense of the word statistical. First I wish to argue that there is no acceptable sense of this word for which quantum theory can be seen as the statistics of systems of particles. To do this, I need to examine the characteristics normally regarded as essential to a particle.

For most people, a particle has a unique trajectory, along which its location is a function of the time. It must also conform to some minimal standards of dynamical behaviour, otherwise any interactions of a quantum system could be interpreted as evidence for a particle or particles following trajectories of some "ad hoc" kind. Krips (1987) in this connection requires a "minimally Newtonian property" such that particles "will not change direction without an external force being impressed upon them". I am not sure whether this is adequate. Wherever an electron produces a scintillation on a fluorescent screen, could we not construct an "ad hoc" force field to account for its getting there ?

I would prefer an adaptation of Krips' criterion, and require merely that a particle should have a trajectory, and that it should be possible to isolate the trajectory by screens and apertures which are supposed to leave it unaffected. (It is precisely our inability to do this for an electron trajectory through a screen with a pair of apertures which throws doubt on the particulate status of the electron). My adaptation of Krips replaces the need to identify the effects of an external force by the supposition that we can recognise physical boundaries which we know to exert no force upon an electron in the relevant trajectory.

The availability of such screens would then make possible the production of particles with known values for both position and momentum. This was the basis of the arguments of Einstein, Podolsky and Rosen (1935). Bohr's response (Bohr, 1935) in effect questioned the possibility of the required arrangement of screens and apertures.

The argument started by Einstein, Podolsky and Rosen took on a very different character as the years passed, and it came, as will be seen below, to be concerned with the reconciliation of quantum theory and the Special Theory of Relativity. At this stage it is the original matter at issue which concerns us. Einstein and his colleagues assumed screens and apertures which defined but did not influence particle trajectories, and hence arrived at particles with positions and momenta. Bohr sought to show that such arrangements of screens and apertures could not be constructed.

Neither the Bohr-Einstein discussion, nor any other source I have been able to trace, has given me an altogether satisfactory account of the minimum essential criteria for particulate status. Since I believe that much of the subjectivism which has got into quantum theory arises from the supposition that the theory applies to particles having too much in common with everyday particles or with the particles of classical dynamics, it is perhaps better to start from the other end and show how limited is the resemblance between quantum entities and classical particles. This will be the basis for my supposition that quantum systems are always fields, showing particulate properties as secondary features.

ARE THERE PARTICLES IN QUANTUM THEORY?

One way of doing this is to take a particular instance of a quantum system supposed to be composed of particles and show that these particles do not satisfy any reasonable minimal criteria for particle status. Jammer (1974 page 169) mentions the ground state of the electron in the hydrogen atom for this purpose.

$$U = (a^3/\pi)^{\frac{1}{2}} \exp(-ar) \qquad (5)$$

where U is the Schrödinger function, r is the radial position coordinate for the electron and a is a constant. The energy of the ground state E is given by

$$E = p^2 / 2m - e^2/r \qquad (6)$$

where p is the magnitude of the momentum vector and e the charge on the electron.

Now E has the value -13.55 electron volts. Hence $p^2/2m$, which is the kinetic energy of the electron considered as a particle, is negative for any value of r exceeding a value R given by

$$e^2/R = 13.55$$

giving a value of R of about 2 Å . (I have allowed myself some flexibility in the choice of units so as to avoid multiplicative constants)

Now the probability of a hydrogen atom in its ground state interacting with some other system as if the electron were at a distance greater then 2Å from the nucleus is given by

$$P = 4\pi \int_R^{\infty} r^2 |U|^2 dr = 0.23$$

In other words, if we wish to regard this electron as a particle, and U as a statistical function determining the frequency of configurations for the hydrogen atom in its ground state, then 23% of hydrogen atoms in the ground state contain electrons with negative kinetic energy.

I am not of course supposing that anyone applying quantum mechanics to atoms and molecules thinks of the bonding electrons as particles. In valency theory for example, an electron signifies little more than the unit by which a continuous distribution of electron stuff can change, and it is the shapes and energies associated with these distributions which matter. However, it is not merely that the practitioner does not think of electrons in molecules as particles, rather that he would not be able to if he wanted to!

Such examples could be multiplied indefinitely. However, a completely general statement covering all the special cases appears to require rather sophisticated mathematics to derive. The so-called "Uncertainty Relations", which are readily derived from the Schrödinger equation, are of no help. For a free particle, these have the form (Schiff 1974, p.61).

$$\delta x . \delta p \geq \hbar/2 \qquad (7)$$

where δx and δp are respectively the standard deviations from their mean values of a coordinate x and its corresponding momentum p. Such a relation does not deny the existence of a particle with a definite position and definite momentum, since it can be regarded as specifying the minimum ranges of values of x and p in a set of systems with the same Schrödinger function, each of which contains particles with sharp values for x and p. Popper (Tarozzi and van der Merwe, 1985, p.4) is among many who have taken this view of the Uncertainty Relations.

THE THEOREMS OF GLEASON ,KOCKEN AND SPECKER.

For a general demonstration of the impossibility of fitting systems of particles with sharply defined coordinates and momenta into the quantum theory we may turn to a series of papers generated by the work of Kocken and Specker (1967), based in turn upon a purely mathematical theorem on the mapping of Hilbert spaces due to Gleason (1957). Important contributions were made by Maczynski (1971), van Frassen (1973) and others. Reviews of the implications of these papers are given by Krips (1987) and by Redhead (1987), and it is on these that I have in the main drawn in what follows.

A quantum system is characterised by dynamic variables, such as coordinates and momenta. These variables have a one to one relationship to linear operators in the way described on page 8 above, and the eigenvalues of each operator are the possible values which can be exhibited by the corresponding dynamical variable in an interaction with another system. The question then is whether these exhibited values can be supposed to be possessed by the system immediately before the interaction. Unless they can, we cannot follow Popper and other realists of similar inclination in relating the Schrödinger function to the statistical range of dynamical variables, each with a sharp value in any particular system.

The Kocken-Specker theorem and its later developments shows that, unless we make what appear to me highly implausible assumptions, a quantum system cannot possess a full set of sharply defined dynamical variables. In particular, a system of particles each with sharply defined position coordinates and sharply defined momenta, cannot be a quantum system.

Now in presenting the matter in this bald and summary fashion I am taking grave liberties with the rigorous presentations in the sources I have quoted. It is not by any means a simple question whether for any particular quantum system there can be said to be a one to one relation between operators of a particular class and dynamical variables of the system. I believe, however, that attention to mathematical rigour is not necessary to my purpose. I am concluding from Kocken-Specker et al that the simplest way of eliminating the sources of subjectivism in quantum theory, through Popper's style of realism, is not open to me. If I am wrong in this, then my task is that much simpler. However, I strongly suspect that Popper's realism is untenable, and I will therefore continue on the assumption that the commonly accepted implications of the Kocken-Specker results are essentially correct.

If the Schrödinger function is not simply a source of probabilities, in the frequency sense, for dynamical variables in an ensemble of similarly prepared quantum systems, then what can it be? Well, instead of treating the probabilities as frequencies in an ensemble, we can treat them as propensities of a single system to behave in certain ways (In fact I shall end up using both sorts of probability , but let us put this aside for the present). We do not then have to assume that the system before an interaction possesses the sharply defined coordinates and momenta that it exhibits in the interaction. The Kocken-Specker theorems then present no problems.

Now there is nothing mysterious about a system with such properties. An electromagnetic system produces a scintillation on a screen which we interpret as an electron from the system interacting with the screen. We do not have to assume that immediately before the interaction there was a sharply defined packet of charge, energy, momentum and spin at precisely the position where the scintillation is observed. All the quantum theory tells us is the probability of such an interaction at a particular time and place. It does not tell us that the interaction was uniquely determined by the configuration of the system immediately before the event. Note that by configuration I mean a set of values for the coordinates and momenta which appear in the Hamiltonian. It may be that the behaviour of the system is rendered determinate by other variables not appearing in the Hamiltonian and not recognised by present-day physics. The possibility of such "hidden variables" is a highly contentious matter, but it is not relevant to our present purpose. For this, it is a matter of indifference whether quantum systems are or are not essentially non-deterministic.

An analogy may make clearer the account I am presenting of quantum interactions. A lightning flash in a thunder cloud has a well-defined system of tracks through the atmosphere. However, before the flash occurs the properties of the atmosphere in the immediate neighbourhood of the tracks may be substantially the same as those over the much larger region through which the flash could have passed as a result of the prevailing charge distribution. These charge distributions could be measured, by instruments in balloons, for example, and this would allow a prediction that a flash was imminent in some region. The exact path of the flash would not, however, be predicted.

Our thundercloud has another analogy to an electron as described by the quantum theory. Once a lightning flash has occurred in the cloud, a region around the flash is in a state which cannot produce another flash. Similarly if a system containing one electron produces a scintillation at a point on a screen then the system loses the ability to produce a scintillation anywhere else on the screen.

THE LOCALITY PROBLEM.

It is at this point that a simple propensity interpretation of the Schrödinger function raises the so-called locality problem. Starting from a state of a quantum system for which, to some approximation,values of coordinates and momenta are known, the time evolution represented by the Schrödinger equation may give rise to a state in which a particular coordinate has a finite probability of being exhibited in an interaction over an arbitrarily large spatial volume. For example, an electron in a radioactive nucleus capable of β-decay may may be known to be inside the nucleus at a particular time, but with passage of time it has a finite probability of detection at distances from the nucleus which become arbitrarily large.

Now in the propensity interpretation of the Schrödinger function, the coordinates of an electron in a quantum system need have no more locality than is represented by a probability distribution over space. The electron need then be no more than a picturesque representation of the inability of the field to manifest electron-type interactions at more than one place at a time in the region covered by the probability distribution. As it is sometimes put, the electron is called into being by the interaction itself. Between interactions, all we have is a kind of fuzziness like the chemist's representation of the 6 electrons in a benzene ring, the fuzz-density at any one point being given by the probability interpretation of the Schrödinger function.

The locality problem appears here in that an interaction at one point within the space in which the probability function has a finite value must immediately reduce the probability function to zero everywhere else in this space. Since the space can be arbitrarily large, this appears then to require the effects of the interaction to be transmitted through the region at speeds exceeding the limit imposed by the special theory of relativity.

Now I shall deal later with the question of supposed conflicts between quantum theory and the special theory of relativity, and all I wish to do here is to show that if such conflicts do not arise for other reasons, then the Kocken-Specker theorems do not necessitate them. As we have seen, these theorems throw doubt on the interpretation of quantum probabilities as frequencies of sharp values of variables in a population of similarly prepared quantum systems. We have also seen that the interpretation of quantum probabilities as propensities for a single quantum system involves difficulties with the special theory of relativity. However, these two kinds of probability are not mutually exclusive. Any one system in a population of similarly prepared quantum systems may not be able to exhibit the full range of coordinates and momenta exhibited by the complete population, but this does not mean that a single system must possess sharp values for its coordinates and momenta. To put the matter informally, the kind of fuzziness required to satisfy the Kocken-Specker theorems is far less than that given by the unrestricted time evolution of a non-stationary quantum system.

For simplicity, consider again the electron involved in a β-decay. (In order to avoid the rather questionable assumption that there are electrons inside an atomic nucleus, the latter can be replaced by a suitable spherical potential barrier). By the Kocken-Specker theorems, we may not envisage the electron at a particular time as possessing determinate sharp values of coordinates and momenta. However, it is entirely consistent with the Kocken-Specker theorems to suppose that at any one time the possible values of coordinates and momenta which could be exhibited by the electron are confined within much narrower limits than are given by a probability distribution derived from the relevant Schrödinger function. In particular, there is no bar to a supposition that at any one time the electron is either still in the nucleus, or it has escaped and will not return to the nucleus.

If the range of values of coordinates and momenta which can be exhibited by a single β-decay electron is less than that given by the statistical interpretation of the Schrödinger function, the extension of the range must be given by relating the Schrödinger function range, not to a single β-decay electron, but to a whole population. This population includes electrons still within the nucleus, whose number decreases with passage of time, and electrons which have escaped from the nucleus and are at varying distances from it.

Although the statistics of quantum systems depend upon two separate sets of distributions, arising from the indeterminate character of momenta and coordinates within a single system on the one hand and from differences between systems in an ensemble of similarly-prepared systems on the other hand, quantum theory gives no means of separating the effects of these two sources. It would be plausible to suggest that the indeterminacy within a single system is just that required, and no more than that required, to satisfy some form of the Heisenberg Uncertainty relations. This would ensure that the sharpness of definition of coordinates and momenta did not exceed that permitted by the quantum theory. At the same time it limits to a very small region the ability of one interaction to inhibit another, and avoids any conflict with the special theory of relativity. The probabilities for systems approximating to Schiff's "minimum wave packets", like the ground state of the hydrogen atom, would then be pure propensities.

However, the quantum theory (at least in the form with which we are here concerned) says nothing about such detailed matters, and for its practical applications there is no need to distinguish the relative contributions of frequency in a population and propensity within a system to the probabilities in the quantum theory. The importance of recognising the distinction between the two kinds of probability arises, not in the practical applications of quantum theory, but rather in the development of a realistic and non-subjectivist interpretation of the Schrödinger function.

THE SO-CALLED COLLAPSE OF THE SCHRODINGER FUNCTION AND THE SUPPOSED ROLE OF CONSCIOUSNESS.

In the above account, as in other accounts of quantum systems, the Schrödinger function varies continuously with time in accordance with the Schrödinger equation, but any interaction of a system exhibits one only of the range of possibilities of behaviour permitted for the system by the form of the Schrödinger function at the time of the interaction. In the course of any interaction therefore the form of the Schrödinger function changes from one admitting a relatively wider range of behaviour of the system to one permitting a relatively narrower range of behaviour. This is sometimes referred to as a "collapse of the Schrödinger function". (It is more usual to speak of the collapse of the wave function, but I prefer to avoid this term as inappropriate to a function dealing with probability distributions in populations of quantum systems).

In so far as our account of quantum systems makes the ranges of values of coordinates and momenta a property of a population of systems, each with more limited ranges of values open to it, the collapse appears here as a rather less dramatic event than it does in other treatments. Nevertheless, as we have seen, some degree of indeterminacy in values of coordinates and momenta, at least to the extent corresponding to the "minimum wave packet", must remain. The quantum theory does not reveal the mechanism by which the sharp values of coordinates and momenta exhibited in interactions are derived from the probabalistic range of values given by the Schrödinger function before the interaction. Nor does the quantum theory tell us whether the range of indeterminacy in the single system represents an essential limit on the scope of determinism in the world, or whether it is a consequence of the presence of hidden variables, not appearing in expressions for the Hamiltonians of quantum systems.

This need not be a matter for surprise. Scientific theories are a sophisticated class of representations of the world created by the minds of men, but like all such representations (including our concepts of every day objects like tables and chairs) they are incomplete, giving accounts of limited applicability to facets of reality. There is no reason to suppose the human race will ever achieve a "theory of everything", and certainly the quantum theory is not it.

Now of all scientists, physicists have always been the most prone to supposing that they were on the verge of discovering a theory of everything. In Victorian times, physicists of great distinction denied that the earth could have existed as long as the geologists claimed, because physicists knew of no mechanism which could have maintained the energy production of the sun over so long a period. At the turn of the century, just about the time Planck was applying quantum concepts for the first time to the energy distribution in black body radiation, and Einstein was developing the special theory of relativity, bright young university entrants were being advised to enter disciplines other than physics, where all the interesting discoveries were supposed to have been already made. In the 20th century, acceptance of Wegener's evidence for continental drift was delayed for 30 years because physicists could not account for the necessary movements in the earth's crust.

In the case of the quantum theory, the exclusion of gravity from its scope and its prima facie statistical character made it an exceedingly unpromising candidate as a theory of everything. Nevertheless, there has been a persistent school of thought in physics which has sought some form of universality for quantum theory. As Rae (1986) says, quantum theory "should be universally applicable. In particular, quantum physics should be able to explain the properties not only of atomic scale particles such as photons, but also of macroscopic objects such as billiard balls or motor cars or photon detectors" Similarly Squires (1986, p46) says of his colleagues and forerunners in physics that "we have claimed that quantum mechanics is a universal theory and applies to everything".Shimony(1989) too appears to deny all possibility of dissent in his bald assertion that "quantum mechanics is a framework theory which ,according to prevailing present opinion,applies to every kind of physical system."

Here then is a contradiction. Quantum theory is supposed to tell us everything about the behaviour of physical systems, yet it does not tell us how ranges of possible values for the variables of a system are reduced to a set of sharply defined values. Some degree of universality could have been saved (putting aside the exclusion of gravitation from the scope of quantum theory) by accepting a fundamental lack of determinacy in the physical world. The quantum theory does not predict a unique evolution for a quantum system because that evolution is not uniquely determined. To the extent that it is, quantum theory predicts it.

However, there appears to have been a reluctance on the part of many of those interested in this question to accept that the physical world could be anything other than strictly deterministic. There may have been less reluctance to admit a lack of determinism in mental phenomena, and this may account for the emergence of an idea that would otherwise have had little to recommend it. This was that the physical world, left to itself, was strictly determined, but that it interacted with mental systems which were not. While isolated from any mental system,the evolution of a quantum system is governed by the Schrödinger equation. Analogies readily present themselves. A thunder cloud builds up a potential distribution in a continuous fashion, but the onset and path of the lightning flash results from causes not involved in the charge separation processes, and in contrast to the laws governing these processes the flash has a random character.

Similarly, stresses build up in the earth's crust around the San Andreas fault in a continuous and predictable fashion, but the earthquake when it comes will appear as a random event.

The particular kind of mental process usually supposed to perform the role of precipitating discontinuities out of a continuously evolving and fuzzily defined physical world is consciousness. Why consciousness I do not know. It is an elusive concept : conscious processes readily and regularly merge into non-conscious processes. If I consciously draw a deep breath do I propagate non-unitary transformations in my surroundings, while leaving undisturbed unitary transformations if my breathing is left to its normal non-conscious regulation?

Even if this difficulty can be overcome, there remains the problem that an interaction of a system with a state of consciousness appears to require the "collapse of the wave function" to precede it. After all, when I become consciously aware of a scintillation on the screen, it already has a sharply defined position on the screen, and a selection has already been made from the probability range open to the quantum system. Does the physical system undergo a discontinuous transformation not because it interacts with a state of consciousness, but because it is about to do so?

Problems of this kind led most (and perhaps all) those who at one time or another entertained a role for consciousness to abandon it. I am happy to follow their example.

Indeed, a list of those quantum physicists who either never thought that consciousness had an essential place in the philosophy of quantum theory, or who considered the possibility and sooner or later rejected it, includes most of the best known names in the history of the subject,viz, Planck, Einstein, Schrödinger, de Broglie, and Bohr (I will justify my inclusion of Bohr in this list below). Heisenberg (1959,p54) also seemed to make his position clear "It (i.e. the collapse of the wave function) applies to the physical world, not to the psychical act of observation, and the transition from the "possible" to the "actual" takes place as soon as the interaction of the object with the measurement device, and thereby with the rest of the world, has come into play, and it is not connected with the act of registration of the result by the mind of the observer" (I would not of course follow Heisenberg in assigning any special significance to the "measurement device" and indeed I am not sure that Heisenberg himself intended it. He may have been following a common practice among quantum physicists of applying the term "measurement device" to any part of the rest of the world which happens to interact with the quantum system in which they are interested. However this may be, it does not weaken Heisenberg's rejection of a role for consciousness)

Heisenberg(1959,page 91) also dismisses the original reason for introducing consciousness when he writes that "the limitations of an axiomatic system will be found by experiment.....the limitations cannot be derived from the concepts."

Jordan placed the collapse of the wave function at an interaction with a macroscopic object (Jammer 1974,p488) and so denied any effect from an interaction with consciousness. The list could be continued, but perhaps the most interesting case is that of E.P.Wigner, who did so much to promote the idea that quantum theory involved essential reference to an observer. In the end, Wigner concluded that "the generally accepted formulation (i.e. that which postulated observer influences) can have only a limited validity".

THE LIMITED SCOPE OF QUANTUM THEORY ; EHRENFEST'S THEOREM AND THE HARMONIC OSCILLATOR.

It has been seen that the philosophical difficulties of quantum theory arise in part from the assumption that the theory is not limited in its application to certain basic field structures, but applies also to mechanical systems generally. In particular, the attempt to apply quantum mechanics to everyday objects leads to the measurement problems and the Schrödinger cat paradox. I will now argue that certain results from within quantum theory itself suggest that it cannot apply to macroscopic systems of the kind covered by classical mechanics. I will do this in two ways, one depending upon Ehrenfest's theorem, and the other upon Schrödinger's treatment of the harmonic oscillator.

Ehrenfest's theorem is proved in most text books on quantum mechanics (see Schiff 1986 p28) It deals with expectation values of the variables of a quantum system. The expectation value of a variable is the average value exhibited by a population of similarly prepared systems all represented by the same Schrödinger function U. In most text book treatments the reference is of course to average values measured rather than to those exhibited in more general interactions, but this does not affect my argument.

For the present purpose, we need expectation values of a coordinate x and its corresponding momentum p. These are written as <x> and <p> respectively and they are given by the equation below (Schiff 1986 pp27,28)

$$\langle x\rangle = \int U^{*} x\, U \, dxdydz$$

$$\langle p\rangle = i\hbar \int U^{*} \frac{\partial U}{\partial x} \, dxdydz$$

Here U is the Schrödinger function of the system, and the integrals are taken over all the space in which U is finite.

Now in general x and p cannot possess sharp unique values at a particular time t. However, this is not true of <x> and <p>. These do have sharp values which are functions of time only, and their dependence upon time is the content of Ehrenfest's theorem, deducible within quantum theory :

$$d\langle x\rangle/dt = \langle p\rangle/m \qquad (8)$$

$$d\langle p\rangle/dt = -\partial V/\partial x \qquad (9)$$

Now Ehrenfest's theorem in this form is the usual justification for claiming that classical mechanics, as applied for instance to planetary systems or billiard balls, is simply an approximation to quantum mechanics, applicable whenever the range of uncertainty of quantum variables is negligible compared with the inherent lack of precision in specifying the variables of the system in question. Equation (8) is then supposed to justify the usual classical relation between a component of linear momentum and a corresponding cartesian coordinate, and equation (9) to justify Newton's 2nd law.

It follows that the coordinates and momenta of a Newtonian system cannot be variables in a Schrödinger equation of a quantum system. Thus the solar system is not a quantum system, and it is not simply our inability to wait long enough that prevents our seeing billiard balls leaking through the potential barrier cushions onto the floor. Nor is it simply the fact that I am looking at it which stops Schrödinger's cat from remaining in a mysterious superposition of existence and disintegration. The dimensions and properties by which we specify and recognise planets and pets are not governed by quantum mechanics.

. Similar conclusions follow from attempts which have been made to represent the behaviour of the harmonic oscillator in classical mechanics as a limiting form of the behaviour of the quantum harmonic oscillator (Schiff, 1986 p74) Once again it is found that the position coordinate of the classical oscillator is related to an expectation value expression for a quantum oscillator, and that the classical oscillator cannot even in principle be seen as governed by Schrödinger's equation containing a classical coordinate. Instead, it appears as a Schrödinger function density distribution (or minimum wave packet to use Schiff's term) which oscillates without change of shape. The quantum fuzziness, or uncertainty in position and momentum, is contained within this oscillatory wave packet. The position expectation value averaged over this wave packet, which is the equivalent of the position coordinate for the classical oscillator, has a unique sharp value at any particular time, and oscillates round the origin with a determinate amplitude and period.

What it comes to is this. Quantum mechanics applies to fields (most commonly the electromagnetic field, but also to fields corresponding to the strong and weak forces of particle physics). These fields are governed by equations containing variables with an inherent indeterminateness. However the theory itself generates entities,corresponding to those in classical mechanics, which are characterised by variables (positions, momenta) which are entirely determinate. These entities (classical particles, planets, atoms, cats) are not susceptible to Schrödinger's detested "quantum jumps" and their states do not permit of superpositions. They are in fact persistent features of an underlying quantum world, retaining the properties by which we characterise them for a range of values of the underlying quantum variables, without partaking in the indeterminacy of the latter.

However, as Squires(1989) has pointed out,the distinction just made between Newtonian and quantum systems cannot be identified with that between macroscopic and microscopic systems. Thus the electron distribution for the ground state of a hydrogen atom has a determinate shape and sharply defined position, although the electron itself is indeterminate in position.The electrons in a crystal form a macroscopic quantum system..

It is nonetheless true that macroscopic objects of everyday experience are Newtonian entities. In such objects the minimal indeterminancy required by the Kocken,Specker and Gleason theorems is achieved at sub-molecular or intra-crystalline levels. The objects themselves are not in any sense superpositions of latent states, such as the live and dead states of Schrödinger's cat.

This account of Newtonian bodies is to be distinguished from treatments of macroscopic bodies such as those given, for example, by Prosperi (in Bastin 1971,p55)and by Cini (in Tarozzi and van der Merwe, 1985 p185). These authors wish to account for the properties of macroscopic bodies in terms of a quantum substratum, without any reference to consciousness or observers, but also without assuming any "collapse of the wave function". The Schrödinger equation is supposed to give a complete account of the time evolution of the microscopic substratum. Since this equation does not give unique sharp values to the microscopic variables, the problem then is to explain how dispersion-free macroscopic variables arise. It is also necessary to show how the time dependence of the macroscopic variables can be determined from their instantaneous values.

As in the account of the harmonic oscillator and the Ehrenfest particle given above, macrostates are presented as probability distributions over microstates. It is then sought to demonstrate that a large class of microstates are macroscopically indistinguishable and can be related to a single macrostate. This no doubt can be done, but it is by no means established that this single macrostate embraces all the microstates allowed in the probability distribution defined by the Schrödinger equation. Indeed, if we demand that the microstates be based upon entities having the minimal requirements for particle states already discussed, then the Kocken, Specker and Gleason theorems appear to require the exclusion of some of the microstates.

Bub (Bastin,1971,p65) is among those who have argued that some exclusion of microstates permitted by the Schrödinger equation must be accepted if macroscopic bodies are to be associated in this way with a quantum microstratum. However, I have no problem in accepting this form of "collapse of the wave function", seeing it merely as a continuous process by which the range of probabilities exhibited by a system is reduced from that permitted by the Schrödinger function. The full range of possibilities is exhibited, not by a single system, but by a population of similarly prepared systems.

It appears to me, therefore, that Bub's objection does not apply to the account I have given of Newtonian bodies.

TWO KINDS OF REDUCTIONISM

A comment by Squires (1989) draws attention to the need to distinguish two different ways in which we can speak of reducing one theory to another more fundamental theory. One of these takes the comparatively simple form of replacing approximate relationships between the variables describing a particular system, identifiable in both theories, by more accurate relationships. Thus both geometric optics and wave optics deal with distribution of light intensity in optical systems, but wave optics describe them more precisely, revealing small scale details not apparent in geometric optics.

In a more interesting kind of reduction, it is not just a question of replacing approximations by closer approximations. Rather the entities featuring in the reduced theory are interpreted in terms of patterns of behaviour or structures in the more fundamental theory. Thus in reducing thermodynamics to statistical mechanics, properties of single systems in the former theory, such as entropy or the Gibbs function, do not appear as properties of single systems in the latter theory, but arise from a consideration of population of systems.

The reduction of Newtonian mechanics to quantum mechanics falls into the second class. The formalism of Newtonian mechanics is not an approximation to that of quantum mechanics. Rather it applies to entities to which quantum mechanics does not apply. Nevertheless as Ehrenfest's theorem shows, Newtonian behaviour can be interpreted in terms of quantum behaviour at a different level of structure.

ELECTRON DIFFRACTION BY A PAIR OF SLITS.

The diffraction pattern produced upon a photographic plate by a stream of electrons passing through a pair of slits is quantitatively predicted by quantum theory, but the physical process underlying it has always been a source of puzzlement. The electrons behave as particles in that each electron can be shown to arrive at a sharply defined position on the photographic plate. This is shown most clearly if the plate is replaced by a suitable fluorescent screen. Furthermore, the diffraction pattern is exhibited even if the electrons are so widely spaced that only one electron is present in the apparatus at any one time. The pattern would therefore appear to be produced by particles following defined trajectories and not interacting with one another.

On the other hand, the pattern is quite unlike the superposition of the pair of patterns obtained by electrons from a similar stream passing through one slit at a time, with the other slit closed. In this respect, the stream of electrons behaves not as a stream of particles, but as a wave phenomenon propagated with a wavelength L related to the magnitude of the momentum p derived from the particle interpretation by the equation

$$L = h/p$$

where h is Planck's constant.

The behaviour of this particular quantum system was interpreted at a very early stage in the development of the quantum theory by de Broglie. He regarded the region of the electron stream as the path of propagation of a pilot wave which controlled the trajectories of the electrons. The pilot wave reacted to both slits, and in particular its field function intensity distribution at the photographic plate with both slits open was not expected to be the sum of the intensity distributions produced by each slit singly. The field intensity distribution controlled the arrival pattern of the electrons at the photographic plate, and this explained why the arrival pattern was not the sum of the single slit pattern.

In this account, the electrons, not the waves, were the carriers of energy and momentum, so explaining the temporally and spatially localised arrivals at the photographic plate. Clearly the account I have given of particle phenomena in quantum systems has a conceptual similarity to de Broglie's. It is true that I find it more helpful to see the particles as manifestations of localised energy and momentum exchanges of the field itself. In this sense, the waves do carry energy and momentum but exchange them in a less uniform fashion than the waves in Maxwell's electrodynamics.

As I have already said, this picture accords better with my view of quantum theory, at least in its earlier stages, as an account of the small scale behaviour of the electromagnetic field. It also avoids the difficulties of those who tried to identify the field function generating de Broglie's pilot wave with the Schrödinger function. This identification is of course inconsistent with the ascription of a statistical character to the Schrödinger function and with seeing it as a property of a population of similarly prepared systems, rather than as a property of a single system. However, a more important cause for the relative neglect of the pilot wave picture was the impossibility of representing the Schrödinger function in a 3-dimensional space except in the case of systems containing a single particle only.

In due course, the pilot wave theory was disentangled from this mistaken interpretation of the Schrödinger function, and I suggest that it has always played a greater part in the mental pictures associated with quantum problems by working physicists than the concurrent philosophical discussions would lead one to believe. This is illustrated by the discussions which arose from Renninger's papers on wave-particle dualism (Renninger,1953,p1960)

Renninger noted that the reduction of a wave function associated with the emission of a particle from the centre of a sphere would result not only from the detection of a particle in one hemisphere, but also from the failure to detect the particle in the other hemisphere. Since it was difficult to believe that the occurrence of nothing at all could have a real physical effect over a region of space, it appeared that either the reduction of the wave function had already happened and that the non-detecting hemisphere played no part in the process, or else that the non-detecting hemisphere did play a part in the process as a boundary associated with a field process connected with the particle emission. The former view would be inconsistent with the known effect of boundaries upon the particle-emission processes (as in the electron diffraction case, where the second slit prevents electrons arriving on the photographic plate at points which could be reached through one slit on its own).

Born and Einstein were among those to see the explanation of Renninger's "negative result measurements" in some form of pilot wave theory (Jammer,1974 p493). As Born said "....both particles and waves have some sort of reality but waves are not carriers of energy or momentum".

LANDÉ AND GROUP PROPERTIES OF ELECTRONS.

Another well estabished school of thought in quantum theory which has much in common with the interpretation I am advocating is that expounded by Landé in his well-known text book (Landé 1965). This ascribes properties to groups of particles as a whole, like the stream of electrons in double slit diffraction, and on this basis develops all the usual results of quantum theory. Stopes-Roe (1962) and others have pointed out that it is then difficult to avoid associating the particles with a field, since in electron diffraction for instance each electron would appear to react to the screen as a whole.

Papers relevant to a field interpretation of quantum theory have continued to appear, among them Shimony (1966), Born and Biem (1968), Pearle (1967 & 1968), Bloch(1968), and Krips (1987). (Perhaps it should be made clear that we are concerned here with field interpretations of the quantum theory of systems of particles,not with the quantum theory of the electromagnetic field itself. I do in fact regard the former as reducible to the latter, but this I think can be done satisfactorily only if we dispose of the idea that there are such things as quantum systems of autonomous particles).

STERN-GERLACH PROCESSES AND THE PROBLEM OF COMPOUND PARTICLES.

I have pointed out the difficulties in supposing that the quantum theory describes the behaviour of particles, unless the particles are entities embedded in fields. These difficulties remain whether the Schrödinger function is regarded as a property of a single system or as a property of an ensemble of similarly prepared systems.

Thus consider the beam of electrons producing a diffraction pattern by passing through parallel slits. If the intensity of the beam is reduced so that only one electron is likely to be present in the apparatus at any one time, we have an ensemble of systems made up of the succession of passages of one electron through one or other slit. A diffraction pattern is still produced, which is quite different from the pattern which would have been obtained by superposing the patterns formed by two beams of electrons each passing through a single slit.

Now, as we have seen, this is not a problem if a single electron is embedded in and influenced by a field reacting to the presence of both slits. The field microstructure gives an array of possible trajectories, one of which is taken by each electron as it passes through the system.

To the rather limited extent that it is valid to envisage the photon as a particle, a similar account may be given of the diffraction pattern produced on a screen by the passage of light through a pair of parallel slits. The field pattern here is that derived from ordinary wave optics, without any reference to photons. Photon trajectories are the normals to wave fronts. Here it is less of a surprise that the diffraction pattern remains if the light intensity is so far reduced that only one photon is present in the field pattern at any one time, and that, as Dirac remarked of electrons, every photon interferes with itself. It is far less easy to think of a photon as divorced from a field than it is to do so for an electron, although I have argued that even in classical physics this is impossible for an electron obeying Newtonian mechanics.

By seeing quantum systems always as field systems, and never as aggregates of autonomous particles, we avoid one problem that faced the school of realists who regarded quantum theory as a statistical theory, in the frequency sense, of ensembles of systems. If the Schrödinger function does no more than present the relative frequencies of occupation of an array of trajectories, then it would be very odd if a particle, having followed one particular trajectory, could then be made to interact with the other trajectories it might have followed.

That this happens is demonstrated very simply by considering the photon as a particle. If a light beam of sufficiently low intensity is divided into two by a half-silvered mirror, then only one of these contains a photon and the other is empty. Yet by recombining the two half beams interference patterns can be produced. Then we have the odd circumstance that the photon interacts with what it might have done but did not.

Now perhaps nobody is worried by the need to accept the existence of a real field controlling all photon phenomena. More surprising is the fact that the quantum formalism suggests that such interference between what happens and what might have happened is possible in all kinds of quantum systems. If it is only the small magnitude of Planck's constant which prevents us seeing quantum effects in macroscopic bodies as everyday events, then in an eternal universe a train running south from Doncaster through Grantham will some day collide at Peterborough with itself arriving through Lincoln.

I have already argued that railway trains are not quantum objects, even in events occurring at arbitrarily low frequencies. They obey Newtonian mechanics, not a form of quantum mechanics which happens to approximate closely to Newtonian mechanics except on rather rare occasions. There are, however, entities intermediate in size and complexity between photons and railway trains for which the matter is not so straightforward. I have already referred to α-particles, which we appear to regard as quantum entities in the treatment of α-decay, but for which we would not wish to postulate some special α-particle field in which to embed it. Would every possible atomic nucleus have to be given its own field ? Then what about molecules ? And do we not apply quantum theory to crystal lattices ?

It is time therefore to look at the apparent quantum behaviour of an entity which it would be implausible to associate with its own special field. A convenient example is the silver atom in Stern-Gerlach processes.

The Stern-Gerlach effect depends upon the possession by the electron of a small magnetic moment. This is not measurable or detectable for the free electron owing to the indeterminacy features of quantum entities (Mott and Massey, 1933, p42), but it has important implications for bound systems of electrons, such as atoms and molecules. In these there may also be a magnetic moment contribution from the spatial motions of the electrons as well as from the internal spin which may be regarded as the source of the inherent magnetic moment of the electron. The magnetic moment of an atom has the discrete behaviour characteristic of many quantum phenomena in that the magnetic axis can assume only a finite number of inclinations relative to an external magnetic field.

In a Stern-Gerlach process, the pole pieces of a powerful magnet are shaped to produce a magnetic field varying rapidly in space between them. This space variation causes a force on a magnetic moment, and hence the path of an atom with a magnetic moment is deflected as it passes between the pole pieces. Each permitted inclination between the atom's magnetic axis and the external field produces a separate deflected path which may be detected upon a photographic plate.

We now confine our attention to the silver atom, which exhibits a magnetic moment corresponding to the internal spin of a single electron. This can take up two orientations in the Stern-Gerlach apparatus, so that a beam of silver atoms produces two spots on the photographic plate. For a silver atom in the entrant beam, the quantum theory does not give a definite orientation for the magnetic axis but only a quantified probability for exhibiting each of the two possible orientations in passing between the pole pieces.

Each of the two beams leaving the pole pieces has a determinate orientation relative to an axis defined by the pole pieces. For a beam of low intensity, the single silver atom in the pair of deflected beams at any one time has a determinate probability of detection in each beam. The detection of a silver atom in one beam, for instance by a second Stern-Gerlach apparatus, would reveal different properties from those of a silver atom in the other beam.

Putting aside the practical difficulties of actually doing it, let us suppose with Krips (1987,p79 onwards) that the two beams emerging from the Stern-Gerlach apparatus are recombined by passing each through a magnetic field antiparallel to the first and equal to it in magnitude and space variation. Krips claims that the recombined beam would then be identical with the original beam, defined by a so-called "pure" state function. This would be experimentally distinguishable from a beam consisting of a statistical mixture of atoms with definite orientations with respect to the relevant external magnetic fields.

I have some misgivings about Krips´ analysis of an experiment which, as far as I can find, has never been carried out. However, I do not dispute that experiments could be devised which would indicate the real existence of both beams when only one silver atom is present at any one time in a Stern-Gerlach process.

Now like Krips, and for that matter like nearly all physicists, I regard each silver atom as occupying one beam only and not as having a ghost atom corresponding to it in the other beam. Nor of course do I regard it as in some state of suspended potentiality in both beams, waiting for an observation to consign it irreversibly to one beam or the other. Am I then forced to postulate a silver atom field filling all space, in the same way that an electron field accounts for the influence of both slits when each electron passes through one only ? And does this lead on to the implausible supposition of an indefinitely large number of overlapping fields, one for each entity (atom, molecule, etc.) showing recognisable quantum behaviour ?

Emphatically no! I see quantum theory as developing primarily as an account of the microstructure of the electromagnetic field. It may well be adaptable without much modification to systems involving the weak and strong fundamental forces. This may involve the recognition of baryons, for example, as features of a baryon field in the same way as electrons and photons are features of a universal electromagnetic field. Such matters are outside my scope.

With silver atoms, the case is different. Silver atoms react to the universal electromagnetic field entirely in virtue of the electrical charge motions embodied in them. It is the latter which correspond to the microstructure of the electromagnetic field embracing both beams in the Stern-Gerlach apparatus. There is no place for a separate field to account for the mechanical behaviour of silver atoms.

In this limited sense, silver atoms, α-particles and the like can be regarded as quantum entities in a way that cats, billiard balls and planets cannot. No problem appears to arise in deciding where quantum mechanics ends and Newtonian mechanics begins. The empty beam in a Stern-Gerlach process does not carry a ghost atom : it is an electromagnetic field structure which can interact, in ways predicted by quantum theory, with the bound electromagnetic field structure which constitutes the orbital electrons in a silver atom. All the evidence suggests that this is how the practising physicist sees the matter.

THE COPENHAGEN INTERPRETATION OF QUANTUM THEORY

I have argued that quantum theory can be freed from its subjectivist elements without altering its essential structure or limiting its practical use, although I have had to abandon the supposition that it applies in principle to all systems.Bodies described by macroscopic coordinates, for example,are not quantum entities.Clearly nothing is lost by such abandonment,since quantum theory is not used in practice for such systems.I have further argued that the resulting interpretation of quantum theory solves many of the puzzles that have been associated with the theory.

I now wish to show that the interpretation I have given is broadly that held by physicists using quantum theory in its wide range of applications in particle scattering, atomic and molecular structure, spectra, solid state physics and so forth. Scientific theories (like mental concepts generally) have successful application because of a certain isomorphism between aspects of their structure and aspects of a world existing independently of mental processes. Such a view loses much of its significance if in fact users of these theories have widely different conceptions of the systems and processes with which they are working, or if in fact there are practising positivists getting by with patterns of recurrences in raw sense data. This is a very contentious matter. To what extent do text books and scientific journals exhibit a basic common conceptual structure in any particular branch of science? Practical navigators and calendar makers got on very well with geocentric concepts for millenia. Has not the quantum theory throughout its history been littered with competing interpretations, from Heisenberg's "potentia" to Maxwell's "propensitons" and from "wavicles" to "many-worlds"?

Yes it has,but I suggest that interpretations differing widely from mine are not present in the minds of practising quantum physicists.Does the chemist visualise the benzene molecule multiplying into disparate universes from nanosecond to nanosecond? Does the crystallographer think the spots form on his photographic plate as he looks at it? It is sufficient therefore for me to show that the interpretation of quantum theory which I am presenting has much in common with broad streams of thought among practitioners of quantum mechanics over a large part of its history, and that it is not a collection of speculations,irrelevant to the application of the subject, cobbled together to buttress a metaphysical doctrine.

I have already shown the connections of my interpretation with the idea of carrier waves developed by de Broglie and his successors, and also with the idea of group properties for systems of particles, as developed by Landé and others. It may be argued that although these ideas have had considerable weight of authority behind them over some 50 years, they have always represented something of a minority school of thought. Surely it may be said, the mainstream orthodoxy has been the so-called Copenhagen interpretation, associated with Bohr and Heisenberg and their successors. Is it not commonly accepted that the subjectivist trends in the philosophy of quantum theory have their origin in this Copenhagen interpretation?

Yes this is commonly accepted, but, in my opinion, wrongly so. As presented by Bohr, the Copenhagen interpretation had no subjectivist tendency, nor was such a tendency countenanced by Heisenberg, at least in his later contributions to the subject. Nor was Bohr a positivist, holding as he did that science was about what we can say about the world, not about what we can say about sense data.

We can see this best by looking at the problem faced by Bohr as early as 1913, when he had accounted for the energy levels of the hydrogen atom, as exhibited by the Bulmer, Paschen and Lyman spectral line series,by the use of Planck's 1900 relationship between energy and frequency.

$$E = h\nu$$

where ν is the frequency of the radiation exchanging energy in discrete quantities E. To do this Bohr had placed electrons, seen as charged particles, with potential energies and momenta given by classical mechanics and classical electrostatics, in orbits around a nucleus. He made the bold assumption that a selection of such orbits, having discrete values for their angular momenta, would be stable, remaining unchanged between emission and absorption of radiation. In so doing, he implicitly denied the status of elctrons as particles obeying classical mechanics and classical electromagnetics, since such particles in his orbits would have radiated energy continuously. In other words his successful use of classical concepts had produced a picture of the hydrogen atom which denied the validity of these concepts.

This contradiction continued in later developments of the quantum theory, culminating in Schrödinger's equation. To apply the latter to any particular system, a Hamiltonian function is derived on the assumption that the system consists of charged particles with potential energies, positions and momenta with sharp determinate values given by classical mechanics and classical electromagnetics. The solutions to the Schrödinger equation however deny the existence of such particles, as we have seen above.

Bohr's response to this problem made no concessions whatever to subjectivism or positivism. For him, quantum systems were not ghostly arrays of potentialities brought into sharpness of definition only by the activities of conscious minds in observing them. Nor can I accept d'Espagnat's view that Bohr's interpretation included essential reference to measuring instruments (d'Espagnat, 1983). As we shall see presently, Bohr often stressed the relevance to a quantum process of the "entire measurement situation" or of the "total experimental arrangement". However, it seems clear that in using such terms, Bohr was referring to the total environment of the quantum process, which could of course include the screens and apertures in electron diffraction for example, but was not confined to measuring equipment, and in the general case there need be no such equipment present at all. In support of this view, one need go no further than Bohr's original paper on the energy levels of the hydrogen atom. These energy levels are stationary states, and no measurement processes are involved in Bohr's analysis of them. The emission and absorption processes by which the energy levels change are not supposed to depend upon any instrument or observer-related interaction.

What Bohr did in fact was to accept the independent reality of the entities such as electrons and electromagnetic fields which occurred in quantum theory and also the failure of classical mechanics and classical electromagnetics to account for the totality of their behaviour. However, "no content can be grasped without a formal frame", and this formal frame must be described by "our customary points of view and forms of perception" (Bohr,1958). For Bohr, these were the concepts of classical physics. The limits of the applicability of these concepts were to be found by experience, and indeed these limits were for the most part derivable from quantum theory. Outside these limits, the classical concepts fail us, but they are sufficient for the purposes of defining the relevant boundary conditions of the quantum process and for interpreting the interactions of the process with its environment. It is of course customary to paraphrase what I have just said by referring to the "experimental set-up" rather than to "boundary conditions", and to "measurements" rather thanto "interactions with the environment". My phraseology is,however, quite consistent with Bohr's position, and with the readiness of the practising physicist to identify any interaction producing a macroscopic trace as a measurement.

For Bohr,then,electrons are not classical charged particles with sharply defined coordinates and momenta,nor are groups of electrons classical fields exchanging energy and momentum in processes continuous in space and time. However, the classical concepts of particle and field are all we have for the purpose, and experience and the quantum theory itself are our guides in choosing the appropriate concept for the particular situation. Bohr used the term "complementarity" to describe the relationship between a pair of classical concepts both of which are necessary for the description of a quantum process but neither of which is sufficient for the description. However, the term has been used by Bohr and by others for so many other things that it is best avoided.

Bohr resolutely refused to speculate on ways of modifying classical concepts so as to remove the incompatibilities. He and most practising physicists have appeared to claim that they have no need to do so, following Wittgenstein's dictum that "what can be said can be said clearly, and what cannot be said must be passed over in silence". There are two things to be said here. First, if physicists deny the need for, and the possibility of, a self-consistent account of quantum entities in the practice of physics,they are not entitled to give highly coloured accounts of such entities to learned symposia.Secondly,textbooks and journals do suggest that physicists make more use of mental pictures to guide the application of quantum formalism than the positivistic tendencies of many of them would imply.These pictures appear to be in accord with the account of quantum theory I am presenting here.

Bohr himself consistently maintained that "quantum attributes reside in the entire measurement situation". Surely here we have a recognition that quantum processes are not concerned primarily with trajectories of particles but with boundaries of fields. He also saw the inability of quantum systems to exhibit simultaneous sharp values for non-commuting variables as connected with "the indivisibility of the total experimental arrangement" and "the mutual incompatibility of two such arrangements" (quoted in Bastin (1971 p.2). Surely what Bohr has in mind here is that the system boundaries determine what kind of variable can be exhibited by a particle (e.g. a sharply defined position in one set-up and a sharply defined momentum in another) and that we must not manufacture paradoxes by supposing that we could have chosen some quite different variable to measure.

I will return to this last matter below in discussing Einstein-Podolsky-Rosen phenomena. Bohr's account of them appears to me to remove much of the mystifications associated with them, particularly if we see it as a field account.

One other aspect of the Copenhagen interpretation appears to me much more questionable. This is the apparent supposition that the range of representations of the physical world available in mental processes is fixed, and does not change as the range of human experience widens.

Thus Heisenberg (1959,p46) writes "The concepts of classical physics form the language by which we describe the arrangements of our experiments and state the results. We cannot and should not replace these concepts by any others. Still the application of these concepts is limited by the relations of uncertainty. We must keep in mind this limited range of applicability of the classical concepts while using them, but we cannot and should not try to improve them".

The wording used by Heisenberg is interesting. If one cannot improve these concepts, why is it necessary to add that one should not try? Such a ban would have prevented the development of science as it is today. 18th century technology got quite a long way in heat engine practice, and vastly extended the scope of mining operations as a result, by regarding heat as a fluid, caloric, flowing down a temperature gradient. Carnot even got as far as formulating a version of the 2nd Law of Thermodynamics (well before the 1st Law was formulated, oddly enough) in the caloric theory. Newcomen and Carnot did very well with the only concepts available to them in their time, but later developments would have been seriously hindered if their successors had not improved these concepts.

Mankind has extended and improved its conceptual representations of reality over millions of years and will continue to do so. Concepts employed in quantum theory are no exception, and the field interpretations discussed in this paper show one direction in which physicists have extended and refined their mental models of quantum entities.

SEPARABILITY IN QUANTUM SYSTEMS

An interesting feature of quantum mechanics is its apparent failure to allow for the separation of a system into two non-interacting sub-systems. However far apart they become, over however long a period, interactions of one appear to affect the behaviour of the other. It is not that these long distance effects can be directly identified, and still less that they can be used to exert a long distance control, variable at will, from one place to another. It is rather as if the range of behaviour open to one sub-system, statistically assessed, is altered by an event involving the other sub-system.

The earliest systematic study of this phenomenon was made by Schrödinger (1935). He treated the problem in its broad generality, but here a quite simple example is sufficient. Consider a system with two degrees of freedom only, and suppose that on separation each sub-system can be completely characterised by one coordinate : x_1 for sub-system I and x_2 for sub-system II. The combined system is characterised by the pair $(x_1\ x_2)$, and its Schrödinger function can be written $U(x_1\ x_2)$.

Since the two sub-systems can be identified separately, there are dynamical variables which can be specified for each sub-system, x_1 & x_2 themselves for example, or momenta or energies. The operators corresponding to these dynamical variables define eigenfunctions and eigenvalues in the way already discussed. The eigenfunctions of x_1 are a complete orthogonal set, and can be normalised, and similarly the eigenfunctions in x_2. The Schrödinger function U $(x_1\ x_2)$ can therefore be written as a series in the two sets of eigenfunctions (Courant & Hilbert 1953, vol 1, p56)

$$U(x_1\ x_2) = \sum_n a_n v_n(x_1) w_n(x_2)$$

Now suppose sub-system II is involved in an event in which the dynamical variable corresponding to the set of eigenfunctions Wn exhibits a value W_k (which must of course be an eigenvalue corresponding to one of the w_n say w_k). By this event the Schrödinger function of the combined system becomes

$$U(x_1\ x_2) = a_k v_k(x_1) w_k(x_2)$$

Thus, as a result of the event involving sub-system II, the part of the Schrödinger function referring to sub-system I has altered, and with it the statistically determined ranges of values of variables capable of being exhibited by sub-system I.

Note that the event involving sub-system II does not introduce any new possible values for the relevant variables of sub-system I. These remain restricted to the eigenvalues corresponding to the eigenfunction v_n . However, their relative probabilities of occurrence in an appropriate interaction does alter, in accordance with the Born statistical interpretation of the Schrödinger function.

It is thus confirmed that no measurement on sub-system I will reveal the event in sub-system II, since the result of the single measurement is a possible outcome whether the event in sub-system II occurs or not. Nor can one detect the altered probability range by repeating the measurement on sub-system I since the first measurement alters the Schrödinger function to an eigenfunction corresponding to the measured variable even if the event in sub-system II does not happen. (Jammer, 1974,p212)

The interaction we have here therefore is not observable, nor is it needed to explain what actually happens, but rather an interaction inferred in order to explain what would have happened in circumstances which do not in fact occur. In traditional logic, we might say we are concerned with the validity of propositions depending upon unfulfilled conditionals. It is not surprising that we may be tempted to wonder what all the fuss is about.

Now clearly the fuss is not about any problem in applying the calculus of quantum theory to the kind of situation we have been discussing. Nor do any new logical inconsistencies appear of the kind we may be inclined to see when we construct a Hamiltonian for a system which turns out not to contain the entities assumed in the construction. There appears to be no difficulty in constructing systems of quantum logic which embrace the calculus employed. (Van Frassen and Hooker in Harper & Hooker, 1976 p221).

Is this a problem then with no significance in the real world, one that can be solved by a dismissive wave of the hand? Jeffrey Bub (1974) argues this view very persuasively. All we need to note (Bub 1974 p83) is that quantum statistical events cannot be fitted into a classical statistical treatment, nor their measures mapped on a classical probability space. More particularly (Bub 1974, p148) we have made the mistake of ascribing to quantum systems sets of properties forming a Boolean algebra.

Bub's approach requires no reference to conscious entities, measuring instruments with effects different from other environmental factors, or causal influences moving at superluminal velocities. It is also difficult to disagree with anything he says. Perhaps the remaining niggling doubt is over what he leaves unsaid. What kind of things are those that do not possess properties forming a Boolean algebra? If events in systems of such things are not susceptible to classical statistical treatment, what basic feature of these things differentiates them from things which can? By leaving a large area of the map blank, Bub is inviting others to write there "here be monsters".

It is worthwhile, therefore, to give some explanation of quantum non-separability, based upon the ideas already put forward here. Though obviously not required for the successful use of quantum theory, and adding nothing to its scope, it will at least show that non-subjectivist explanations are possible.

EINSTEIN-PODOLSKY-ROSEN PHENOMENON

A particular illustrationof a system showing this non-separability is that described in a paper by Einstein, Podolsky and Rosen (1935). It consists of a pair of particles with coordinates measured along a particular direction x_1 and x_2 ,and corresponding momenta p_1 and p_2. Although the quantum theory cannot determine simultaneous sharply defined values for x_1 and p_1 , and for x_2 and p_2, respectively it can determine simultaneous sharp values for the quantities x_1-x_2 and p_1+p_2 , since it turns out that they share a set of eigenfunctions.

Now consider two possible measurements which could be made on such a system. First x_1 could be measured, giving a sharply defined value for this variable. Since x_1-x_2 is sharply defined, the measurement also gives a sharply defined value for x_2. Alternatively, p_1 could be measured, giving by the same argument a sharply defined value for p_2.

Einstein, Podolsky and Rosen made the assumption, which was in due course challenged, that since the separation of the two particles could become arbitrarily large, the measurement of x_1 could not be the cause of the aquisition of a sharp value by x_2, similarly for p_1 and p_2. Therefore, they argued, x_1 and x_2, p_1 and p_2 must all have had sharp values in the system before any measurements were made. Since quantum theory procedures cannot produce sharp values for such a set of variables, it was further argued that quantum theory must be incomplete.

The purpose of the three authors was to show how a form of determinism could be retained for systems of particles with simultaneous sharply defined coordinates and momenta, despite the admitted practical success of a quantum formalism which could not predict them. Quantum theory was correct in the predictions which it made, but there existed real physical values for variables of quantum systems which it did not predict uniquely but only in statistical ranges.

Now this study is not concerned with determinism. Clearly we live in a world in which some events are connected by recognisable causal relations with some other events. Clearly too, we cannot identify a sufficient range of such causal relations to account for all events in any arbitrary volume of space in a particular period of time from a knowledge of earlier events in some region enclosing this volume. Whether this limited scope for causality is a result of human ignorance, or whether it is a fundamental feature of the universe, is a question which has always interested philosophers and it may be that the history of the human race will end with the question still unanswered.

However this may be, it soon became clear that the paper by Einstein, Podolsky and Rosen was not going to change many minds on the subject. Although it seems probable that Einstein at least saw the paper as a contribution to the defence of determinism, its main thrust was to establish the lack of completeness of quantum theory, and much of the subsequent comment was concerned with showing that this had not been done.

It should be emphasised at this point that the kind of incompleteness in question here is not the same as that I see in the quantum theory. I have argued earlier that the theory is incomplete in the sense that it applies only to electromagnetic systems and perhaps, in rather modified form, to systems involving the weak and strong forces. It does not apply to macroscopic bodies or to gravitational systems. I do not suggest, as Einstein, Podolsky and Rosen did, that it is incomplete because it gives only statistical predictions of momenta and coordinates for particles which do in fact possess determinate, sharp values for them. I hold rather that the entities to which quantum theory applies do not possess simultaneous sharp values for two dynamical variables unless the operators corresponding to them have a common set of eigenfunctions.

I can therefore consistently support what became, I think, the majority view, that Bohr (1935) successfully rebutted the arguments given by Einstein and his co-workers on this matter. Bohr denied assumption (1) above that the experimenter has the choice of measuring either the coordinate or the corresponding momentum of a particle for a particular quantum system in a particular state, as defined by the relevant Schrödinger function. Bohr claimed that reference to the position of a quantum particle presupposed reference to a specified physical arrangement, and if this arrangement allowed a precise value for a position coordinate it could not give a precise value for the corresponding momentum. Bohr illustrated this by supposing that the value of x_1-x_2 was established by apertures in a plane perpendicular to the x-direction. The value of p_1+p_2 could then be established from the total momenta of the two particles approaching the screen less the momentum transferred to the screen. The momenta of the particles approaching the screen could be made determinate by selecting them, again by the two apertures, from a stream of particles with undetermined distribution along the x-direction.

Bohr then showed that a screen and aperture system could not be arranged so as to select values for both x_1-x_2 and p_1+p_2 . To select a value for x_1-x_2 ,the screen must be prevented from moving in the x-direction, whereas to determine the momentum in the x-direction transferred to the screen the latter must be capable of movement in the x-direction, since a fixed screen is one which absorbs momentum without measurable response.

Admittedly many doubts were raised about Bohr's analysis. There is certainly something unconvincing about the idea of measuring the momenta of electrons, say, by their effect upon a screen mounted upon springs. Arguments from imagined experiments have a doubtful logical status anyway, and perhaps they should not be based upon procedures too far removed from practical execution. Even if one puts this on one side, on the grounds that the original paper had not put forward any more practical realization of the experiments, there remain doubts about the supposition that a pair of apertures would produce a pair of quantum particles which would remain at a determinate distance apart for a time long enough for the proposed measurements to be made. Indeed, in a comment on the original paper, Epstein (Jammer 1974 p230) had claimed that the time dependence of the dynamical variables of the two particles should have been taken into account.

Increasingly, discussion came to emphasise Einstein's assumption that a measurement on one particle could not have any effect upon another particle at an arbitrarily large distance from it. If this assumption cannot be sustained, the argument for simultaneous sharp values of x_2 and p_2 prior to any measurement no longer holds, since either of them separately could have been produced by measurements of x_1 or p_1 respectively.

By the time therefore that a more practically realizable version of the Einstein, Podolsky and Rosen experiment was put forward by Bohm (1951), the problem had become the apparent need for quantum causal effects to be transmitted at speeds greater than that of light. Before discussing the Bohm variant and its several practical realizations we must therefore see what relation the problem of superluminal velocities has to our main concern, the implications of quantum theory for the nature of mental entities.

ACTION AT A DISTANCE AND REDUCTIONISM

Stated baldly, there appears no reason at all to believe that a physical world existing independently of and prior to mental activity would necessarily exclude action at a distance. After all, Newtonian gravitation, which was regarded as a basic feature of the physical world for some 200 years,has forces transmitting effects across space instantaneously. No-one, as far as I know, saw this as justifying subjectivist acounts of the physical world. Indeed, the contrary view that it supports a materialist view has been more usual.

Nevertheless, it is undeniable that the supposed need for action at a distance in quantum systems has commonly been thought to support subjectivism. Two quite recent examples will be sufficient, one of a physicist who welcomes such support, and the other of a physicist who wishes to rid quantum theory of action at a distance because of it.

Mermin (1958) gives a popular account of the experiments of Aspect and his co-workers, which I consider below.As far as I can see,he deduces from them nothing of philosophical relevance except that they show action at a distance. However, this is sufficient for Mermin to entitle his paper "Is the Moon there when nobody looks? Reality and the Quantum Theory" In case the question mark in this title leads anyone to think that Mermin had doubts about the connection between action at a distance and subjectivism there is also the sub-heading "Quantum Mechanics is Magic", with no question mark. I think therfore we do no injustice to Mermin in ascribing to him the view that action at a distance, if it exists, implies that mental entities either create, or can vary arbitrarily, the behaviour of physical entities.

Now it may well be that believers in magic find a world that permits action at a distance a more congenial place than one that does not. However, I think that nothing Mermin says supports a converse view that action at a distance makes a belief in magic more plausible. His arguments are not therefore a serious challenge to anyone who starts out from the belief in the objectivity and priority of physical entities vis a vis mental entities.

My second example presents much more serious problems. I have already referred to the work of physicists such as Marshall who wish to deduce much or all of quantum behaviour from minimalist adaptation of classical electromagnetism combined with the postulate of a stochastic vacuum field. Their work would, I think, embrace the objective I have here of ridding quantum theory of reference to mental entities, but would of course do this only as part of some much larger enterprise which may or may not turn out to be successful. I do not think we need wait for this outcome to decide whether physics can be presented without essential reference to observers.

Marshall has presented his rejection of action at a distance in a variety of papers, and in one of these (Tarozzi and van der Merwe 1985, pp257 - 270) he relates this rejection to the concept of a field. The forces of nature, he claims, are always mediated by fields propagating through space with finite velocities. "Any system, no matter how large and how complicated, interacts through fields propagating across the boundaries with its environment". Elsewhere (ibid,p88), along with Santos and Selleri, he adds "All known interactions (gravitational, weak, electromagnetic and strong) decrease with distance, and hence physical connections between two atomic systems should go to zero as their mutual separation increases".

Now I suspect that Marshall will in the long run be proved right on these particular matters. I would however regard them as subject to empirical test. I would also claim that the discovery of a force propagated instantaneously, or not decreasing with distance, would give little reason to assume the intervention of mental states (and still less reason to suppose that the moon ceases to exist when we shut our eyes). I shall argue below that there is as yet no experimental demonstration of action at a distance, nor likely to be any in the foreseeable future. Where I cannot follow Marshall ,however, is in the apparent belief that he can rule out action at a distance in an a priori fashion, saying (Marshall 1988, p4) that the abandonment of the principle of local action means the reversion from science to magic and (Marshall 1986) that an analysis which is non-local cannot be scientific. Was Newtonian mechanics non-scientific or magical?

ACTION AT A DISTANCE: AN EMPIRICAL QUESTION ?

I suspect that Marshall's preoccupation here is not simply with infinite speeds of propagation, but more generally with speeds exceeding that of light. He may well be right that our conceptions of the world necessarily rest in some causal framework, and that "this means insisting on the normal macroscopic notions of 'past' and 'future'". (Marshall in Tarozzi and van der Merwe, 1985 p267). If the space and time coordinates which occur in the Schrödinger equation belong to a frame of reference of the kind occurring in the special theory of relativity, then propagation of a causal effect at a speed greater than that of light will permit interchange of the time order of cause and effect by a suitable coordinate transformation. Any acceptable notion of causality must have difficulty with this.

A serious problem here for the non-specialist is that the philosophical problems of quantum theory have usually been discussed in terms of a non-relativistic form of the theory. Relativistic forms of the theory do exist, but there appear to be doubts whether these satisfactorily reconcile quantum theory and the special theory of relativity. Marshall and Santos (1988 p188) believe they do not, and quote Dirac (1976) as supporting this view. However, a continuing problem of reconciling quantum theory and the special theory of relativity, however much of a puzzle it is within physics, is not necessarily one in philosophy.

As Bitsakis (Tarozzi and van der Merwe 1985, p72) has put it, "locality is not a necessary condition for a realistic and causal conception of quantum mechanics, because it is possible to imagine,and eventually discover, more general forms of determinism than the known relativistic local ones" Such more general forms seem to me not to be demonstrated by the experiments I discuss below, but I do not see how they can be excluded on a priori grounds.

Marshall and Mermin represent highly contrasting forms of the view that to entertain superluminal propagation opens the door to magic and mysticism or, more prosaically, to the non-eliminable observer. There has on the other hand, been a strong school of thought in physics which has accepted superluminal propagation within a broadly realistic and causal interpretation of the quantum theory. The hidden variable theories, developed over more than 30 years by Bohm and his associates, fall into this class. (A recent paper, Dewdney, Holland, Kyprianidis and Vigier 1988, gives earlier references). I do not deal with such theories, because I seek a realist account of what working physicists do and of the concepts they employ, and it appears to me that working physicists have not made practical use of hidden variables in quantum theory. This does not exclude the possibility that at some time some form of hidden variable theory may turn out to be true. It is simply that if this does not happen it will have little relevance to the validity of simpler non-subjectivist accounts of quantum theory, such as the one I am discussing.

To sum up, I believe that if superluminal propagation of causal effects is ever demonstrated, this will not have any subjectivist implications. Whatever the outcome of the controversies on the experimental work which I consider next, it will not throw doubt on the reducibility of mental states to physical states.

My purpose in discussing this experimental work then is not to dispose of any threat to my view of quantum theory, but rather to show how the application of this view reduces the force of the argument that it demonstrates superluminal propagation.

EMISSION AND ABSORPTION OF RADIATION IN QUANTUM THEORY.

As I have said, I am concerned to show that the mental pictures used by working physicists in their employment of the quantum theory are sufficient to deal with the supposed philosophical puzzles of the theory. The experimental work most widely quoted to demonstrate apparent non-locality, non-separability or superluminal causal propagation involves the emission and absorption of radiations by atoms. Before I discuss the implications of this experimental work, I wish to show that the conceptual account of quantum theory I have advocated is in general agreement with thinking among those applying quantum theory to energy interchange between atoms and the electromagnetic field.

A typical approach is described by Loudon (1973). Consider a radiation field with an energy density W(n)dn at frequency n, exchanging energy with atoms capable of emitting or absorbing radiation of frequency n by transitions between an upper state 2 and a lower state 1. There are N_1 atoms per cubic metre in state 1 and N_2 in state 2. Then the rate processes are characterised by constants A_{21}, B_{21} and C_{12}, such that

rate of spontaneous emission	=	$N_2 A_{21}$
rate of stimulated emission	=	$N_2 B_{21} W(n)$
rate of absorption	=	$N_1 C_{12} W(n)$

For our purpose the first question of interest is the relation between the two kinds of emission, spontaneous emission at a rate independent of the radiation energy density and stimulated emission at a rate proportional to it. Loudon (1973, p15) gives the following equation, generally applicable to all atomic transitions

$$A_{21} = \hbar n^3 B_{21}/\pi^2 c^3 \qquad (10)$$

where h, π and c have their usual significance. This suggests some mechanism in common between stimulated and spontaneous emission, characteristic of the field rather than the particular atom and atomic transition concerned. So-called spontaneous emission appears therefore not to be a process internal to the emitting atom :it is more natural to think of it as induced by a ground state of the electromagnetic field, sometimes referred to as the vacuum field. This vacuum field is not capable of giving up energy, and can be neglected for the treatment of many electromagnetic effects (but not all - see Loudon (1973) on the Casimir effect). The B_{21} coefficient reflects only the emission induced by the electromagnetic energy over and above that contained in the vacuum field. The effect of the latter is reflected in the A_{21} coefficient.

This conceptual structure is again revealed in Loudon's treatment of the energy levels of the electromagnetic field itself (Loudon 1973 p9 and following). He derives these by regarding the field as an assembly of quantum harmonic oscillators, each with the usual quantised energy levels. It is of course a characteristic of the quantum oscillator that its ground state retains an irremovable half quantum of energy, again showing the need for the concept of a vacuum field.

It would be tempting to go even further and see A_{21} simply as a combination of B_{21} with an energy density for the vacuum field given by the coefficient $\hbar n^3/\pi^2 c^3$ in equation (10). However, Loudon's expression for the energy density of the vacuum field differs from this by a factor of 1/2. Clearly the semi-classical approach at this stage of Loudon's development reveals very little of the nature of the vacuum field.

What is perhaps more significant is that quantum field theory, which treats the electromagnetic field with fewer appeals to intuition based in classical physics, also presents spontaneous emission as a field induced phenomenon, arising in the same equation as the stimulated emission, and not in a separate equation as it does in the semi-classical presentation.

The above view of spontaneous emission clears away another puzzle: the electron in level 2 is in a stationary quantum state, and in the absence of an external stimulus should undergo no transitions at all.

The experiments relating to quantum non-locality we are about to consider involve "spontaneous" atomic emissions. I claim that I am following the conceptual processes of physicists working on interactions between radiation and atoms when I make the following assumptions

(1) The energy levels of the electromagnetic field are determined by a structure of "field modes" which always reflect geometric and other properties of the boundaries of the volume of space in question.

(2) These field modes cannot give up the last half-quantum of energy corresponding to their ground states.

(3) It is this ground state of a field mode which induces spontaneous atomic emissions.

(4) The emitted radiation appears in the field mode which stimulated it, and has its field properties determined by this, i.e. it has the same direction of polarisation as the stimulating radiation, and the field mode plus atom system satisfy energy and momentum conservation conditions.

Assumptions (1), (2) and (3) are justified by my earlier discussion. Assumption (4) extends Loudon's remarks on emission stimulated by an external light source to emission stimulated by the vacuum field (Loudon, 1973,,p24).

EINSTEIN,PODOLSKY AND ROSEN AND THE FREEDOM OF MEASUREMENT PRINCIPLE

We can now return to the apparent need for superluminal causal effects in quantum systems. As we have seen, Einstein and his collaborators ruled out such effects, and concluded from a consideration of a particular quantum system that a particle must in fact have simultaneous sharp values of corresponding momentum and position coordinates. Quantum theory does not predict such sharp values, only statistical ranges. The failure of the quantum theory to predict sharp values does not of course rule them out : the quantum theory could give ranges of values for populations of systems, each of which could exhibit sharp values. However,problems then arise from the theorems of Gleason, Kocken and Specker.

We have seen how these last problems can be overcome by regarding quantum probabilities as the combined result of variations between populations of systems with the same Schrödinger function on the one hand and an essential indeterminacy of values of momenta and coordinates in each system on the other hand. The indeterminacy within each system deals with the requirements of Gleason, Kocken and Specker, and the variation between systems avoids the need to postulate superluminal causal effects within the limited spread of coordinates demanded of any one system.

This still leaves a problem in systems of the kind considered by Einstein, Podolsky and Rosen (1935). Here the coordinates fuzzily determined by quantum theory, namely $x_1 - x_2$ and $p_1 + p_2$, define the configurations of the system, not in one locally confined region, but in a pair of locally confined regions which may be arbitrarily far apart. The coordinate $x_1 - x_2$ may well be allowed a limited degree of indeterminacy within any one system of a population, so satisfying Gleason, Kocken and Specker. It may also be allowed an additional variance over the population of systems so giving the total range of variance demanded by the quantum theory.

Now, however, any interaction of the system exhibiting a value of x_1 (or, as it is more narrowly put, any measurement of x_1) not only fixes a value of x_1 but also a value for x_2 which refers to a part of the system which may be arbitrarily far from that to which x_1 refers. The limited fuzziness we allowed to $x_1 - x_2$ no longer helps us : however limited this fuzziness, its removal at x_1 forces its removal at x_2 and we are back with superluminal propagation.

Of course, quantum theory does not forbid the definition of x_2 as sharply as we may require. It merely demands that the more closely x_2 is defined, the greater the range that must be available for p_2. However, Einstein and his co-workers claimed to have described a quantum system in which both x_2 and p_2 had to be sharply defined. Either x_1 could be measured, requiring a sharp value for x_2, or p_1 could be measured, requiring a sharp value for p_2 . Neither measurement disturbed the region of space to which x_2 and p_2 referred, so that the sharp values for x_2 and p_2 could not be caused by the measurement process itself. Einstein and his co-workers claimed that this meant that both x_2 and p_2 actually possessed sharp values in the undisturbed system. Bearing in mind Gleason, Kocken and Specker, are we forced to reject this claim by accepting superluminal propagation of a causal effect from the region containing x_1 and p_1 to that containing x_2 and p_2?

I have already described Bohr's comments on the 1935 paper. Bohr was not in fact concerned with avoiding superluminal propagation, on the impossibility of which he probably agreed with Einstein. Rather, he rejected the supposed proof that x_2 and p_2 had sharp determinate values in the undisturbed system. In cases like this, if quantum theory did not allow the prediction of a sharp value for a variable, then the system did not in fact posess such a sharp value. As others (not I think Bohr himself)have put it, the sharp value is brought into being by the act of measurement.

Bohr's answer was in fact a denial of what has sometimes been called the "Principle of Freedom of Measurement". We cannot in one and the same environment, or in one and the same experimental set-up, have a choice of measuring either x_1 or p_1. In a system permitting a measurement of x_1 and hence implying a sharp value for x_1, we cannot measure p_1, so no sharp value for p_1 is implied. Similarly, a set-up permitting measurement of p_1 precludes the measurement of x_1.

As I have shown, Bohr's argument is easily fitted into the picture of quantum systems which I am advocating here. Its importance for the present purpose is that it makes unnecessary any presumption of a superluminal causal influence from the region of x_1 and p_1 to the region of x_2 and p_2.

Bohr described experimental conditions permitting the measurement of x_1 but not p_1, and different experimental conditions permitting the measurement of p_1 but not x_1. His examples did not, however, amount to a general proof that no experimental conditions could be found permitting the free choice of measuring either x_1 or p_1. The problem for those who believed quantum theory implied superluminal propagation was to suggest experimental conditions which did permit such free choice.

Neither the particle pair system proposed by Einstein and his co-workers nor the measurement arrangements considered by Bohr had much prospect of practical realization. Practical ways of studying similar apparent cases of non-separability in quantum systems were however suggested. The work of Aspect and his co-workers (Aspect, Grangier and Roger 1981, Aspect, Dalibard and Roger 1982) is based on a type of test originally suggested by Bohm (1951).

THE ASPECT EXPERIMENTS

In both sets of experiments, Aspect and his co-workers studied a system in which excited calcium atoms revert to the ground state in two stages, emitting in succession bursts of radiation of wavelengths 422.7nm and 551.3nm respectively. This double emission falls into a class for which theory predicts and experiment confirms that the two radiation packets have the same plane of polarisation. Pairs of emissions within a certain beam width pass to polarisers, one emission of the pair to one polariser set at an angle a , say ,to an arbitrary axis, and the other emission to the other polariser set at an angle b to the axis. Radiation transmitted through the polarisers reaches a pair of photomultipliers, one for each polariser. The signals from each polariser pass to monitoring devices which count the numbers of responses of each photomultiplier, and also the number of coincidences in time of the responses in the two photomultipliers.

The correlated but separated events in this system are the reflections or transmissions at the polarisers for the pairs of emissions from a single atom. These in turn determine the recorded events, namely the excitations of the photomultipliers. The question of interest then is whether the pattern of correlations between pairs of photomultiplier excitations implies superluminal interactions between the events at the polarisers.

To answer this question, adjustments must first be made to the raw data, relating to imperfections of real polarisers and to photomultiplier efficiencies. The nature of these adjustments depends upon a theoretical interpretation of the relevant physical processes. Let me put this matter on one side for the present, and assume we have suitably adjusted data. Let me also for the present confine my attention to the results in Aspect's first paper (Aspect, Grangier and Roger, 1981). Accepting the theory-laden adjustments, the proportion of coincidences between transmissions through the two polarisers R to total bursts of emission reaching the two polarisers R′ is given by

$$R/R' = 1/4\ (1+\cos 2(a-b)) \qquad (11)$$

This expression is suggested by certain theoretical considerations, and is confirmed within experimental error by Aspect's results.

So far in this account of Aspect's work, I have said nothing to prejudge the contentious parts of the interpretations of equation (11).
In particular I have not yet used the term "photon", because conflicting views on the nature of the photon underly conflicting views on the implication of equation (11).

In its minimal interpretation, a photon is no more than a convenient way of referring to the discrete nature of energy exchanges between atoms and the electromagnetic field. There need be no entity corresponding to it in either the atom or the electromagnetic field. However, in order to avoid superluminal effects within the electromagnetic field itself, some degree of localisation of energy, in quantities corresponding to photons, appears to be necessary (see the discussion on the corresponding problem for electrons on page 13) Such localisation can be accomodated without much offending against the continuity features of the electromagnetic field (see, for instance, Prosser, 1976). In quantum field theory too, the photon has a rather shadowy status as a unit of occupation number of the energy levels of a particular field mode.

Compared with these conceptions of the photon, some of those employed in discussion of the Aspect experiments give it much more automonous character. It is imagined for instance as having a precise location in space, and a definite polarisation, analogous in some ways to the angular momentum of a classical particle. The polarisation is also supposed to be acquired by the photon at its time of emission from an atom, without the intervention of a pre-existing field structure.

The relevance of this is that equation (11) cannot be reconciled with photon emissions uninfluenced by a field embracing emitters,polarisers and detectors without assuming causal effects travelling between the polarisers at speeds greater than that of light.

To show this, we have to demonstrate that equation (11) cannot be satisfied for all possible values of (a-b) if the polarisation properties of the emitted pairs are unaffected by the polariser settings. A single example of such a failure to satisfy the equation is sufficient, and I adapt one from a paper by Kraus (Tarozzi and van der Merwe, 1985 p84).

Consider three possible settings for the polarisers, at angles 0, a and 2a to an arbitrary axis. It is convenient to consider coincidences produced not only when both of a pair are transmitted by the polarisers, but also when both are reflected. The two kinds of coincidences are equal in number, so that with an obvious notation we have

$$C\,(0,a) = R'/2(1+\cos 2a)$$

$$C\,(a,\ 2a) = R'/2(1+\cos 2a)$$

$$C\,(0,2a) = R'/2(1+\cos 4a)$$

where C (0,a) refers to the total coincidences, reflected and transmitted, for polarisers at angles 0 and a respectively, etc. It follows that total non-coincidences are as follows

$$N\,(0,a) = R'/2(1-\cos 2a)$$

$$N\,(a,2a) = R'/2(1-\cos 2a)$$

$$N\,(0,2a) = R'/2(1-\cos 4a) = R'\sin 2a \qquad (12)$$

where N (0,a) is the number of pairs giving reflection at one polariser and transmission at the other, etc.

It follows that since non-coincidences with polarisers at 0 and a are $R'/2(1-\cos 2a)$, and non-coincidences between a and 2a are $R'/2(1-\cos 2a)$, the non-coincidences between 0 and 2a are at most $R'(1-\cos 2a)$.

$$\text{i.e.} \qquad N\,(0,2a) < R'(1-\cos 2a) = 2R'\sin^2 a \qquad (13)$$

The expressions (12) and (13) for N (0,2a) are inconsistent, since

$$R' \sin^2 2a > 2 R' \sin^2 a \quad \text{if } a < \pi/4$$

as can be seen by writing the left hand side as

$$4 R' \sin^2 a \cos^2 a.$$

The inconsistency arises from the assumptions that the polariser responses of a pair of photons are fixed at the time of emission, that they are not influenced by the polariser settings and that the response of one photon to a polariser has no effect on that of the other. We therefore have the following choice:

either, (1), causal influences in quantum systems can travel at speeds greater than that of light,

or, (2), the polarisation properties of radiation emitted by atoms are determined by the surrounding electromagnetic field, which in turn is affected by boundary conditions such as polariser settings.

As I have suggested above, the second alternative seem to me to fit into quite widely held views on absorption and emission of radiation. I have therefore no problem in embracing it and rejecting the first alternative. There are those who would reject both alternatives and I will return to their case in connection with Aspect's second experiment.

Before doing this, let me be quite explicit about the process of field-induced emission of radiation as it applies to this case. The vacuum field filling all space is the site of a structure of ground state field modes. These have an objective reality, and are not merely heuristic appendages to the use of the formalism of quantum field theory. Their structure is determined by boundary systems, considered in the widest sense, including not only enclosures but also optical systems such as lenses, filters, reflectors, polarisers and sources and sinks of radiation. The field plays an active part in excitation processes in photomultipliers and in emission processes in atoms. The field mode inducing radiation emission determines the polarisation property of the emission. Since in this case the field mode is a property of a space containing polarisers and photomultipliers, the polarisation properties of the emitted radiation are determined in relation to these particular devices. It is meaningless to ask how the radiation would react to polarisers with different settings. Altering these settings alters the field mode structure, and in turn the emitted radiation.

I claim that this picture is not very different from that implicit in text books on quantum optics (cf Loudon, 1973). It must be emphasised, however, that it is not necessary to be as specific about mechanisms as I have been in order to reject experiments of the Aspect type with fixed polariser settings as evidence for superluminal propagation. All that is needed perhaps is to note the possibility that experimental arrangements in place at the time the radiation is emitted may determine the polarisation properties of the radiation. This is certainly a logical possibility, and it is not inconsistent with any generally accepted principles in physics.

The less specific grounds for rejecting the first Aspect experiments as evidence for superluminal propagation are, I think, accepted fairly widely as valid (see, for example, d'Espagnat, 1984 pp 229,230),and they are given by Aspect and his co-authors in the paper which I will now discuss.

ASPECT'S SECOND EXPERIMENT.

Clearly the presumed influence of the polariser settings on the polarisation properties of the emitted radiation becomes questionable if the settings are established after the emissions have occurred. The experiments now discussed (Aspect, Dalibard and Roger, 1982) were motivated by this consideration.

The production of pairs of radiation bursts from suitably excited calcium atoms followed the lines of the first experiment, but now each of the pair of emissions was directed to a switching device and thence to either of two polarisers with different settings. At time intervals of the order of 10ns, each switch redirected the incoming beam from one polariser to the other. Since the time of travel from the emission source to any of the four polarisers was about 40ns, the arrangement was intended to ensure that no stationary field pattern determined by the polariser settings met by the emissions could affect the polarisation properties of the emissions. The switching frequencies in the two beams were not only different but incommensurate, so as to avoid the regular recurrence of time intervals with a particular pair of polariser settings out of the four possible pairs in any one experimental run.

As in the first experiment, the proportion of coincidence to total emission was related to the relative orientation of the polarisers by equation (11) above. By the argument already given, such a relationship is impossible if the responses to the polariser settings are already determined at the time of emission, and if, as appears to be the case here, the polariser settings cannot influence the emissions.

It appears to be agreed that the assumption of action at a distance can then be avoided only by challenging one of the steps made in processing the data. The photomultipliers have a detection efficiency of only about 10%, and it is assumed in the data processing that the radiation bursts detected are a random selection of those in the relevant beam. If, however, a detection depends upon a certain concordance between the polarisation properties of the beam and the setting of the polariser, a given pair of emissions does not have to respond according to equation (11) for all possible settings. Putting it simply, if the response is inappropriate, the radiation is undetected by the photomultiplier.

This interpretation of the experimental results,without action at a distance,appears to be generally accepted as a logical possibility, but many physicists who have written on the subject dismiss it on the grounds that it is utterly implausible. (cf Six, in Tarozzi and van der Merwe, 1985 p171, Clauser and Horne, 1974, and many others).

Plausibility judgments are difficult to evaluate. In my discussion of the experiments with fixed polariser settings, I gave a picture of events based upon a somewhat literal interpretation of treatments of quantum optics. Let me see how this can be applied to the experiments with variable polariser settings.

Again we think of a field mode capable of inducing an emission from a calcium atom and also of exciting an electron in a crystalline solid in a photomultiplier. Now, however, in the 40ns or so separating an induced emission from the corresponding excitation, we have the channel connecting the two events interupted several times for 10ns intervals. We therefore imagine our field mode structure as a superposition of the four structures corresponding to the four pairs of relative settings of the polarisers. A particular burst of radiation from a calcium atom is induced by one of these four component field structures. If this burst of radiation meets a polariser setting corresponding to the field structure which induced it, then it may be detected at the photomultiplier. If the other burst of radiation from the same two-stage calcium atom transition also meets a polariser set at the appropriate angle, then it too may be detected, and we may have a coincidence recorded by the monitoring equipment.

For a pair of emissions from the same calcium atom transition, the probability of a coincidence is related to the relative settings of the polarisers which characterise the field pattern which induces the emission. This probability has the relation of equation (11) to the angle between these two polariser settings. We cannot derive contradictions by considering the probability of a coincidence if the same pair of emissions meet other polariser settings : the photomultipliers would not respond and the question does not arise.

As far as I can establish, this picture is not inconsistent with any well-tested physical theory. If I am right, then the propagation of causal influences at speeds greater than that of light has not yet been demonstrated experimentally.

Six (Tarozzi and van der Merwe,1985,page 180) has objected to any account of this kind that it assumes the existence of two sorts of photons,detectable and undetectable. This seems wrong. What we have here is not an assumption of the existence of undetectable photons,but an experimental demonstration of the existence of undetected photons.I take the non-detection to be systematically determined. Six takes it to be random. Randomness here may mean that detection or non-detection is determined by factors internal to the detector.For me detection,like any other form of absorption of radiation,is induced by a field,and the field structure is influenced by geometric features such as polariser settings.

It has also been objected that the disturbance to the field pattern caused by the polariser switches should have caused departures from relation (11) between the relative polariser settings and the proportion of coincidences.In fact (11) remained valid for the second Aspect experiment.

I do not find this surprising.The detection of a photon is the result of an interaction between an electron in a detector and a field mode and equation (11) is a consequence of a definite relationship between two such field modes.The regular interruptions to the passage of photons along the channels provided by the field modes would be expectedto reduce the number detected (a reduction in detection was noted,but it was explained differently). The coincidences which are detected should still satisfy (11)

BELL'S INEQUALITIES

It is usual to apply the statistical tests known as "Bell's Inequalities"(Bell,1964) to the correlations in experiments such as those of Aspect. Infractions of Bell's inequalities would show that the correlations could not arise from additional constraints imposed on the photons at time of emission. The correlations in both series of experiments carried out by Aspect infringed the Bell inequalities (Aspect et al, 1981 and 1982) This infringement indicates the same feature of the experimental results as the inconsistency I demonstrated between three applications of equation (11).

The application of Bell's Inequalities is a much more general test than mine, which is restricted to the particular case to which I applied it. I chose the less general test because it appeared to me to show more clearly the origin of the problem.

However, as evidence for transmission of a causal effect between a pair of polarisers, Bell's test is invalid for exactly the same reason as mine, namely the failure to exclude the possibility of an influence on the radiation emission process by the polariser settings. (Pena, Cetto, Brady, 1972). Aspect's second experiment purported to rectify this flaw, and I have given my reasons for supposing that it did not do so.

ENHANCEMENT

The kind of process to which I appealed in rejecting the application of the Bell Inequalities to Aspect's experiments belongs to a wider class to which the name "enhancement" has been applied. (Clauser and Horne, 1974) It appears to be generally accepted that enhancement is not ruled out for these experiments,or others like it, by anything in current physical theory. Objections to enhancement therefore rest solely upon the supposed implausibility of the mechanisms assumed to be involved. Obviously I do not find my own account implausible, but if I am "sweeping the thing under the carpet" (Snowdon, 1987), I would appeal to Holmes' observation that when we have excluded all the probable explanations, an improbable one must be accepted.

Marshall is consistent on this matter. Since he rules out action at a distance on a priori grounds, he can take Aspect's work as proving the existence of enhancement phenomena. The version of enhancement I have given is along the lines of a more precise account given in Marshall (1980).He has since offered a rather different version (Marshall,1989) which I do not discuss here.

CONCLUSION

I claim that I have shown that quantum theory, and the interpretive concepts relating it to observation, can be presented without reference to subjective entities such as observers, conscious minds or measuring equipment. In so doing, I have made use of ideas which have been current for 20 to 30 years, and which to my knowledge have never been refuted. I have suggested that these ideas have always been close to those used by people when they have been practising physics rather than metaphysics (I am not of course opposing the practice of metaphysics, but merely presenting my own version).

In particular, I have sought to show that these ideas are consistent with what is often regarded as mainstream thought in quantum theory, namely that of the Copenhagen school. My disagreement with this school is not over any subjectivist tendencies, because neither Bohr nor Heisenberg (certainly in his later years) had any, but rather over their non-evolutionary view of representations of the world in human brains.

It is true that I have taken a more limited view of the scope of quantum theory than is commonly done, but my treatment does not, I suggest, exclude it from any field in which it is actually used, and this is surely sufficient.

More recent worries over the foundations of quantum theory have centred upon its apparent incompatibility with the special theory of relativity. I have shown that, however this incompatibility is resolved, it provides no basis for a renewal of subjectivist accounts of quantum theory.

On other grounds, however, action at a distance does give rise to uncomfortable philosophical problems. I have argued that no experiments carried out so far have in fact demonstrated action at a distance, nor are such experiments likely, I believe, in the foreseeable future.

In the form I have presented it, quantum theory is no bar to a view that mind states are brain states, and brain states evolved from a pre-existing physical world, operating according to laws unaffected by the emergence of mental entities of any kind.

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