From the Heisenberg Picture to Bohm: a New Perspective on Active Information and its relation to Shannon Information.

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Abstract.

In this paper we discuss some of the background to the notion of active information introduced by Bohm and Hiley to account for quantum processes. To appreciate the full significance of this new notion, we show why it is essential to distinguish our approach from the approach that goes under the name 'Bohmian mechanics'. We then show for the first time how the quantum potential emerges from the Heisenberg picture thus providing a new perspective to the whole approach. This enables us to clarify the role of the energy associated with the quantum potential and the status of active information. We conclude with some remarks on the relation between active information and Shannon information.

1. Introduction.

I would like to take this opportunity to explain my attitude to Bohm's approach to quantum theory. The reactions to the proposals over the years have always puzzled me. Those who find it unacceptable nearly always become rather passionate and use arguments that I find far from compelling. Equally some supporters of the approach, no doubt in reaction, tend to over exaggerate its claims. Somehow the middle way proposed by Bohm and myself seems to have been missed (Bohm and Hiley 1987, 1993 and Hiley 1987)

First let me take a typical example of the position adopted by those who feel the approach has no merits. Consider this quotation taken from Zeh (1998). "It is pointed out that Bohm's quantum mechanics is successful only because it keeps Schrödinger's (exact) wave mechanics unchanged, while the rest of it is observationally meaningless and solely based on classical prejudice". These are undoubtedly strong words backed by equally strong feelings. So how do we answer such criticisms?

Let me first explain why I got interested in the Bohm approach. I had been brought up on standard quantum mechanics and I was asked to that 'particles sometimes behaved like waves and sometimes like particles'. There was a 'measurement' problem; there were schizophrenic cats, which I am told makes Stephan Hawking reach for his gun! (exactly who/what he intends to shoot is not clear!) Wigner had some trouble with his friend and we were told that "no phenomenon was a phenomenon until it was an observed phenomenon". To someone who had enjoyed the tales of *Alice in Wonderland*, this was great stuff but rational physics---?

The problem as I saw it seemed to come from the fact that we have to uses totally different mathematical structures to describe classical and quantum phenomena. If only it was possible to bring the two descriptions to a common form, then may be, just may be we could begin to see the exact differences between the two domains. It seemed to me that Bohm's way of dealing with the Schrödinger equation by splitting it into its real and imaginary parts under polar decomposition of the wave function offers us just such a possibility, particularly as the imaginary part gives the conservation of probability, while the real part had a striking resemblance to the classical Hamilton-Jacobi equation. Admittedly we could have gone the other way as some have done and re-formulate classical physics in the Hilbert space formalism, but I preferred to explore the former. Could the re-appearance of Hamilton-Jacobi equation be a coincidence? Of course it could be, but then we must remember that Schrödinger's original 'derivation' of his equation started by *assuming* the classical Hamilton-Jacobi equation (Schrödinger 1926). Clearly there had to be a connection.

I felt that what was now called for was an exploration of this formalism without taking on board any pre-assumed metaphysical baggage and certainly without resorting to classical prejudices. But equally I realised that we must also leave behind some of our quantum prejudices. Not an easy thing to do because both are deeply ingrained by now, but surely the important question is whether by looking at things from a different perspective we can get any new insights into quantum phenomena.

In order to provide some more background to my discussion, I would like to start from a more general perspective. Once we accept the Born probability postulate, and I have no reason to doubt its validity, the wave function must be interpreted in terms of possibilities or, better still, in terms of *potentialities*. Now it is well known that Heisenberg (1959a) favoured the use of potentialities. What is well less known is that Bohm (1951) also proposed that the wave function should be thought of in terms of potentialities. Bohm argued that the potentialities were latent in the particle and that they could only be brought out more fully through interaction with a classical measuring apparatus.

This of course is essentially the conventional view, so why did Bohm bother to make alternative proposals? It was the complete absence of any account of *the actual* that troubled him. In the quantum formalism nothing seemed to happen unless and until there was an interaction with a measuring apparatus. There was no actualisation until some form of instrument was triggered. Surely something triggered the instrument? Why was the measuring instrument so different? Isn't it just another collection of physical processes governed by the same laws of physics? What the Bohm approach seemed to offer was a way of providing an account of an actual process underpinning the observational results. The nature of this actual process is not clear, but surely searching for an actual underlying process is not pandering to classical prejudice? Indeed the driving force behind the present search for a generalised quantum mechanics for gravity is a search for an ontology (see for example Hartle 1992)

There is a second feature in Zeh's (1998) criticism that I find curious. There have been frequent arguments that because the 'trajectories' appearing in the Bohm approach are 'unobservable' they are meaningless. Yet in quantum mechanics the wave function is 'unobservable' but I never hear anybody calling it meaningless. The wave function, according to Bohr, is simply a term in an algorithm from which the probable outcome of any given experiment can be calculated. As such it does not say anything about how a particle leaving a source arrives at the detecting instrument. In Miller and Wheeler (1983) a quantum process is likened it to a 'smoky dragon'.

What the Bohm trajectory does is to provide an explanation of how an ensemble of individual particles *could* travel and arrive at the detecting instrument so as to account for the observed probability distribution. Do we need this kind of information? I, in common with many others certainly do. It helps to provide an understand the kind of underlying process that could produce the observed probability distributions without Alice-type stories. Do the particles actually travel along such paths? I don't know but in the absence any information to the contrary I am quite happy to imagine they do. As is well known we cannot 'see' the particles travelling along a trajectory, hence Zeh's criticism. But equally can we claim that it does *not* travel along a trajectory? I feel that it is better to adopt a position that understands the particle to travel along such a trajectory, unless it leads to some contradiction, than to confess that one has no idea how particles get from A to B. But for me there is a more fundamental question, do localised classical-type particles actually exist at the quantum level or is the quantum behaviour a pointer to subtler forms of motion that have more to do with *process* than with particles/fields in motion in space-time? Because of space limitations I will not comment on this suggestion further. More details can be found in Brown and Hiley (2000) and Hiley (2001).

What about the strong advocates of the Bohm approach? Are they beyond reproach? Unfortunately not and their efforts has generated the label 'Bohmians'. As Chris Fuchs greeted me at the beginning of this conference, "You're a Bohmian, aren’t you?" "Well no Chris not in the sense you mean!"

I think this adjective takes its meaning from a particular view strongly advocated by Dürr, Goldstein and Zengi (1993) who have actually coined the phrase "Bohmian mechanics". Their take on the Bohm formalism is what I call *mechanistic minimalism*. That is they take a position that attempts to keep as many of the traditional features of a mechanistic view of physics as possible, introducing the minimum number of assumptions that seem necessary to generate the formalism. This gives the impression that they are trying to keep as close as possible to the notions of classical physics, even though elsewhere they claim to be developing a radically new theory. It could be that it is this particular feature to which Zeh (1998) was objecting.

The choice of the term "Bohmian mechanics" is rather unfortunate because Bohm himself did not think the quantum formalism suggested a *mechanistic* interpretation. In his classic book *Quantum Theory*, Bohm wrote under the section entitled 'The Need for a Nonmechanical Description' "This means that the term *quantum mechanics* is very much a misnomer. It should, perhaps, be called *quantum nonmechanics*" (Bohm 1951b). The appearance of his later paper (Bohm1952) did not change his position on this point. This can be clearly seen in his book *Causality and Chance* where he gave many arguments against adopting a *mechanistic* outlook in physics (Bohm 1958). Here we can already see the foundations of his later more radical ideas being formed, details of which were summarised in his book *Wholeness and the Implicate Order* (Bohm 1980). The position that he finally adopted in his original 1952 proposals can be found detailed in our book *The Undivided Universe* (Bohm and Hiley 1993).

In passing I would like to stress that the mechanistic view is not without its merits. It keeps everything simple. Indeed in its simplest form, John Bell (1987) has eloquently shown how it is possible to bring out differences between this approach and the standard approach to quantum mechanics. But Bohm was not content to leave it at that. There are strong indications that something much more radical was needed. Together we decided to explore the structure more deeply to see if it was telling us anything more. We found it was. Some of these features have already been discussed in our book "The Undivided Universe" (Bohm and Hiley1993) and in this paper I want to discuss some of these features in a new way.

2. Review of the development of the formalism.

With that brief background to the differences in outlook towards the Bohm formalism, let briefly summarise the position that Bohm and I adopted (Bohm and Hiley 1987, 1993). As Zeh (1998) correctly observes we assume that the Schrödinger equation is valid and no new mathematical terms need to be added to this equation. We also assume the Born probability postulate.

The derivation of the two equations that form the basis of the Bohm approach is very straightforward, so straightforward that I wonder what all the fuss is about! Simply write the wave function expressed in polar form,

$$\Psi(r,t) = R(r,t)\,\mathrm{exp}[S(r,t)] \qquad (1)$$

(R, S are real and $\hbar = 1$). Then the imaginary part of the Schrödinger equation becomes

$$\frac{\partial P}{\partial t} + \ .\frac{(P\ \ S)}{m} = 0 \qquad \text{with } P = R^2. \qquad (2)$$

This is just the conservation of probability equation.

The real part of the Schrödinger equation is

$$\frac{\partial S}{\partial t} + \frac{(\ \ S)^2}{2m} + Q + V = 0, \qquad \text{where } Q = -\frac{1}{2mR}\ \ ^2R \qquad (3)$$

It is this equation that first attracted Bohm's attention, mainly because of its close similarity to the classical Hamilton-Jacobi equation. If Q is zero and S the classical action, then the equation is identical to the classical equation. Now the classical canonical theory establishes the relations $\frac{\partial S_c}{\partial t} = -E$, and $S_c =$ $\boldsymbol{p}$, so that equation (3) becomes

$$E = \frac{\boldsymbol{p}^2}{2m} + V \tag{4}$$

which is just an expression for the conservation of energy. We write S_c to emphasise that this is the classical action and as such it is a generating function of the canonical transformations, which are simply elements of the symplectic group. It is this symmetry that plays a central role in dynamical theories. It generates the canonical transformations in classical physics, while in quantum mechanics it is the group of automorphisms of the Heisenberg algebra (see Folland 1989 and de Gosson 2001). I cannot emphasise strongly enough the importance of the symplectic symmetry in the approach that I am adopting.

What Bohm's original approach does now is to assume that when the action S_c is replaced by the phase of the wave function, the momentum of a particle is given by $\boldsymbol{p} =$ S, while in the stationary state, where energy is well defined, we have $\frac{\partial S}{\partial t} = -E$. In other words these canonical relations are carried over to the quantum domain. Thus equation (3) can be regarded as a generalised expression for the conservation of energy provided we regard Q as a new form of potential energy which is negligible in the classical world and is apparent only in quantum systems. This energy has traditionally been called the quantum potential energy. It should not be thought as the source of some mysterious new force to be put into the Newtonian equations of motion. Rather it appears as a necessary consequence of the symplectic symmetry. This will be brought out in the following sections.

Although I have only emphasised the symplectic symmetry here, it is actually the double cover of this group, the metaplectic group that plays a key role in quantum mechanics. This leads to a more refined approach that we will not discuss here. A beautiful account of this structure can be found in de Gosson (2001)

Let me emphasise that what we have done above is very straightforward. Equation (3) is a direct result of the Schrödinger equation. Nothing mathematically new has been added. What we then do is to look at the extra term Q and ask "What is the physical reason that Q appears and what could it possibly represent?" Is this not a standard procedure in physics?

3. The DGZ approach.

In order to avoid the confusion that I find in the literature, I want to contrast Bohm's position to that adopted by Dürr, Goldstein and Zengi (1993), the quintessential 'Bohmians'.

To see the source of the differences let me now briefly outline the assumptions made by Dürr et al (1993). Their approach also assumes the validity of the Schrödinger equation and the Born probability postulate. Rather than examining the contents of the Schrödinger in term of its real and imaginary parts, they argue that we need to add one new idea, namely, that the movement of an individual particle is described by the so called 'guidance condition'

$$p = \quad S \tag{5}$$

Here S is the phase of the wave function.

They then claim to derive equation (5) from two further basic assumptions, namely,

(a) the equation defining the motion of a particle must be a first order differential equation, and

(b) this equation must be invariant under the Galilean group.

It is over the status of this equation where the two views diverge. DGZ want to make equation (5) a fundamental equation independent of yet consistent with the Schrödinger equation. Thus they have a statistical theory with one fundamental equation of motion, namely, equation (5), Schrödinger's equation and nothing else. It is for this reason I referred to their approach as being mechanical minimalism.

Of course at one level there is no difference between these two points of view and it could be argued that I am merely quibbling over a small difference. After all both approaches use equation (5) to calculate the trajectories and both interpret the trajectories in exactly the same way, namely, that they provided a way of discussing the actual evolution of individuals in a fundamental statistical theory. But Bohm felt that in order to get a better understand of this behaviour we must go beyond the mechanistic picture. He did not feel the need to regard equation (5) as a new fundamental equation as it emerged from the real part of the Schrödinger equation (3) itself. Not only does it provide the form of equation (5), but it also provides us with the canonical relation $\frac{\partial S}{\partial t} = -E$ as well as providing a new form of energy Q which only acts in the quantum domain. This quantum potential energy must be taken seriously. Indeed without it energy would not be conserved and without understanding this energy the form of the trajectories are just another quantum mystery.

All of this is in contrast to DGZ. They feel the quantum potential is somehow artificial and should be avoided. In some sense they are implicitly following on from Heisenberg (1958) who argued that the approach needed "some strange quantum potentials introduced *ad hoc* by Bohm". There is no elaboration as to why he thought it was *ad hoc* and I continue to find it difficult to understand how a term that arises naturally from a basic equation called *ad hoc*?

DGZ continue, "From our perspective the artificiality suggested by the quantum potential is the price one pays if one insists on casting a highly non-classical theory into a classical mold (sic)" (Dürr et al 1996). And again Goldstein (1998) writes "In particular, Bohm's invocation of the "quantum potential" made his theory seem artificial and obscured its essential structure."

This last sentence indicates a particularly strong position. As seen from the above it is not necessary to "invoke" the quantum potential. It emerges from the mathematics as a term in equation (3) and we simply give it a name. Then all we do is to respond to our curiosity and explore consequence of this additional energy. Surely it is normal practice in physics to attempt to attach physical meanings to terms that arise naturally in fundamental equations.

DGZ are happy to embrace the imaginary part of the Schrödinger equation given in the form of equation (2) and give it meaning as an expression of the conservation of probability, as we do. But when it comes to the real part of the same equation, it is dismissed because it seems 'artificial'. To make such a comment implies that one has some idea of how quantum particles *should* behave, and when the trajectories do not fit this preconceived framework, it is dismissed as 'artificial' and 'obscure'. The fact that it has apparently such a bizarre behaviour is no reason for ignoring it.

DGZ are not alone in rejecting features of the approach that seem too strange. Others like Scully (1998) and Aharonov and Vaidman (1996) have used similar dismissive arguments, this time against the trajectories themselves. The trajectories are called ‘surreal’ apparently because the trajectories do not conform to what we would expect from classical grounds. We have shown in detail that their criticisms cannot be sustained (Hiley, Callaghan and Maroney 2001), but we must stress again the appearance of apparently ‘bizarre’ behaviour is no reason for ignoring or even dismissing it. Quantum phenomena are 'bizarre' when compared with our intuitive ideas based on classical theories. I personally would be very surprised if the trajectories and the quantum potential did have classical properties. Quantum theory is a "highly non-classical theory" and we should expect surprises, and we should use all available approaches to explore in exactly what ways the behaviour is non-classical.

Does taking the quantum potential lead to significantly different ways of looking at quantum phenomena? I think it does. By focusing on the quantum potential we saw the significance of quantum entanglement in an entirely new way, namely its striking implication for quantum non-locality(see Bohm and Hiley 1975). The importance of this was also recognised by Bell (1987) and led him to think about the problem, which eventually led him to derive his famous inequalities. The role of the quantum potential in all this was brought out particularly clearly in the calculations of Dewdney, Holland and Kyprianidis (1987) for the case of two coupled spin-half systems in an entangled state. Their work showed how the dramatic appearance of the non-local was mediated by the quantum potential. Normally such calculations explaining an experimental result would be greeted as an indication of the success of the theory but in this case, for reasons that I fail to understand, they are regarded as meaningless.

For some it is the appearance of non-locality that appears to further weaken the appeal of the approach in spite of experimentally confirmed violations of the Bell inequality. It is sometimes regarded as the Achilles heel of the theory. However for me, it is not its weakness but its strength. It offers a detailed explanation of the Einstein-Podolsky-Rosen paradox rather than leaving it all rather vague and mysterious. It is an explanation that enabled me to make much more sense of Bohr's answer to EPR puzzle (Hiley 1995).

To summarise this section then, let me emphasise that the differences between the advocates of Bohmian mechanics and our own approach is not about the need to have a account of the actual, but about what form this account should take. Clearly such a choice is largely decided by what each group regards as an acceptable physical explanation. There is no dispute about the form of the equations. Where Bohm and I differ from many advocates of Bohmian mechanics is the attitude we adopted to the formalism. Our long period of working with the formalism and reflecting on how it works has led us to believe that rather than a simple return to a mechanistic picture something much more subtle is involved. We tried to bring this out in our book but clearly we have not got across our message!

4. More about the quantum potential.

The fact that the quantum potential does cause difficulties even to the supporters of the Bohm interpretation has encouraged me to try to explore its origins from a different perspective. We see above that quantum potential emerges from the Schrödinger picture, but where is the quantum potential in the Heisenberg picture? After all the two pictures are equivalent provided we keep clear of quantum fields, so that something equivalent to the quantum potential must be present somewhere in the algebraic picture. Elsewhere in Monk and Hiley (1998), and Brown and Hiley (2000), I have suggested that we should take a more general point of view where the algebraic structure of the quantum formalism should be considered in some sense as more basic, while the representation is considered as a secondary feature of the theory.

Although I will not explore the general point of view outlined in those papers here, I want to see how the quantum potential emerges from the Heisenberg picture. To explore this let us start by considering the density operator as the fundamental description of state. Rather than consider the full generality offered by such a state, let us consider here for simplicity only pure states so we can write $\hat{\rho} = |\Psi\rangle\langle\Psi|$. The generalisation to the mixed state will be discussed elsewhere.

Let us consider the time derivative

$$i\partial_t\left(|\Psi\rangle\langle\Psi|\right) = i\partial_t\left(|\Psi\rangle\right)\langle\Psi| + i|\Psi\rangle\partial_t\left(\langle\Psi|\right) \qquad (6)$$

If we now use the Schrödinger equation and its conjugate

$$i\partial_t|\Psi\rangle = H|\Psi\rangle \qquad \text{and} \qquad -i\partial_t\langle\Psi| = \langle\Psi|H \qquad (7)$$

where H is the Hamiltonian, we find

$$i\partial_t\hat{\rho} = \hat{H}|\Psi\rangle\langle\Psi| - |\Psi\rangle\langle\Psi|\hat{H}$$

$$i\partial_t\hat{\rho} + \left[\hat{\rho}, \hat{H}\right]_- = 0 \qquad (8)$$

which is the Liouville equation where the time derivative is simply the commutator. This is, of course, all well known.

Now let me introduce a new idea. Let me introduce something that looks like an anti-derivation defined by

$$i\partial_t\hat{\rho}^* = -\left[i\partial_t\left(|\Psi\rangle\right)\langle\Psi| - i|\Psi\rangle\partial_t\left(\langle\Psi|\right)\right] = -\left[\hat{H}|\Psi\rangle\langle\Psi| + |\Psi\rangle\langle\Psi|\hat{H}\right]$$

The overall minus sign is chosen for convenience. Thus we can write

$$i\partial_t\hat{\rho}^* + \left[\hat{\rho},\hat{H}\right]_+ = 0 \tag{9}$$

where the second term is now the anti-commutator. Although equation (9) is formally quite acceptable, it is not clear what physical meaning, if any, can be given to the time `anti-derivative'. To explore its possible physical meaning, let us first re-call what we do with the Liouville equation. We choose a representation and form

$$i\partial_t\langle\boldsymbol{r}|\hat{\rho}|\boldsymbol{r}\rangle + \langle\boldsymbol{r}|\left[\hat{\rho},\hat{H}\right]_-|\boldsymbol{r}\rangle = 0$$

Then we write

$$\langle\boldsymbol{r}|\hat{\rho}|\boldsymbol{r}\rangle = \Psi^*(\boldsymbol{r},t)\Psi(\boldsymbol{r},t) = P(\boldsymbol{r},t) \tag{10}$$

where $P(\boldsymbol{r}, t)$ is the probability density. Then

$$i\partial_t P(\boldsymbol{r},t) + \langle\boldsymbol{r}|\left[\hat{\rho},\hat{H}\right]_-|\boldsymbol{r}\rangle = 0 \tag{11}$$

To put equation (11) into its more familiar form we choose a Hamiltonian

$$\hat{H} = \frac{\hat{p}^2}{2m} + \hat{V} \tag{12}$$

Then it is straightforward to show that in the Schrödinger representation

$$\langle\boldsymbol{r}|\left[\hat{\rho},\hat{H}\right]_-|\boldsymbol{r}\rangle = -\frac{1}{2m}\left[\left(\quad\Psi^*(\boldsymbol{r},t)\right)\Psi(\boldsymbol{r},t) - \Psi^*(\boldsymbol{r},t)\quad\Psi(\boldsymbol{r},t)\right] = i\quad.\boldsymbol{j}$$

so that we obtain the continuity equation in standard form

$$\partial_t P + \quad.\boldsymbol{j} = 0 \tag{13}$$

where $\boldsymbol{j}$ is the probability current which can be written in the form

$$\boldsymbol{j}_r = \langle \boldsymbol{r}| \;{}_p\left(\hat{\rho}\frac{\hat{p}^2}{2m}\right)|\boldsymbol{r}\rangle \tag{14}$$

Now let us follow the same procedure for the LHS of the anti-commutator equation

$$i\langle \boldsymbol{r}|\partial_t\hat{\rho}^*|\boldsymbol{r}\rangle = i\Big[\partial_t\big(\Psi^*(\boldsymbol{r},t)\big)\Psi(\boldsymbol{r},t) - \Psi^*(\boldsymbol{r},t)\partial_t\big(\Psi(\boldsymbol{r},t)\big)\Big]$$

Again write $\Psi(\boldsymbol{r},t) = R_r(\boldsymbol{r},t)\exp[iS_r(\boldsymbol{r},t)]$ so that

$$i\langle \boldsymbol{r}|\partial_t\hat{\rho}^*|\boldsymbol{r}\rangle = 2P(\boldsymbol{r},t)\partial_t S_r(\boldsymbol{r},t)$$

Thus we obtain

$$P(\boldsymbol{r},t)\partial_t S(\boldsymbol{r},t) + \tfrac{1}{2}\langle \boldsymbol{r}|\big[\hat{\rho},\hat{H}\big]_+|\boldsymbol{r}\rangle = 0 \tag{15}$$

Thus the gives us an equation for the time development of the phase. This equation is gauge invariant and leads to a simple derivation of the Aharonov-Bohm effect and the Berry phase (Brown and Hiley 2000).

If we now choose the special case of a stationary state where the energy is fixed, i.e., $\Psi(\boldsymbol{r},t) = \psi(\boldsymbol{r})\exp[-iEt]$ we find

$$P(\boldsymbol{r})E + \tfrac{1}{2}\langle \boldsymbol{r}|\big[\hat{\rho},\hat{H}\big]_+|\boldsymbol{r}\rangle = 0 \tag{16}$$

Thus the expectation value of the anti-commutator is just an expression of the energy density in a stationary state. As equation (11) is an expression for the conservation of probability so equation (16) can be regarded as an expression for the conservation of energy.

Up to this stage the quantum potential has not made an explicit appearance. We can soon rectify that by evaluating the anti-commutator for the Hamiltonian (10). It is straight forward to show that equation (15) becomes

$$\partial_t S_r + \frac{1}{2m}\left({}_r S_r\right)^2 - \frac{1}{2mR_r}\left({}_r^2 R_r\right) + V(\boldsymbol{r}) = 0 \tag{17}$$

This is of course identical to equation (3) above so that the quantum potential only takes on an explicit form when we go to a representation. (We have added the suffix *r* because we are using the position representation). What is more we see that *both* the 'guidance' condition $\boldsymbol{p} = \nabla S$ *and* the quantum potential $Q = -\frac{1}{2mR}\nabla^2 R$ appear *together* in equation (17), both emerging from the anti-commutator in equation (16). Let me stress once again neither of these terms appears explicitly in the operator equations (8) and (9). *They only appear when we choose a representation for the operators*. Thus it is only when we project operators into a representation space that these two conditions arise.

Indeed we can bring this out even more clearly by going to the momentum representation. In this case we find the corresponding conservation of probability in momentum space is

$$\partial_t P(\boldsymbol{p},t) + \nabla_p . \boldsymbol{j}_p = 0 \tag{18}$$

where $\boldsymbol{j}_p$ is the probability current in momentum space given by

$$\boldsymbol{j}_p = -\langle \boldsymbol{p} | \nabla_r \left(\hat{\rho} V(\hat{\boldsymbol{r}}) \right) | \boldsymbol{p} \rangle \tag{19}$$

While the energy equation (9) becomes

$$\partial_t S_p + \frac{p^2}{2m} + V\left(\nabla_p S_p \right) + Q_p = 0 \tag{20}$$

In this representation the quantum potential, Q_p, is no longer a simple expression except in the case of the harmonic oscillator (see equations (21) and (22) below). In general it becomes a complicated function of higher order derivatives such as $\left(\nabla_p^s S_p \right)$ and $\left(\nabla_p^r R_p \right)$. The form of the function depends on the actual form of the potential *V*. Here R_p and S_p are the amplitude and phase of the wave function in momentum space. More details of the *p*-representation will be found in Brown and Hiley (2000).

Since we have the equivalent pair of equations (18) and (20) in the *p*-representation we can calculate trajectories in this representation as well. This means that we have two sets of trajectories for the same system and, in effect, we have *two* phase spaces. *Two* phase spaces? Surely there is something wrong here. No, nothing is wrong! Take the algebraic formalism of quantum mechanics very seriously without dragging in our normal prejudices about classical or quantum mechanics. Assume the algebraic elements describe some new notion of 'process' *per se*. What does the structure of this process tell us about the nature of quantum processes?

Let us follow Gel'fand's ideas (see Demaret *et al*. 1997) and ask what the algebraic structure tells you about the underlying phase space. Because the algebra is non-commutative there is no *single* underlying manifold. That is a mathematical result. Thus if we take the algebra as primary then there is no underlying manifold we can call *the* phase space. But we already know this. At present we say this arises because of the 'uncertainty principle', but nothing is 'uncertain'. What I am suggesting is that the word 'uncertainty' is not appropriate. The nature of quantum processes are such that it is not possible to specify a point in phase space by using the eigenvalues of operators *X* and *P* together. This, of course, is merely Gleason's theorem. (Gleason 1957)

If we use the eigenvalues of *X*, that is the *r*-representation, then we must *construc*t a momentum $p_B = \ {}_r S_r$, which is not an eigenvalue of operator *P* in that representation. This will give us one of the phase spaces. If however we were to consider the *p*-representation and use the eigenvalues of *P*, you cannot use the eigenvalues of operator *X* in the same representation. We must now construct an $x_B = \ {}_p S_p$, which is not an eigenvalue of operator *X* in the *p*-representation. This is the second phase space. In previous papers we called these spaces *shadow phase spaces*. This leaves us with the interesting question as to the relationship between these two spaces. We will not discuss this relationship here. Again I refer the interested reader to Brown and Hiley (2000) and Hiley (2001)

Finally in order to emphasise the similarity between the *r*- and *p*-representations, let us consider the harmonic oscillator. In this case the potential is quadratic, $V = \frac{K}{2}\boldsymbol{r}^2$, so that equation (20) reads simply

$$\partial_t S_p + \frac{p^2}{2m} + \frac{K}{2}\left(\ {}_p S_p \right)^2 - \frac{K}{2R_p}\left(\ {}_p{}^2 R_p \right) = 0 \qquad (21)$$

while in the *r*-representation the harmonic oscillator gives

$$\partial_t S_r + \frac{1}{2m}\left(\ {}_r S_r \right)^2 + \frac{K}{2} r^2 - \frac{1}{2mR_r}\left(\ {}_r{}^2 R_r \right) = 0 \qquad (22)$$

These equations clearly reflect the symmetry of the harmonic oscillator in phase space. In both cases we see that we have a guidance condition and a quantum potential, thus emphasising that both arise in the same set of equations, namely, those that arise from the *specific representation of the anti-commutator*.

Notice one immediate advantage of the algebraic approach is that it restores the symmetry between the *r*- and *p*-representations. This lack of apparent symmetry was another feature criticised by Heisenberg. But there is no lack of symmetry. There is complete symmetry reflecting the fact that the symplectic (canonical) symmetry that is present in both the classical domain and in the standard interpretation is also present in the Bohm approach as it has to be.

Notice also the role played by the commutator and the anti-commutator of ρ with *H,* the Hamiltonian. Nowhere does the quantum potential or the guidance condition appear in the algebra of operators. They only appear when a particular representation is chosen and then they both appear together in the same equation as illustrated in equations (17) and (20). Thus as long as the Schrödinger equation is assumed to be valid there is no need to establish the guidance condition as a primitive condition.

5. Interpretations.

Recall that Bohm's original intention to provide a description of the evolution of an *actual* quantum process. For such a mere potentialities is not sufficient. But the Bohm approach uses the wave function to obtain equation (17), therefore this equation must still contain information about potentialities. What better way to discuss potentialities than through the concept of a potential, the quantum potential!

But there is more. Clearly equation (17) looks like a quantum version of the conservation of energy, if we regard the two terms $\frac{1}{2m}(\ _rS_r)^2 + Q$ as referring to the *internal energy* of the particle evolving in a given experimental set up. It is essential to regard this energy as internal as the quantum potential has no external source. It is these two terms that provide us with the possibility of distinguishing between the *actual* and the *potential*. We can regard $\frac{1}{2m}(\ _rS_r)^2$ as describing the *actual* kinetic energy of the particle, implying that we can regard $p_B = \ _rS_r$ as the momentum of the particle.

Notice further that $p_{\mathrm{B}} = \ _rS = \left(\psi^*\hat{P}\psi\right)$ which means we are only taking part of the available kinetic energy. We call p_{B} the *beable* momentum to distinguish it from the value of the operator *P* in the *r*-representation (for more details see Belinfante 1973). Clearly *Q* is not *ad hoc* because it must be present to conserve the total energy of the system. Thus far from ‘obscuring its (the theory's) essential structure', the quantum potential plays an essential role in the formalism.

6. The quantum potential and information.

I now want to look at a possible interpretation of the quantum potential in terms of *information*, a concept that is central to this conference. To do this I will restrict my consideration to the *r*-representation only. Before going into details I need to discuss the role of information in physical theories. I first want to distinguish my use of the term 'information' from the more usual notion of Shannon information.

To motivate this discussion I need to review the notion of a potential. In classical physics a potential is a force field generated by an outside agency and describes the potential evolution of the particle starting from a given position. As particles in different positions experience different effects we can think of the potential as being revealed through the movements of an ensemble of particles.

How do we think about the quantum potential? It describes a field of energy so can it be regarded as producing a force on the particle? There are some problems with this view. Firstly, as we have already remarked above, the quantum potential has no external source so that there is nothing for the particle to 'push against'. The energy is internal so clearly there is something more subtle involved. Here it is more like the role the gravitational field plays in general relativity where the gravitational energy curves space-time itself.

Secondly, the quantum potential does not arise directly from the Hamiltonian and therefore does not appear explicitly in the algebraic equations (8) and (9). The quantum potential only appears when we project equation (9) into a particular representation space. This is even more like gravitation where the 'force' appears only when we project the geodesics into a Euclidean space. It is only in this space that we see the deflected trajectories revealing the presence of the gravitational force.

Thirdly, the form of the quantum potential in equation (17) shows that it depends on the amplitude of the quantum 'wave'. Not only does it depend on a second derivative, but the amplitude also appears in the denominator. This implies that the potential is no longer proportional to the amplitude of a 'wave' as one would expect from classical physics. Because of this the potential gives rise to effects that are totally different from those expected from a classical wave. In the classical case the force produced by a wave is directly proportional to its amplitude as any swimmer will know from direct experience.

The appearance of the amplitude in the denominator also explains why the quantum potential can produce strong long-range effects that do not necessarily fall off with distance. These are the typical properties of entangled wave functions. Thus even though the wave function spreads out, the effect of the quantum potential need not necessarily decrease. This is of course just the type of behaviour required for an explanation of the EPR paradox.

If we examine the expression of the quantum potential in, say, the two-slit experiment, we find that it depends on the width of the slits, their distance apart and the momentum of the particle. In other words it contains information about the overall experimental arrangement. Of course this is not mathematically at all surprising because the wave function is a solution of the Schrödinger equation, which must necessarily reflect the boundary conditions.

But we want to suggest that we can think of the process in terms of a local particle being fed this information locally through the information contained in the potential field as the particle evolves along its path.

But how are we to understand these puzzling features physically? Because there is nothing to push against we should not regard the quantum potential as giving rise to an efficient cause, ('pushing and pulling') but it should be regarded more in the spirit of providing an example of Aristotle's formative cause. That is the quantum potential gives new *form* to the evolution of the trajectories, in a way that is very reminiscent of the morphogenetic fields proposed by Waddington (1956) and Thom (1975) in biology. The form is provided from within but it is, of course, shaped by the environment. Thus the quantum potential reflects the experimental conditions. Close one slit and the quantum potential changes and the subsequent evolution of the particle is different. There seems to be a kind of 'self-organisation' involved.

Now self-organisation requires the notion of information to be *active*. In the case of a biological system, this information is clearly provided by the environment, soil conditions, lack of moisture etc. In a quantum system I want to suggest that the information is provided by the experimental conditions, its environment. But this information is not passive. It is active and causes the internal energy to be redistributed between the kinetic (p_B) and potential (Q) parts. Thus the quantum process is literally 'formed from within'. Notice I am not using the notion of information in its usual sense but following its meaning in a literal sense. As Miller (1987) points out, the etymological origins of the word 'information' stresses the active role of information. In-form literally means to form from with in. He writes, "The central stem ({\em forma}) carries the primary meaning of visible form, outward appearance, shape or outline. So *informare* signifies the action of forming, fashioning or bringing a certain shape or order into something." So when I use the word information here, I do not mean information for me, the experimenter, but the activity taking place in the system itself. In other words information is playing an objective and active role in all quantum processes.

This notion fits in very nicely in with Bohr's original answer to the EPR paradox(Bohr 1935). Bohr argued that we should not regard the coupling between entangled pairs as arising from a mechanical force. Rather he talks of "an influence on the very conditions which define the possible types of prediction regarding the future development of the system". Bohr felt this was a key point because he italicised the phrase in his original paper and repeated it word for word in his Discussions with Einstein (Bohr 1949). In a mechanical explanation all `influences' must be mechanical, but our proposals suggest a more 'organic' picture where information that is playing a dynamic and objective role, namely, it is active. Thus once again rather than contradicting Bohr, I argue that the Bohm interpretation actually offers some clarification of his position!

7. Active information and Shannon information.

I do not have time here to discuss the advantages of treating the quantum potential as an information potential but fortunately these have already been discussed in Bohm and Hiley (1987, 1993), and in Hiley (1995, 1999). I would now like to finish by outlining how our notion of active information relates the more commonly used Shannon information.

Firstly it is important to emphasise once again that our concept of information is not 'information for us' but objective information for the particle. It was very evident from the papers presented at this meeting that the word 'information' was being used in the sense of 'information for us'. Here I see my task as attempting to relate these two very different notions.

Let me start by considering what happens in a typical measurement. For simplicity I will consider a Stern-Gerlach magnet which separates the wave function into two distinct regions of space. Traditionally these are called 'wave packets' but I prefer to call them 'channels'. Thus as a particle passes through the S-G magnet it will enter one the channels, say, I. While it is travelling in channel I it will be acted upon by the quantum potential calculated from the wave packet associated with that channel.

As we have argued above this information is 'active'. The quantum potential generated by the other wave packet does not produce any direct effect on the particle. We call the information from channel II 'passive'. At this stage we cannot discard any of the information because if the two channels overlap again, the information associated with both wave packets will become active and contribute to the overall quantum potential. The passive information in the channel II becomes active once again. Retaining both bits of information is essential to explain interference.

Measurement involves some of the information becoming inactive, i.e. it never acts again. To bring this about we have to add some new instrument in which a local irreversible process takes place, like the blackening of a photographic plate, or the click of a Geiger counter. Let us place the detector in one of the channels, say, channel II. Since the detector is described by a wave packet, η, that is well localised, the resulting wave function will be

$$\Psi_{(\text{output})} = a\psi_1(\boldsymbol{r},t)\eta_{(\text{unfired})} + b\psi_2(\boldsymbol{r},t)\eta_{(\text{fired})} \qquad (23)$$

By 'fired' here we simply mean that the irreversible discharge has occurred. Even if the state of the detected particle is only slightly changed so that the coherence between the two wave packets associated with the different channels is not destroyed, there will still be no interference effects because the information in the `unfired channel' becomes deactivated. It has become 'inactive'.

To see how this comes about we must calculate the quantum potential in the output channel after the device has fired. For this purpose we use wave function (23) evaluating it at the final positions of all the particles involved in the irreversible process. Since, in the final state, the detector particles have moved over macroscopic distances, the contribution from the 'unfired' state is zero. Thus the first term in equation (23) makes no contribution to the quantum potential. If on the other hand the device had not fired, then the contribution from the second term, the fired state, would be zero. Thus the act of firing or not firing alone will ensure that 'interference' between ψ_1 and ψ_2 is no longer possible.

It must be emphasised that the observer's role here is no different from that of an observer in classical physics. The observer is only needed to set up the apparatus in the first place.

The observer does not play an active role such as "looking" at the detector to collapse the wave function. All that is necessary is that the wave packets associated with the two states of the detector are spatially distinct. By spatially distinct I mean that in evaluating the total quantum potential at the positions of the particles in the detector, only one of the wave packets gives a non-zero contribution. Irreversibility is not demanded at this stage.

Where does irreversibility enter the argument? Suppose the detector had fired. If we could move all the particles in the detector into their 'unfired' position by a reversible process so that η can be factored out, then interference between the two branches of the wave function would re-appear. But can we return all the particles in the 'fired' state to their 'unfired' positions? Surely all we have to do is to charge up the detector again. All the particles will have returned to the unfired position and η will be the same in both terms of equation (23) and the interference returns. But to reverse the positions we would have to `charge up' the detector again. The wave function of the charger would have to be taken into account and the story would start again. We are up against irreversibility so we could never get the contributions from ψ_1 and ψ_2 to become coherent again.

If the detector had not fired requires a somewhat different argument. Recall that we want to factor out the wave packet of the detector again. In this case we must move all the particles into a 'fired' position, but which fired positions? There are an infinity of such positions, so which do we choose? But anyway the detector has not fired so the whole exercise is pointless. The contribution from the fired wave packet to the quantum potential is always zero, consequently there will be no contribution from ψ_2 either. Thus the possibility of interference between the two channels is lost forever.

Since it is impossible in practice to retrace all the trajectories of all the particles in such a cascade, irreversibility renders the information permanently 'inactive'. From now on the particle behaves as if the wave function has collapsed. But there is no actual process of wave function collapse. There is a redistribution of quantum potential energy so that from now on the system behaves as if it were in a mixed state described by the density matrix

$$\rho = |a|^2 \psi_1^*(\boldsymbol{r},t)\psi_1(\boldsymbol{r},t)\eta^*_{(\text{unfired})}\eta_{(\text{unfired})} + |b|^2 \psi_2^*(\boldsymbol{r},t)\psi_2(\boldsymbol{r},t)\eta^*_{(\text{fired})}\eta_{(\text{fired})} \qquad (24)$$

Clearly the entropy of the system has increased, which is exactly what is required. We can also look at this in more heuristic terms. The Stern-Gerlach magnet increases the number of accessible quantum states available to the system, and this means the entropy must have increased.

Turning to consider this entropy increase in terms of Shannon information, we see that the information capacity of our system has increased. To see this in a particular example consider the case where the original particle with its spin along the *x*-axis is fired into the Stern-Gerlach magnet aligned along the *z*-axis. This produces two possible output states so that we have we have created two qubits. The Bohm approach would claim that the two bits of information have their physical origins in the activity in one of the two possible quantum potentials. One becomes active when the particle is in channel I, while the other becomes active when the particle is in channel II. This is information for the particle and therefore measures some form of objective information.

The application of these ideas to quantum teleportation was subject to a recent paper by Maroney and Hiley (1999). They have shown how quantum teleportation could be explained in terms of transfer of active information. This is particularly relevant to the topic of this conference.

The current outlook is to look at the two quantum states and to argue that the information is for us because we do not know which of the two spin states a particular particle is in. In other words this information is about information for us. We should notice that this is not an either/or situation. 'Information for us' must be carried by some physical process and quantum processes provide a novel way of carrying active information with, as we have seen in this workshop, some fascinating consequences[1]. But if we are to look at physics in terms of information as Wheeler (1991) has speculated then I feel we should be talking about objective information not subjective information.

[1] In an extremely interesting development of these ideas, Andrei Khrennikov (2000) has proposed a kind of generalised information dynamics on p-adic space which can be applied to cognitive processes in general.

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