THE BOHM APPROACH RE-ASSESSED.

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I have been rethinking the Bohm interpretation (BI) over the last couple of years. This has led me to re-examine its relationship to the standard approach (SQM). This short note based on a letter I wrote to Roger Penrose summarising my latest views.

1. According to the conventional view, we are not supposed to be able to construct a $x - p$ phase space in SQM, yet Bohm does just that. How does he get away with it, while remaining within the quantum formalism and reproducing the results of the standard formalism? Bohm uses the x-representation, that is, he starts with $\psi(x,t)$, so it is OK to use the position co-ordinate, what then is the P_B he uses in the so-called 'guidance condition' from which one calculates the 'trajectories'?

It is clearly not an eigenvalue of operator $\hat{P}$ since $\psi(x,t)$ is not an eigenfunction of $\hat{P}$. The 'no go' theorems of von Neumann, Kochen and Specker, Gleason, Penrose and others have shown that this is not possible. So BI is not using the eigenvalues of the so-called dynamical operators to label all the properties of the particle. OK so it is violating the usual, often implicit, assumption that all we can talk about are the eigenvalues of these operators, which we choose to call 'observables'. What the BI suggests is that we can a get a lot of interesting insights as to how particles could be behaving if we drop this assumption and introduce the notion of 'beables' and distinguish them from 'observables'.

Objection 1. We must only talk about things we can measure. We must only talk about observables. OK then why do we talk about the wave function? I do not know how to measure the wave function directly.

Objection 2. The uncertainty principle implies that particles cannot simultaneous have values of x and p. Wrong, what the uncertainty principle claims is that we cannot *measure* x and p simultaneously. If we cannot perform the measurements, then we cannot say for sure a particle can have x and p together but equally we cannot say for sure that they *do not have* values for x and p simultaneously.

The proposal Bohm and I made in our book, “The Undivided Universe” [1] was “Let us assume the particle travels with the beable momentum $P_B = \nabla S$ and see what we get.” What we find is that

(i) the interpretation produces results that agree with SQM and experiment. This should be of no surprise since we use the Schrödinger equation, but simply analyse its real and imaginary parts separately.

(ii) we get an intuitive picture for what may be going on at the level of individual particles.

(iii) we can show that in setting up the model, an extra ‘quantum potential’ term appears. Contrary to some would have us believe, this term is *not* added, it is already present in the real part of the Schrödinger equation. We have simply explored what the potential looks like in different situations and tried to understand its significance. We find this potential is responsible for all quantum phenomena including the non-local behaviour found in entangled states. As this quantum potential becomes smaller and smaller, the quantum ‘trajectories’ smoothly approach the classical trajectories as the QP tends to zero as I showed in one of my papers [2]. We can also show that in the classical limit, the non-locality disappears leaving us with a local classical world.

Polkinghorne [3] has made a different objection. He claims we assume the Schrödinger equation, and goes on to ask, “Where does that come from?” He continues by claiming that we have “pulled it out of thin air” but then adds “rather it came out of Schrödingers head”. For some reason, which is not clear to me, this is levelled against BI and not SQM. Yet in teaching the conventional approach we always assume the Schrödinger equation. However leaving that aside, I decided to look again at the origins of the Schrödinger equation. Schrödinger was well aware that his own ‘derivation’ was far from satisfactory and was, in fact not sound, writing in a foornote “I am aware this step is not unambiguous” [4].

However Guillemin and Sternberg [5], amongst others, have shown that for Hamiltonians up to quadratic in x and p, the Schrödinger equation can be rigorously derived from classical Hamiltonian flows provided we go to the covering group of the symplectic group, namely, the metaplectic group. However the restriction to quadratic Hamiltonians is severe and physics has more general Hamiltonians. Recently Maurice de Gosson [6] has shown that you can go to a larger covering group, $Ham(2n)$, and again show rigorously that the Schrödinger equation lives in covering space. As de Gosson puts it “This theorem is essential, both from a mathematical and physical point of view, because it makes clearer that the generating function, that is ultimately the classical flow determined by H, suffices to determine the wave function."

What he also shows is that if you project from a flow in this larger covering space back down onto the symplectic space, you find that the quantum potential appears. What seems to be happening here is that the BI results from trying to force quantum processes into an inappropriate space. It doesnt mean you cant do it, you can, but you need to introduce a form of 'compensating' field to get it to work. I immediately thought about the analogy with GR where the gravitational force arises from the projection into a flat Minkowski space-time. However there are major differences. The biggest of these is that gravity is universal, whereas the QP is restricted only to a group of particles in the same entangled state, but nevertheless the spirit is the same.

2. Could the BI be a result of non-commutative geometry where we know that if we take the non-commutative structure as basic, all you can find are 'shadow' manifolds underlying the structure? Here I am thinking of Gel'fand's work [7]. This shows that we can either start from a *a priori* given spacetime and construct an algebra of dynamical functions or we can start with the dynamical algebra and deduce the metric and topological properties of the underlying space. If the algebra is commutative, we find a unique underlying space but for a non-commutative algebraic structure, the best you can do is to find sets of shadow manifolds.

This led to another seemingly different question. We know the Schrödinger picture is isomorphic to the Heisenberg picture for a finite number of degrees of freedom, where then is the quantum potential in the Heisenberg picture or rather in 'matrix mechanics'? I call this the 'algebraic approach' where we do not give primary relevance to Hilbert space. This is part of a more general approach that is outlined in Haag's book, "Local Quantum Physics" [8]. Here Haag embeds the algebras in Minkowski space-time. My idea is different and ultimately I want the non-commutative structure to determine the structure of space-time itself in some suitable limit. This I feel would be the 'true' quantum gravity, but as always the difficult question is how to do it!

I felt I should start with a much more modest aim of trying to see where the quantum potential arises in the algebraic approach. It is a long story and I will spare you many of the details. It essentially meant replacing the wave functions by elements of the minimal ideals in the algebra. This is difficult for the Heisenberg algebra (it is nilpotent and therefore has no primitive idempotents) and it meant I had to extend the algebra in the same way that the boson algebra is extended in quantum field theory by adding the projector to the vacuum. This enables us to find the algebraic equivalents of the Schrödinger equation. However the structure of the approach forces us to choose a special element of the algebra which is used as the description of the state of a system. This element is a primitive idempotent, which is chosen by examining the physical situation. (See Hiley [9] for more details). With this idempotent we can construct an element of the minimal left ideal, Ψ_L. Together with its Clifford conjugate, $\Psi_R = \widetilde{\Psi}_L$, we can form what I call the Clifford density element, $\rho_{\text{cliff}} = \Psi_L \Psi_R$. The reason for the choice of name is that it plays a role similar to that of the density operator in SQM. Since in this form it is idempotent signifying a pure state, i.e.$\rho^2_{\text{cliff}} = \rho_{\text{cliff}}$. Of course, we can find a more general element which will correspond to a mixed state, but for simplicity we will limit our discussion here to pure states. We then find two equations describing its time evolution

$$i\partial_t \rho + [\rho, H]_- = 0$$

$$\Psi_L \overleftrightarrow{\partial}_t \Psi_R + [\rho, H]_+ = 0$$

where $[\rho, H]_\pm = \Psi_L \overleftarrow{H} \Psi_R \pm \Psi_L \overrightarrow{H} \Psi_R$. Here H is the Hamiltonian expressed in terms of elements of the algebra, which means these equations describe the time evolution of elements in the algebra. Notice our brackets play the role of the commutator and the anti-commutator in SQM.

The first equation is very familiar. It is simply the algebraic form of the Liouville equation. I have not seen the second equation in the literature anywhere before although I have seen hints of it here and there. To see the significance of this equation we must convert the elements of our algebraic ideals into elements of Hilbert space used in SQM. We then find that $\Psi_L \overleftrightarrow{\partial}_t \Psi_R = 2\rho\partial_t S$. Thus the second equation tells us how the phase develops in time. I know that in SQM, the existence of a phase operator has generated a huge literature but the phase I am using here is not a phase operator, it is simply the phase.

I am confident that there are no problems with these two equations as there exists a similar pair of equations in the deformation algebra using Moyal products [10] (see below). I get further confidence by looking at gauge invariance and finding the Aharonov-Bohm effect and the Berry phase emerge directly from this second equation in a couple of lines.

If we look at these two equations we see no quantum potential at all. However if we find an x-representation of these equations in a Hilbert space using the projector $\pi = |x\rangle\langle x|$, we immediately find the first equation become Bohm's equation for the conservation of probability. The second immediately becomes Bohm's quantum Hamilton-Jacobi equation containing the quantum potential. This then is further evidence that the BI follows by projecting from the non-commutative structure into a shadow manifold. In other words the Bohm trajectories are exactly what one would expect from a non-commutative structure.

Furthermore I can also project into the p-representation and get a BI for the momentum space. This counters the criticism originally made by Heisenberg [11] that there is no x, p symmetry in the Bohm approach. But the p-space is just another shadow manifold, which restores the $x - p$ symmetry. Thus we see that these shadow manifolds give us different views of a process that is unfolding in a non-commutative 'geometry'. For a complete discussion of these results see Brown and Hiley [12].

3. How sound are the above two equations?

As I remarked above the appearance of an equation that I had not seen in the literature before caused me some concern and on the suggestion of David Fairlie [13], I looked for a similar equation using the Moyal * product (deformation algebra). There you find two brackets, the Moyal bracket (the analogue of the commutator) and the less well know, Baker bracket (the analogue of the anti-commutator bracket, or more strictly, Jordon product). The advantage of this approach is that in the limit to order h, the Moyal bracket becomes the Poisson bracket, while the Baker bracket simply becomes the ordinary commutative product. The time dependence of the Wigner distribution can then be expressed in terms of two equations that are the exact analogue of the two equations above.

The one containing the Moyal bracket becomes the classical Liouville equation in the limit, while the one containing the Baker bracket becomes the classical Hamilton-Jacobi equation. So it all fits beautifully.

4. How does all this fit into the principle of equivalence, the problem of quantising the gravitational field?

I have not done enough work on this question but let me make a start. One of the reasons I took the Bohm approach seriously was because it had in it the notion of a 'trajectory', or in the terms used by those working in relativity, a 'geodesic'. The key question then is what are we to make of these trajectories or geodesics? If we assume that there are quantum particles and they follow these trajectories then we certainly will not get schizophrenic cats or any of the other puzzles SQM throws up. This throws into question Penrose's assumption that a definite result occurs because of the tension between rival space-times arising from each wave packet.

All of this is because the BI does not have a collapse problem. Take Schrödinger's cat. Whether the cat lives or dies depends on the position of the trigger particle in its 'wave packet'. If the particle is in the front of the packet, the cat dies. If it is in the rear, it lives. The problem is that we cannot experimentally control where the particle is going to end up in the initial 'wave packet'. Therefore our probabilities arise from our inability to control the initial x and p of the trigger particle, so the statistics arise from the contingent initial conditions, not because the evolution is 'woolly'.

All of this brings us back to the central question, "What exactly is the Bohm energy-momentum that plays a central role in BI?" Recall that in the original approach, the Bohm energy-momentum, ($E_B = -\partial_t S$ and $P_B = \nabla S$) is normally presented as arising from a comparison with the classical Hamilton-Jacobi equation. In this comparison we have to replace the classical action with the phase of the wave function. Bohm gave no justification for the replacement. I have always felt uneasy with this step.

I have recently made a remarkable discovery, so obvious that I am surprised we didn't spot it sooner. The Bohm energy-momentum relations can be obtained directly from the *energy-momentum tensor* of standard quantum field theory (QFT). Let me show you how this is happens.

For a particle described by a Lagrangian, $L(\psi, \partial_\mu\psi)$ the energy-momentum tensor is

$$2iT^{\mu\nu} = \bar{\psi}\gamma^\mu(\partial^\nu\psi) - (\partial^\nu\bar{\psi})\gamma^\mu\psi = \bar{\psi}\gamma^\mu\overleftrightarrow{\partial}^\nu\psi \tag{1}$$

For the Schrödinger particle, the energy and momentum part of this tensor becomes

$$2iT^{k0} = \psi^*(\partial^k\psi) - \psi(\partial^k\psi^*)$$

If we use $\psi = Re^{iS}$ in this equation, we find immediately $E_B = -\partial_t S$ and $P_B = \nabla S$. By using eqn (1) we can also obtain expressions for the Bohm energy-momentum of the Pauli and Dirac particles [14] [15]. Thus we have the general rule

$$\rho P_B^\mu = T^{\mu 0}$$

We can now ask whether conventional QFT uses P_B^μ or not. It actually does but rather than treat it as an entity in its own right, it forms

$$P^\mu = \int T^{\mu 0} d^3x.$$

and then shows that

$$\frac{d}{dx^0}\int T^{\mu 0} d^3x = 0 \quad \forall \mu.$$

In other words conventional QFT integrates over all space so is considering only the *global* energy-momentum. What it then shows is that the total energy-momentum four-vactor is conserved. Thus any non-locality in energy would not be seen by conventional QFT because it considers only *global* properties. BI on the other hand considers *local* properties. However when the local properties are considered, P^{μ}_{B} does not contain all the energy. This is why the quantum potential is needed. It describes the 'missing' energy-momentum. It is quite clear that energy is missing because we do not use the whole of the energy-momentum tensor. We have left out T^{jk}, which if we were describing a fluid, would represent the stress energy in the medium.

How are we to make sense of this situation? I suggest that we follow Bohm and consider the energy-momentum P^{μ}_{B} as being the energy-momentum associated with the core of some extended process, the core being treated as the Bohm particle. This is in complete agreement with what Bohm himself had in mind. In his book "Causality and Chance" [16] he writes,

> Finally our model [*sic.* the original 1952 proposal] in which wave and particle are regarded as basically different entities, which interact in a way that is not essential to their modes of being, does not seem very plausible. The fact that wave and particle are never found separately suggests that they are both different aspects of some fundamentally new kind of entity which is likely to be quite different from a simple wave or a simple particle, but which leads to these two limiting manifestations as approximations that are valid under appropriate conditions.

To motivate further what we have in mind, let us assume that we can model the vacuum state on some kind of 'lattice' structure as Bohm [17] has been suggested elsewhere. Then the particle could be regarded as some kind of dislocation within this lattice structure. In this case we can associate an energy-momentum with the effect of this dislocation on the lattice energy and the quantum potential would then be associated with the stress in the lattice. This is the type of model discussed originally by Frank [18] for a different purpose. In our case the stress is related, in some way, to the presence of the apparatus. In other words we are giving a specific reason for Bohr's insistance that quantum phenomena cannot be separated from the experimental conditions that give rise to specific experimental results. Of course, I am not suggesting that the vacuum state literally has the structure of a lattice, rather the particle has a central core with a surrounding field. This whole structure cannot be reduced to a point-like or 'rock-like' entity except in some limit where classical physics takes over.

I am suggesting it may be some form of extended process or a 'quantum blob' of the type that has been suggested by de Gosson [6]. But even in this case one can think of the Bohm P_B^μ as in some way parameterising the individual processes, thus reducing the 'wooliness' of the standard description and avoiding the collapse problem of SQM.

In the view I am presenting here, there are none of the usual problems that arise from linear superposition. All the linear superposition is encoded in the quantum potential.

5. Does the BI contain the classical limit without the need for adding new physics?

The new physics I am thinking here is either decoherence or the suggestion that Penrose [19] has made that we have to bring in the non-lineararity of general relativity to collapse the wave function. In BI there is no need for new physics for reasons that I will now try to explain,

In a linear superposition, we know that, in BI, when wave packets become spatially separated, only the wave-packet where the particle actually is contributes to the QP acting on the particle. As long as the wave functions continue without overlapping, the interference disappears. However if the wave-packets overlap again, the interference re-appears. In some situations such as the Mach-Zender interferometer we need this possibility. However in other situations the interference ceases completely. In these case we need to ensure we have some way of 'killing off' the effects of the 'empty' wave-packets for 'ever'. We spent a whole chapter in "The Undivided Universe" discussing this problem. We concluded that this would happen, provided you allowed some coupling to the environment. The coupling that we propose is different from the coupling that is assumed in the usual discussions of decoherence.

Our argument is that if the particle interacted with the environment, it would produce further separated wave-packets in which only one would be occupied. This scattered particle would then collide with an other particle which would be scattered, occupying yet another separating wave packet and so on. Interference would only return if at least one each of the empty scattered wave packets could be returned to overlap. Since the scattering of the environmental particles was random, the chances of this happening would be remote. Thus the QP would never return spontaneously to its original form and interference would not reappear. Of course it may be possible, in principle, to exactly reverse everything but such a possibility is very hard to achieve so the system will behave as if the wave packet has collapsed, but no collapse has actually taken place. This argument is supported by Anton Zeilinger's interference experiment with hot bucky-balls [20]. Thus in our view, it is not new physics that destroys interference. It is the fact that you cannot in practice reverse all the particle positions in such a way as to restore interference.

With this background, my worry is that his proposed experiment, Penrose [21] will not be able to distinguish between the processes that I am talking about and what Penrose is hoping for. Maybe we re-examine more carefully these proposals to see if there is some instance where we can make a clear distinction.

Let us leave this question of collapse aside for now since it only makes sense in a theory which uses wave packets and the algebraic theory I am proposing does not use such packets. Rather it argues that the QP,which contains all the information contained in the wave description, arises form projecting a non-commuting process-type structure into a local description. Or to put it another way, we are going to be forced to think more clearly about the meaning of the phrase 'non-commutative geometry'. This is where I feel the real clues in our attempts to develop a quantum gravity will arise. Sorting all this out is going to be very tricky as there is already some dispute as whether we should call this non-commutative structure 'geometry'! But whatever we call this structure, there is a lot of interesting mathematical work going on in this area. For example, I have been talking with Freddy van Oystaeyen [22] and we seem to be working on a similar research programme only he suggests that we should be talking about non-commutative topology. Certainly these ideas seem to be more basic than the richer structure of non-commuting algebras.

I am very attracted to the idea that physical processes should be used to determine the structure of space-time itself, rather than assuming we know what these structures are to begin with and working with a fixed geometry. Euclidean geometry is constructed using rigid rods, Minkowski geometry is constructed using light rays reflected off stationary points in a way that is clearly brought out in the Bondi's k-calculus [23]. If we take the phase of the light being reflected off moving points into account, a generalisation of the k-calculus can be shown to link up directly with the Wigner distribution [24]. In other words the statistical properties of the Heisenberg group seem to play a role in determining the kinematic structure of these spaces. What we dont have clear is how the phase properties are related to curvature. We know that gauge groups generate curvature, but GR cannot be successfully derived from a gauge theory as far I as know. Bohm [17] did make an attempt to regard GR as a gauge theory using translational symmetry and showed how this could be tied up with twistors. Maybe this is something worth looking at. Time will tell.

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