Quantum Heterodoxy: Realism at the Plank Length

Adrian Heathcote
Department of Traditional & Modern Philosophy
University of Sydney

Appeared(?) in *Science and Education*, April, 2003

Quantum Mechanics has long presented significant problems for philosophers because it seems to support, and be supported by, a cluster of philosophical doctrines that most philosophers working today would reject. I will call this cluster of doctrines, *Idealism*—though the term is certain to cause some confusion, enmeshed as it is in the complexities of the philosophy and religion of the Eighteenth and Nineteenth Centuries. The founders of quantum mechanics absorbed this Idealism as part of the intellectual climate of their youth and then used it as the guiding philosophy in their interpretation of quantum mechanical phenomena. This interpretation was then passed back to philosophers and, more importantly, the philosophically sensitive layperson, as if it were now a vindicated philosophy. The result has been, increasing from the 1960s onwards, a popular resurgence of Idealism, but borne now on the back of science as it once was borne of the back of religion.

This resurgence of Idealism has brought in its train a number of other unfortunate ideas. By aligning quantum mechanics with Idealism, the founders also aligned Realism with classical mechanics—something that is still present in the idea, common to many commentators on quantum mechanics that Realism is equivalent to Hidden Variables. Thus we still hear that the vindication of quantum mechanics over hidden variable theories in the testing of the Bell Inequalities, is a decisive refutation of Realism, as it is not. We also find that there is a tendency to disparage the achievement of Newton—since, apparently, the Mechanism involved in Newtonian Mechanics is less congenial than the Mechanism involved in Quantum Mechanics. This same spirit has also encouraged such popular follies as *Quantum Healing*, and *The Tao of Physics*.

In my view, philosophers have taken too little notice of this interaction between quantum mechanics and Idealism. For even though Idealism is a deeply implausible piece of philosophy—and even, perhaps, *because* of the fact that it is—it should be quite surprising that there is any alignment between the two views. Why should there be *any* connection between a philosophy born of the pious idea to save our perceptual knowledge from scepticism—abetted by a theory of perception that it seems is impossible to describe without entailing an infinite regress—and a physical theory created to solve problems in the spectral lines of excited atoms?

If we were to examine the two views, however, I think that the surprise would melt away and we would find that they do not align that well at all; that they have rather been forced into an uneasy marriage and deformed in the process. But I will not argue this in what follows. Rather my aim is to show that being Realists about quantum mechanics in the only way that this makes sense—*i.e.* accepting the explanatory structures of the theory at face value, as our best guide to how the microphysical world really is—faces a genuine difficulty in the measurement problem. But I also want to argue that the prevailing *anti*-Realist stance of the Copenhagen Interpretation has led to a serious misunderstanding of the theory that has had consequences throughout. Thus we ought to think that quantum mechanics needs supplementation on the character of measurement interactions, but that it already offers us a rich ontology of states to be going on with in our attempts to understand those measurements. In the first section below I will describe the conceptual background within quantum theory; then I will sketch the two problems; finally, I close with some general remarks about their significance for Realism.

1 Conceptual Structure

Suppose that we have a system in a (pure) state; then, if the system is left to itself it will evolve in a deterministic or unitary fashion according to the Schrödinger Equation. If we were to make a measurement of this system to determine the value of some observable property (position, momentum, spin along some chosen axis, *etc*) then, if the system was in a superposition of definite states (called *eigenstates*) before the measurement was made, after it will be in some particular eigenstate (which is represented by an *eigenvector*). The probability of that eigenstate was given by the "weight" of that eigenstate in the original superposition. (We will ignore here the possibility that the value found—which is called the *eigenvalue*—may be associated with more than one eigenstate: this can occur when there is, what is called, a degenerate spectrum. This possibility will not concern us here, though it will reappear in a slightly different form in section 4.)

The probability of finding the system in some particular eigenstate after the measurement is not interpretable as an epistemic probability, for if it were, all systems would have to be in particular eigenstates of all observables all the time (and we be ignorant of just which they were) and that is not possible. The reason it is not possible is that some pair of observables may be incompatible. If they are incompatible their corresponding operators will not commute and an eigenvector of one will be a superpositional vector of the other. So states cannot always be eigenstates of all observables and probabilities must be probabilities of an objective change in the system.[1] Indeed in certain cases this argument can be considerably strengthened to show that it is actually mathematically impossible for a system to be in eigenstates of all the members of some collection of observables. This is the content of the famous Kochen and Specker 'No Hidden Variables' Theorem. The upshot is that there can be no 'ignorance interpretation' of superpositional states and the consequent collapse.

We can put this same point the other way round. Since probabilities are measures of objective changes in systems, before measurements are made superpositional states are states in which systems do not have definite properties—or more exactly, in which not all properties can *always* be definite.

This simple picture is probably familiar to everyone, since it is the basic presentation of innumerable textbooks. Unmeasured systems evolve according to Schrödinger's equation, while measured systems undergo the so-called 'Collapse of the Wave Function'. Operators may fail to commute and in one particular instance of this, with position and momentum, there are Uncertainty Relations constraining the possible values of measurements. Moreover, if a system's state is completely given by the quantum mechanical formalism and observables are represented by non-commuting operators then probabilities must be probabilities of objective changes in the state of a system.

But though unavoidable this picture is also quite unsatisfactory. Unless it is specified what a measurement is, we simply have two different kinds of state evolutions with no acceptable way of knowing which is governing the evolution of a system at any moment. Suppose we take the natural interpretation of a measurement as just an instance of a physical interaction between the measurement apparatus and the system that is being measured—a view that leading advocates of the Copenhagen Interpretation, such as Bohr, had rather assumed was correct, despite their official Idealism. What then is the outcome?

This matter came to a head in 1935 with the work of Schrödinger and von Neumann. If a measurement were simply an interaction with a measuring device, then, rather than the measuring device collapsing the superposition of the particle,

[1]The term *objective probability* is sometimes used by philosophers in other ways, and in my view is best avoided unless a great deal of care is taken to say what it does, and does not, mean. It is more perspicuous to say that the probability is the probability of some objective change in the system.

the superposition of the particle will infect the measuring device and throw it into a superposition. It was this that Schrödinger wanted to dramatise with his famous cat example. Take a particle in a spin superposition S with two eigenstates S_+ and S_- so that

$$S = \frac{1}{\sqrt{2}} S_+ + \frac{1}{\sqrt{2}} S_-.$$

Now consider a measurement apparatus M, initially in a resting state, but with two pointer positions $\Uparrow$ and $\Downarrow$, for the spin states. Consider further this apparatus hooked up to a device that will drop a pellet of cyanide into a sealed chamber with a cat inside if and only if the apparatus goes into the $\Downarrow$ state. Let us now allow the particle and the apparatus to interact in the normal method for a measurement to take place. What happens to the cat?

Schrödinger's reply was that if we allow the interaction to take place and follow the unitary evolution of the, now combined, system of particle and apparatus, we will not get what we expect to get, namely the particle in, say, state S_+ and the device in state $\Uparrow$ and the cat alive (or, for the other alternative, the particle in state S_- and the device in state $\Downarrow$ and the cat dead). Rather the state of the combined system is now a pure state C in the tensor product space and we have

$$C = \frac{1}{\sqrt{2}} (S_+ \otimes \Uparrow \otimes (\mathsf{cat\ alive})) + \frac{1}{\sqrt{2}} (S_- \otimes \Downarrow \otimes (\mathsf{cat\ dead})).$$

Thus the particle has driven the composite system into a superposition rather than the apparatus collapsing the superposition of the particle. Not only is the cat not in a live or dead eigenstate, it—or rather its state—is not obviously even disentangleable from the combined system as a whole. (This is a point that is usually not made entirely clear in popular expositions of Schrödinger's Cat.)

And yet, as Schrödinger notes, when we open the device we will see either a live or a dead cat, and the apparatus indicator will be pointing $\Uparrow$ or $\Downarrow$. How can we reconcile this observational result with the Unitary evolution of the Schrödinger equation? Schrödinger thinks we cannot. To be consistent with observation, quantum mechanics must postulate the second kind of state change—the collapse of the wave packet. Only then will we have a result that is in conformity with experimental results.

Here we have an illustration of the Measurement Problem in quantum mechanics: it is from this that we get the two state transformations of the simple picture. Much ink has been spilled on it since 1935—and the theorems for it have become quite sharp, particularly since Wigner's work in the 1960s. In the next section I want to bring out an aspect of this problem that has been given little or no attention.

2 Negative-Outcome Measurements

Let us shift our attention from spin to position and momentum—observables that are closer to our classical hearts.

The wave function for position has a specific form—it is a square integrable function defined over the points of space, represented by $\Re^3$. These functions form a space $L^2(\Re^3)$, which is a the Hilbert space of states in this instance. In fact we can simplify the discussion below by suppressing two of the spatial dimensions—so that we consider the functions in $L^2(\Re)$. Thus each square integrable function is a "vector" in this Hilbert Space.

The momentum wave function is also a square integrable function in the same Hilbert Space. In fact it is the Fourier transform of the position wave function.[2] Thus there is an intimate connection between the position state function and the momentum state function—a mutual dependency that is the underlying explanation for the Heisenberg Uncertainty Relations.

The position operator is defined by

$$(\mathrm{Q}\psi)(x) = x\psi(x), \qquad \text{for } \psi(x) \in D(\mathrm{Q}) \subset L^2(\Re) \tag{1}$$

where the RHS gives the dense domain in $L^2(\Re)$ over which the position operator is defined (note that it is not defined over the entire Hilbert Space). The momentum operator P is defined by

$$\mathrm{P}\colon \psi(x) \longmapsto -\hbar d\psi(x)/dx. \tag{2}$$

The domain of this operator is the set of all absolutely continuous functions—which again is dense in $L^2(\Re)$. The Hamiltonian operator H is given by

$$\mathrm{H} = 2m^{-1}\mathrm{P}^2 + \mathrm{V}(x) \tag{3}$$

where m is the mass of the particle, P^2 is the square of the momentum operator, and $\mathrm{V}(x)$ is the self-adjoint operator for the potential energy. The domain of H is the intersection of the domains of P^2 and $\mathrm{V}(x)$—though the latter operator has to be chosen carefully so that the resulting domain is dense in $L^2(\Re)$.

The operators in (1), (2) and (3) are rather misleadingly named for they do not give position, momentum or energy of any system—rather they give the *expectation values* for these quantities. Thus they are mean values and not necessarily what one

[2] In fact to extend the Fourier transform from all smooth functions to all those that are square-integrable it is necessary to use an approximation, giving us what is known as the Fourier-Plancheral Theorem. This makes the Fourier transform a unitary transformation of the Hilbert Space onto itself. It is also equivalent to the Stone-von Neumann Theorem on the uniqueness of the Canonical Commutation Relations.

will find if one makes a measurement. This fact was obscured in Dirac's original presentation of quantum mechanics and has been responsible for a certain amount of misunderstanding ever since.[3]

If we want to understand the nature of measurements—the values that may result and the probabilities of same—then we must look, not to the unbounded operators above, but to their bounded spectral projections. For it is only if we look at the operators that describe results of performed measurements that we will get the correct mathematical modelling of the collapse of the wave function. And because these projection operators are bounded they *do* have eigenvalues and eigenvectors.

The spectral projections are orthogonal projection operators defined roughly by projecting a "vector" down onto a measurable set of the unbounded operator with which it is associated. Thus with a function $\psi(x)$ in $L^2(\Re)$ and a self-adjoint operator A with spectrum σ(A), a spectral projection of A, denoted P_Ω^{A}, is a map with the effect

$$P_\Omega^{\mathrm{A}}: \psi(x) \to (\chi_\Omega \psi)(x). \tag{4}$$

The map χ_Ω is the characteristic function with value 1 for $x \in \Omega$ and 0 for $x \notin \Omega$. The set Ω is a Borel set of the real line and thus after a position measurement is made—provided the particle is found in Ω—the function will be projected entirely into that subset and normalised. The wave function thus collapses inward, into a more strongly localised function. The *support* of the function is now a subset of the support that it had before the measurement was made. (The support of a function $\psi(x)$ is defined as the closure of the set of points at which the function is not equal to zero.)

The probability of finding a particle in some set Ω when it is initially in state $\psi(x)$ is thus given by the usual product calculation

$$< \psi(x), P_\Omega^{\mathrm{Q}} \psi(x) > = \int_\Omega |\psi(x)|^2 dx. \tag{5}$$

It is obvious that this probability will be less than one iff Ω is a proper subset of the support of the original $\psi(x)$.

We have already noted that the momentum wave function is the Fourier transform of the position wave function. We now point out an important fact about the supports of the two functions. The Paley-Weiner Theorem states that if the support of $\psi(x)$ is compact then the support of its Fourier transform is the whole of $\Re$, since the latter is then an analytic complete function. In other words if the position wave function is localised—even if the area of its localisation is extremely large—the momentum wave function is *infinitely* unlocalised. The converse also holds: if the

[3]Indeed it was this that was partly responsible for the misguided attempt to force an eigenvector and eigenvalue structure on these operators, when their spectra are, in fact, continuous.

momentum wave function has compact support then the position wave function has support over all of $\Re$.

The situation is thus far more severe than Heisenberg and many other followers of the Copenhagen Interpretation believed. They believed that only if the position was *pointwise* localised would the momentum wave function be spread out over the real line. They were misled in this by a misinterpretation of the Heisenberg Uncertainty Relations (HUR)—which is actually a far weaker constraint than the Paley-Weiner theorem. Indeed the HUR is not about localisation so much as it is about variances from the expectations given by the unbounded operators (1) and (2). They also misrepresented pointwise localisation as being equivalent to the system having particle-like properties, rather than wave-like properties, as per the Complementarity Principle.[4] In reality pointwise localisation is *always* incompatible with the wave function being in $L^2(\Re)$ and is at any rate not required for the particle to *be* a particle—a point that we will return to.

We have now assembled the materials necessary to state our problem. We have seen that there is a probabilty of finding a particle in some Borel set Ω. That probability will be positive provided that the intersection of Ω with the support of the wave function $\psi(x)$ is a measurable set. If the particle is observed, the act of making the measurement changes the wave function to some new function which has support entirely within Ω. This is confirmed by the fact that an immediate remeasurement—so that there is no time for the particle to change by ordinary Unitary evolution out of Ω—must still find the particle within Ω, but now with probability one. But what happens if on the first measurement we had *failed* to find the particle within Ω? Then immediate remeasurement must still fail to find the particle within Ω and so the wave function $\psi(x)$ must have collapsed—but now to 0 within Ω.

If we think of Ω as initially lying entirely within the support of $\psi(x)$ then we may think of the act of having a negative-outcome measurement as making a "hole" within the wave function for the particle. Not observing the particle has still changed its wave function—it has just flattened it down to zero on Ω and raised the wave function appropriately on either side.

We can take this puzzling situation further by considering what happens to the momentum of our unobserved particle. We have seen that the momentum wave function $\widehat{\psi}(x)$ is the Fourier transform of the position wave function $\psi(x)$ and that if the latter changes as a result of a positive-result measurement the former must change as well. This will still hold if we were to fail to observe the particle—$\widehat{\psi}(x)$ will change as a result of the new position wave function. Oddly, not observing the

[4]The correct view was argued by Paul Busch and Pekka Lahti in 'A Note on Quantum Theory, Complementarity, and Uncertainty' in *Philosophy of Science* 52 (1985) pp. 64–87. They went on to address other aspects of this problem in a number of important papers that appeared throughout the Eighties.

particle affects its velocity!

There are several reasons why this situation is anomalous. Firstly, it is obvious that wave functions can have shapes that are more exotic than we usually imagine. We grow accustomed to thinking of a wave function as a narrow spike—where the narrowness is on the order of the Plank length—and forget that there are many other functions in $L^2(\Re)$ that also represent possible states. By not observing a particle where it has some chance to be we can create a wave function that has a wide hole at its centre. Indeed there is no reason why the wave function cannot have separate pieces, in between which it is zero. Since the wave function is not a localised object it cannot represent a point-wise localised object such as a particle. The wave function of the particle is something other than the particle itself. It is wrong to try to constrain one's idea of what the wave function could be to make it fit one's intuition of what a particle is.[5]

But there is a second reason why the situation is anomalous. It is natural to think that if the particle fails to be detected by a particle detector then that must be because its wave function did not "come into contact" with the device. But if its wave function changes as a result of a negative-outcome measurement then there must have been some interaction. The detector is not changed, though the particle has been. Even the momentum wave function of the particle has been changed by this event. Does this not violate the principles of conservation of energy and momentum? In fact in this case it does not seem to matter whether one imagines the particle interacting with the detecting device or not, conservation laws still seem to be violated. This is because whether there is interaction or not, the particle detector is unchanged while the particle certainly has been.

In one of the few discussions of this problem in the literature, R.H. Dicke has argued that there *is* interaction with the measurement device but no conflict with the conservation laws.[6] The essence of his argument is that we can balance out the effects of positive and negative measurements and acheive the zero-sum that the conservation laws require.

Specifically, he considers a variant of Heisenberg's Microscope *Gedankenexperiment*. Suppose that an atom is in its ground state and confined to a box that is shallow

[5]Here is one thing that it is tempting to say which cannot be said: that when the particle fails to be detected there is no collapse. That will make the probabilities fail to add up, for if the probability of detecting a particle is μ then the probability of not detecting it must be $1 - \mu$.

[6]There is nothing new under the sun. The idea of negative-outcome measurements was thought of by the present author who developed certain properties of their measurement characteristics. He then discovered, somewhat to his amazement, that it had been discussed by others earlier, first by Mauritius Renninger and then by Dicke in two papers 'Interaction-Free Quantum Measurements: A Paradox?' *American J. Physics*, 49, 925–30 and 'On Observing the Absence of an Atom' in *Foundations of Physics*, 16, 107–113. It will be clear from what follows that I don't believe the oddity of the situation has been fully appreciated.

in the direction of the photon propagation, but long transverse to that propagation. The wave function of the atom is to have all its support in the interior of the box. We subject the atom to a burst of radiation, which is to be coherent and of short wavelength compared to the size of the atom. It is directed at the left-hand side of the box and the scattered photons, if any, observed. Dicke argues that even when no atom is observed there will still be an interaction between the atom and the radiation by the process of "photon handling". In this process a photon is absorbed by an atom and then re-emitted into the photon stream in its original state. Dicke admits that this looks to be a violation of the conservation of energy, but thinks that the problem can be resolved: if the energy is increased when a negative result occurs then, given that the total system is governed by Schrödinger equation the energy must decrease when a positive measurement is made. Dicke concludes

> . . . the combined mean energy after the observation is the same as before. . . In conclusion energy conservation is no problem. . . If the mean energy is increased when the atom is observed to be missing, it is decreased when the atom is present. All debts are paid and the books are balanced.[7]

There are several problems with this argument. The first is that the principle that Dicke states is not the Conservation of Energy Principle. What is required for conservation of energy is that the total energy of the system is conserved *on every trial* not just on the average over a number of trials. What if the experiment is done just once—that will leave us with a violation of conservation of energy that is unbalanced by any future act? The second point is that it follows from Dicke's argument not from first principles that the total energy is decreased by a positive result measurement. This is in effect solving the problem by fiat. Also it is quite implausible that it should be so: after all the particle was in its ground state and no lower energy is possible. What strange negative state does Dicke think the particle has fallen into?

I conclude that Dicke's argument must fail. It is worth recalling that when Schrödinger gave his famous cat example in 1935 he also pointed out in the same paper that the collapse of the wave packet must violate conservation of energy, since it is an interruption of unitary evolution. Our negative outcome measurements just make the same point more graphically.

3 The Interpretation of Superpositions

We have been looking at the superpositional pure states of quantum mechanics, at how they might change to yield eigenstates after measurements, and what it means

[7] Dicke (1986) p. 113

to take them with full ontological seriousness. On this latter point it is worth setting out the alternatives more clearly.

There are two tempting, but ultimately mistaken, ways of seeing (superpositional) quantum states. The first is the 'ignorance interpretation', in which we believe the system is really in some eigenstate but we are ignorant of which. This is usually called the Hidden Variable view. As already noted there are good, perhaps even overwhelming, reasons to believe that it can't be true. The second way of seeing the quantum state is to see the system as having this as a direct characterization of its form before measurement. Thus, a system that has a position wave function $\psi(x)$ in $L^2(\Re)$ is seen as spread out over the entire support of its wave function. It is, so to speak, *smeared out* in space. This view has been encouraged by the doctrine of Complementarity, which pictures the state in the light of its measurement outcomes. Thus a particle that evinces wave-like behaviour *is* a wave, and a particle that strikes a screen with pointlike scintillations *is* a particle. Complementarity sees the manifestation of these "natures" as conditional on the measurements being performed. Bohr's Idealist interpretation of this complementarity was to think that (to adapt the quip of Gertrude Stein) that as far as reality goes there is no *there* there. But for those who retrench from the full Idealist view there is the idea that particles are 'smeared' objects. This seems to be Dicke's view, above.

Both of these view are, I suggest, mistaken. Particles are not smeared out in space, rather it is the state function that determines probabilities that is distributed. But this probability function is not an epistemic function either. The probabilities are fully objective. We can gain a better understanding of this if we look at the total set of states.

4 Mixed States

Thus far our picture of quantum mechanics has been restricted to, what are called, pure states. Indeed there is a long tradition—particularly in the physics literature—of seeing such states as the only kind of states there are. But there are compelling reasons for thinking this not to be so. There are other states, called *mixed states*, that are fully real and that moreover are responsible for many of the most distinctive features of quantum systems.

What, then, *is* a mixed state?

A mixed state is a weighted sum (a convex combination) of pure states where the total weight is one.[8] Thus suppose that we identify the pure states not with the one-

[8]The *locus classicus* for the study of mixed states and their decompositions is E. Beltrametti and G. Cassinelli *The Logic of Quantum Mechanics*, Encyclopedia of Mathematics and its Applications Vol. 15, Addison Wesley, 1981. This is a book that should not have been allowed to go out of print. See also

dimensional subspaces of the Hilbert space, but rather with the projection operators onto these rays, then a mixed state is a weighted sum of projection operators. Thus

$$W = \sum_i w_i P_{\psi_i} \qquad \text{where } \sum_i w_i = 1 \tag{6}$$

Going the other way—*i.e.* starting with a mixed state—we can get a canonical decomposition of it into a weighted sum of orthogonal projection operators P_{ψ_i}.

Mixed states were introduced by von Neumann in his classic *Mathematical Foundations of Quantum Mechanics* as a way of integrating statistical mechanics into quantum theory. Thus the trace class operators determined by (6) are also called Density Operators, Density Matrices, or Statistical Operators. Given their provenance it has been natural to provide them with an ignorance interpretation. Thus the w_i have been taken to be epistemic probabilities and some particular w_k is the probability that the system is in the state P_{ψ_k}. Thus the system is *really* in some pure state but we do not know which (much as the ignorance interpretation of superpositions took it that a system is really in some *eigenstate* but we did not know which).

But though density operators were introduced explicitly to represent our statistical ignorance it is by no means clear that they *can* play that rôle. The reason is that there is no unique decomposition into pure states. The canonical decomposition into orthogonal Projectors mentioned above is only unique if there is no degeneracy in the eigenvalues w_i.[9] If two eigenvalues are the same (so there is degeneracy in the spectrum)—*i.e.* if we have $w_i = w_j$ for $i \neq j$— then there will be another set of projection operators $\{P_{\varphi_i}\}$ such that

$$W = \sum_i w_i P_{\varphi_i}$$

and so that the w_i can be matched with the eigenvalues in the original decomposition. If we forget the requirement that the Projectors be mutually orthogonal then even if there no degeneracy in the canonical decomposition there will be an infinite number of decompositions into pure states. Thus *if we have no reason to prefer an orthogonal decomposition there will* always *be an infinite number of ways to decompose a mixed state into pure states*. This means that we cannot consider the weights as simple probabilities or we will have to sum the weights over all the possible sets of pure states and the result will be infinite.[10]

the more recent article by G. Cassinelli, E. De Vito and A Levrero, 'On the Decompositions of a Quantum State' in *J. Math. Analysis and Appl.* 210 (1997) 472–483

[9]Since W is a trace class operator its eigenvalues are given by the w_i of the canonical decomposition. These eigenvalues are independent of the orthonormal base.

[10]In this case a defender of the ignorance interpretation would have to maintain that the probabilities are conditional probabilities, conditional on making a restriction to some particular decomposition of pure states. But it would still be unclear which of the infinite number of pure states is the real one.

When we come to interactions and tensor product spaces this non-uniqueness of decompositions of mixtures manifests itself in new ways. It also has a great deal of significance for the measurement problem.

Given a composite system $A + B$ defined on $\mathcal{H}_A \otimes \mathcal{H}_B$ in a pure state, the two factors A and B will often be in mixed states W_A and W_B and if they are then they are not sufficient to determine the state of $A + B$. This is often expressed by saying that the state of $A + B$ does not supervene on the states of its components. It is a whole greater than the sum of its parts.

One implication of this is that the ignorance interpretation of mixtures cannot be maintained. For if we were to have a decomposition of $W_A = aP_{\psi_1} + (1-a)P_{\psi_2}$ and of $W_B = bP_{\varphi_1} + (1-b)P_{\varphi_2}$ then if the ignorance interpretation were correct A would be in either P_{ψ_1} or P_{ψ_2} and B would be in either P_{φ_1} or P_{φ_2}. There are four possible combinations here, where the probabilities are multiplied together (because the possibilities are conjoined). The four are

$$ab,\ a(1-b),\ (1-a)b,\ (1-a)(1-b).$$

These four probabilities will sum to 1 and thus the composite system will be a mixed state not a pure state, contrary to our assumption that the composite state was pure.[11]

This merely reinforces a general point. The non-uniqueness of the decomposition of mixtures is a quantum phenomenon that follows from the superposition principle. The ignorance interpretation of mixtures cannot be maintained for mathematical reasons having to do with the way that the convex set of states—*i.e.* the states both pure and mixed—emerge from the Hilbert space structure and the tensor products of same.[12]

Indeed the failure of the ignorance interpretation could be said to be confirmed empirically by the violation of the Bell Inequalities in the experimental tests of the EPR situation. It was noted above that a pure composite system determines, but may not be determined by, the mixed states of its components. The correlations that occur in the EPR experiment are then encoded in the pure state of the composite but not in the mixed states of the constituent systems. This explains the non-locality inherent in the situation. The states of the component particles are local but the state of the composite is non-local. To confirm that this is so through experiment vindicates the notion that the convex set of density operators is the basic ontological structure of quantum theory. For what would happen if the ignorance interpretation of mixtures was correct and only the pure states were genuine? Then the component

[11]The idea of this proof is found in a number of places. The version that occurs here is taken from R.I.G. Hughes' *The Structure and Interpretation of Quantum Mechanics* Harvard University Press, 1989.

[12]Technically, the pure states are the convex hull of the set of total states. The pure states are also known as the vector states—the familiar one-dimensional subspaces of the Hilbert space.

states would have to be pure (only we not *know* which pure state the system is in) and the composite would merely supervene on the components, and the probabilities would not come out right. Thus the confirmation of the violation of the Bell Inequalities is a confirmation not just of the falsity of the ignorance interpretation of superpositions, and thus of the falsity of Hidden Variables, but also a confirmation of the falsity of the ignorance interpretation of mixtures.

The pedagogical implications of this are worth emphasising. Students in physics classes are regularly taught that Aspect's testing of the Bell Inequalities show that Local Hidden Variable Theories are ruled out by experiment, and this is undoubtedly a valuable thing to teach. But students rarely ever learn how quantum mechanics manages to do any better. And for those physics instructors who adhere to the ignorance interpretation it is not clear that an explanation is possible, for where in the pure states of individual systems is the non-local information about the composite to be stored? In the same vein it is not useful to start students off with the claim that states are represented by rays, or vectors, in a Hilbert Space. This means that mixed states are, by instructional fiat, taken not to be genuine states. If an instructor wishes to argue this, then so be it; but it is a point that must be argued; it can no longer simply be assumed.

The oddity of the failure of the ignorance interpretation of mixtures is easily overlooked. Von Neumann introduced density operators precisely to represent our statistical ignorance. The mathematical entities that he used turn out not to fit that purpose at all. But—strange miracle!—instead they do something else: they represent a new kind of state. And that new kind of state turns out to have the most profound implications for our understanding of nature.[13]

We close this discussion of mixtures with some remarks on their relevance for the measurement problem.

We saw in section 1 that there are essentially two problems with measurement: one is to get the apparatus in the right state (ideally an eigenstate) after the measurement; the second is to get the probabilities of the outcomes to come out right. It has often been assumed that the way to achieve these outcomes would be to have the composite system at the end in a mixed state—where that would represent our ordinary classical ignorance of its state before we look. It is then usual to note that the interaction will not produce a mixture with the right probabilities, and this has become the basis to the claim—often made—that the measurement problem is insoluble.[14] However Beltrametti and Cassinelli have argued that the problem only

[13]We might wonder in this context: what *does* represent genuine statistical ignorance? A mixture of mixed states?—and may that not have surprising quantum aspects too? But if not that, then what? Surely ignorance cannot be that hard to attain(!)

[14]See for example Harvey Brown's 'The Insolubility Proof of the Measurement Problem' *Foundations of Physics*, 16, (1986) pp. 857–870.

arises if we ignore the superselection rules that forbid certain superpositions. As we move up from the Plank length toward mesoscopic and macroscopic systems these superselection rules should have a cumulative effect on the quantum behaviour of an object—flattening it out in the classical direction. This would explain why quantum effects were only noticeable in microscopic systems. They suggest that if the measurement apparatus approximates to a classical system then the superposition that we saw in section 1 will be replaceable by a mixed state with a unique decomposition into eigenvectors. Thus, for Schrödinger's Cat we really have a mixture $\frac{1}{2}(\Uparrow \text{ and } \mathsf{cat\ alive}) + \frac{1}{2}(\Downarrow \text{ and } \mathsf{cat\ dead})$. However, though there is no denying that this is in accord with experience, and though it is plausible that superselection rules should produce convergence to an apparatus that *appears* classical, it is also not fully satisfactory to simply insert the correct answer. Until we have a complete quantum theory (with superselection rules) of a requisite measurement apparatus, with or without a cat, it is idle to speculate on whether quantum theory can or cannot give a proper account of quantum measurements. But note that if Beltrametti and Cassinelli are correct then we have no need to postulate a collapse of the wave packet because ordinary interaction with a measurement device would suffice. It remains to be seen, however, whether this would solve the problems created by our negative-outcome measurements, described in section 2.[15]

5 Realism or Idealism?

Where does the failure of the ignorance interpretation of mixtures leave our picture of reality? Does quantum mechanics still appear to support a metaphysical Idealism, in which it is possible to say that "the moon is definitely not there when we are not looking at it"?

If we think that Idealism is simply the denial of Realism and that the latter is to be identified with a Hidden Variable view then we will undoubtedly draw the conclusion that Idealism has triumphed. The idea that every system is always in an eigenstate of every observable is no longer tenable. But should this be what Realism means? Even when a quantum system is not in an eigenstate it is still in a unique, well-defined, state and that state is the perfectly objective, mind-independent, aspect of the external world that explains why we have the experimental results that we have. If anything deserves to be thought of as a primary quality of matter it is surely this.

Indeed in many respects quantum mechanics is more Realist than classical me-

[15]It is also clear that we would *not* want a quantum mixed state, but rather a *classical* mixed state. If the mixed state above were quantual then since it has degenerate eigenvalues it would have an infinite number of decompositions.

chanics. In classical mechanics the statistical operator could properly be described as subjective: it reflects our lack of knowledge of the true composition of the system. But this no longer holds in quantum theory: instead, as we've seen, the density operators describe perfectly objective states of systems in which all possible decompositions are in a sense present at once, albeit latently. Quantum mechanics is thus a move against the subjectivising tendencies of Idealism, making us realise that the world has more structure than we previously thought, not less. The explanatory power of these states is clear from the EPR situation. But we do not have to go that far, for we can see the same primacy in the Pauli Exclusion Principle: the state of one Fermion "forbids" another from also having that state.

The historical alignment between the Copenhagen Interpretation and Idealism has meant that this point has been slow to be received.[16] Idealists believe that the world is ontologically impoverished—stripped bare of the structure that is needed to explain our experience. Instead they place all explanatory structures "in the mind" where they think that they are more effective. Thus Kant, for example, moves all of the familiar features of objects: their size, shape, mass, location, causal relations, *etc.*, into the mind as structural constraints on the contents of consciousness, leaving the world itself—the noumenal world—as not merely unknowable, but as having no features that we might cling to in thinking about it. (In a strange manœuvre Idealists usually then perform a *second* inversion, in which they redub the internal phenomenal world 'the world'—making it seem that their view is not the audacious denial of common sense that it is.[17])

The original impulse behind Idealism appears to have been the desire to make the objects of knowledge secure from scepticism by eliminating any external gap between knower and known. But the fallacious arguments that were deployed for that end have had a life of their own and have themselves become a significant force for scepticism. The banner cry of such a view is the claim that "Everything is subjective". The Copenhagen Interpretation fell into the same pattern of realignment that had been set by Kant: '*There is no quantum world*,' said Bohr. 'There is only an abstract quantum description.' What confirms this abstract quantum description is therefore not objective events in the real world but features of our experience—with no account possible of why we have the experiences that we do.[18] Heisenberg

[16]Indeed the Copenhagen Interpretation has probably been responsible for slowing our appreciation of the falsity of the ignorance interpretation of mixed states.

[17]This strategy was already deployed with bravura audacity by Berkeley in the opening pages of *The Principles of Human Knowledge.*

[18]Note that Bohr (like Kant) is unable to say that they are caused in us by the we-know-not-what noumenal world since, for him, there either is no *causality*, or it is banished inward with the rest of the ontology. But the problem of why we have the experiences that we do—and what is this *we* that has them?—is a problem that Idealists have fled from since Berkeley's own unembarrassed answer.

is similarly emphatic in his Kantianism: 'The hope that new experiments will lead us back to objective events in time and space is about as well-founded as the hope of discovering the end of the world in the unexplored regions of the Antarctic.' Thus, for Heisenberg, realism is equivalent to flat-Earthism!

As a result of these emphatic declarations physicists often have a schizophrenic attitude to reality and realism. On one side they know perfectly well that the particles that they observe in cloud chambers and create in accelerators are real—they are not mental entities, nor mental or social constructs. Moreover they know that the machines that they have built to make their observations, and indeed the bodies that they have (or are), are likewise real and are indeed composed of those same particles. But—and here is where the schizophrenia lies—they have also been schooled in the Copenhagen interpretation to claim the exact opposite: to believe that elementary particles are not in space and time at all; that indeed they don't really exist. And they are forced into this position because they falsely believe that Copenhagenism triumphed over Einstienian Realism in the 1930s—whereas, in reality, both positions lost, for entirely different reasons. Younger physicists then learn this confused view, patching into it scraps of philosophical excess picked up from the usual unreliable sources.

The fault, however, does not lie *entirely* with Idealism—for there is an ambiguity in the term 'realism' that has been exploited to create misunderstandings where there need be none. In the first sense, to be a realist about quantum mechanics is simply to think that we should believe in the entities and structures that subserve its explanatory hypotheses. Put simply, belief goes along with explanatory success.[19] On the other meaning of 'realism' to be a realist is to believe that Classical states exhaust the set of total states that a system might have—and therefore must be possessed by a system at all times. In short, classical states could not be dispositional. Quantum Mechanics only casts doubt on realism in this latter sense. Note, however, that the denial of this latter view does not automatically take one to Idealism but rather to realism in this first sense![20]

In sum: the quantum state is an objective part of the real world; it may well even evolve in a purely deterministic fashion; the particles to which it belongs are capable of interacting causally and when they do their quantum states can become entangled—this entanglement is also something entirely objective. The quantum state gives rise to the qualities that we observe, but, contrary to the tendencies within Idealism, that state itself is far richer than the small window of observation is able to reveal. Our best reason for being realists about this state is that it is our only

[19]And must be tempered by explanatory failure.

[20]Note, for parity, that when we speak of realism in the context of space-time theories we never think that to be a realist is to be a Newtonian! Rather we think that it is to take seriously the rôle of space-time and its associated tensor fields within General Relativity.

means of explaining the phenomena that are peculiar to quantum theory.

As I write, there is a light breeze stirring at some trees. Crows call invisibly, far off, and a winter sun streams through a break in some branches. All of these things are real. Quantum mechanics has not robbed them of that reality, rather it has made it plain that they are the manifestation of something with truly unforseen complexity. Reality is not less than we thought—it is very much more.

Appendix: Kuhnian Ruminations

In his famous *The Structure of Scientific Revolutions* T.S. Kuhn attempted to present a general theory of how scientists behave in the transition from one scientific theory to another. He oulined an account of, what we might think of as, *cognitive pathology*, as scientists accomodate the negative results that accrue to an old theory and make the move to a new one. As an attempt at a sociological and psychological theory this was perhaps a worthy enterprise—the more so when it is detached from Kuhn's nugatory philosophical opinions—and yet, in Kuhn's hands it was also strangely limited.

The reason it is limited is not far to seek: Kuhn had only two clear examples of large-scale theoretical change (what he calls a "paradigm shift") to generalise from: the Copernican Revolution and the Einsteinian Revolution. Two cases are not a lot to make a generalisation from—something to which the inductive probability-averse Kuhn should have been more sensitive.

If we were to look at the long crisis that has been engendered by quantum mechanics we would find that the cognitive pathology associated with it is far more difficult to analyse than Kuhn's favourite exemplars. The Copenhagen Interpretation established itself as the dominant view by offering two completely contradictory responses to classical physics. One was to deride it, and the philosophical realism that was associated with it, as passé and superceded—only adhered to by those too old or hide-bound to make the transition. The second was to keep classical physics as the appropriate description of macroscopic objects while giving a purely instrumentalist account of the microscopic realm. This strange strategy allowed adherents of the theory to advertise themselves as progressive for adopting quantum mechanics without reservation at the micro-level, while giving crude and often very non-quantum-like descriptions of idealised measurements drawn largely from classical mechanics. Thus we have the uncomfortable sight of Heisenberg and Bohr making dizzying philosophical pronouncements on the non-existence of material objects while attempting to explain-away deeply intriguing features of quantum theory with experimental descriptions that would not have been out of place in Descartes' time.

In retrospect it is not difficult to see why Kuhn failed to include the reception of the Copenhagen Interpretation as an instance of cognitive pathology. To see it in this way—that is, as an awkward transition between classical physics and a full embrace of quantum mechanics on its own terms—would have required Kuhn to detach himself from the philosophical views (the Relativism, the Instrumentalism, the Pragmatism) that he shared with Copenhagenists.[21]

Indeed that, I suggest, is the right way to see Kuhn's work: that it is not a study of cognitive pathology engendered by crisis; it is rather an instance of said pathology, engendered specifically by the crisis within quantum mechanics. What Kuhn was doing was really Copenhagen Sociology.

[21]It is worth adding that Kuhn argues for the thesis that he is trying to establish very oddly. Given that he is attempting to establish that scientists become irrational at moments of profound scientific change there is little or no evidence drawn from the notebooks, diaries, or autobiographies of actual scientists in such circumstances. Rather, Kuhn presents a set of philosophical claims and argues on that basis that scientists *must* be irrational at such moments. Oddly though, the only real support that these philosophical claims have is that they must be true if the alleged crises occur at all.

My own guess is that if and when profound changes occur in science the behaviour of scientists will depend entirely on the specifics of the situation, and cannot be generalised over. The response to relativity theory, for example, had little in common with the response to quantum mechanics even though many of the individuals were the same in the two cases. Also: I am not as convinced as Kuhn that the majority of scientists *will* behave irrationally, but if they do there is no reason to think that there can be a theory of this. It may be the mark of the irrational that no two manifestations are exactly alike—thus spoiling any idea of having a grand theory to cover all.