The ‘Beables’ of Relativistic Pilot-WaveTheory

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It is widely–believed that the pilot-wave approach can make do with classical fields as beables. I argue that there is then no guarantee that the theory will solve the problem of measurement. Only particles as beables are sure to do that. Here there are two familiar strategies, due to Feynman and Dirac; of these only Dirac’s is amenable to pilot-wave methods. The beables had better be particles, and, wth opposite charge, the absence of particles in an infinite negative energy sea.

1 Non-Relativistic Quantum Mecanics (NRQM)

It will be helpful to have the exampe of the non-relativistic theory spelt out in some detail. The eables are structureless point-particles. In the simplest case, here we have a single spinless particle, the Schrödinger equatn referred to a Galilean frame is:

$$\left(-\frac{\hbar^2}{2m}\nabla^2 + V(\mathbf{x},t)\right)\Psi(\mathbf{x},t) = i\hbar\frac{\partial}{\partial t}\Psi(\mathbf{x},t). \tag{1}$$

Familiar manipulations yield, for the real and imaginary parts of $\Psi = Rexp(iS)$:

$$\frac{\partial S}{\partial t} = \frac{(\nabla S)^2}{2m} - \frac{\hbar^2}{2m}\frac{(\nabla^2 R)}{R} + V = 0 \tag{2}$$

$$\frac{\partial R^2}{\partial t} + \nabla\cdot\left(\frac{R^2\nabla S}{m}\right) = 0. \tag{3}$$

By Gauss’s theorem, it follows from (3) that the integral of $R^2(\mathbf{x},t)$ over all space is constant in time (assuming R and ∇S vanish at infinity). Eq.(3) should be compared with the equation of continuity in classical hydrodynamics, with $\rho(\mathbf{x},t)$ as the density function of a classical fluid, and $\mathbf{v}(\mathbf{x},t)$ the velocity distribution function:

$$\frac{\partial \rho}{\partial t} + \nabla\cdot(\rho\mathbf{v}) = 0. \tag{4}$$

The spatial integral of the density function over a volume V is the total mass contained in V; Eq.(4) states that any change in this mass must be compensated by a net flow of mass into V across its boundary ∂V, as given by the surface integral of the momentum density $\rho\mathbf{v}$ over V. Evidently Eqs.(3) and (4) will have a similar interpretation if R^2 is taken to be the analog of ρ, the mass-density, and if the gradient of the action S is interpreted as a momentum density (as it is in classical Hamilton-Jacobi theory). We define a velocity vector field on the basis of this analogy:

$$\mathbf{v}(\mathbf{x},t) = \frac{1}{m}\nabla S(\mathbf{x},t). \tag{5}$$

What is its physical meaning? It is at this point that the pilot-wave theory and conventional quantum mechanics part company. According to the pilot-wave theory, there exist *particles with definite trajectories*. The *allowed* trajectories are the integral curves of the vector-field $\mathbf{v}$. So long as S is well-defined, these trajectories are continuous functions of the time, one and only one of which passes through any point at each time. Formally, we are to find a family of functions $\mathbf{y}(t)$ such that

$$\frac{d\mathbf{y}}{dt}|_{t=t_0} = \mathbf{v}(\mathbf{x},t_0)|_{\mathbf{x}=\mathbf{y}(t_0)}. \tag{6}$$

Solving Eq.(6) for given S at space-time point p with coordinates $(\mathbf{y}_0, t_0)$, so long as $\mathbf{v}$ at p and time t_0 is non-zero, we obtain a unique trajectory $\mathbf{y}_p$ through p with $\mathbf{y}_p(t_0) = \mathbf{y}_0$. The position of a particle determines its velocity, and hence its change in position $\delta\mathbf{y}_0 = \mathbf{v}(\mathbf{y}_0, t_o)\delta t$ in time δt after $t = t_0$; and thereby its entire trajectory.

If we suppose that only one trajectory actually exists (the one going through the point p, say), and that positions along this trajectory are what are observed in subsequent measurements, then obviously there is no measurement problem of the conventional kind. Regardless of whether or not the state evolves into a superposition of 'position eigenstates', there will be only one particle position at each time t, as fixed by $\mathbf{y}_p(t)$.

The method is easily generalized to n-particle systems, replacing throughout the coordinates $\mathbf{x} = (x_1, x_2, x_3)$ on the 1-particle configuration space by $\{x_i^r\}$, $r = 1, ..., n, i = 1, 2, 3$ on the $3n$-dimensional configuration space Γ (assuming the systems are unconstrained). Given that at time t_0 the system occupies the configuration space point $\gamma \in \Gamma$, we once again obtain a unique trajectory through γ (a map $R \to \Gamma$ with $t_0 \to \gamma$), and with that unique positions at each time for each of the n particles.

In this way the measurement apparatus, too, can be modeled in the theory. It now follows, with no further assumptions, that the space-time properties of macroscopic objects, including pointer positions, will be just as definite, no more and no less, than those of their constituent particles. At the very worst the pointer might be vaporized or otherwise broken into parts; but even in that case, the result will be describable in classical terms (because the behavior of the constituent particles will be classically describable). There can be no Schr"dinger cat paradox in consequence.

It is another matter as to whether the statistics of outcomes match the values predicted, using the usual measurement postulates. To return to the 1-particle case, they will if the sample population is selected in accordance with the probability measure:

$$\rho(\mathbf{x},t) = |\Psi(\mathbf{x},t)|^2 = R^2. \tag{7}$$

If particle positions are 'typical', at time t_0, i.e. they have the distribution (7) at $t = t_0$, then they will be 'typical' at all other times (by virtue of the equation of continuity, Eq.(3)). What of the remaining equation, Eq.(2)? Solutions to this determine ∇S as a function on $\Gamma \times R$, and thereby determine the trajectories $\mathbf{y}_p$ for variable p. We can, however, combine (2) and (6) into a single second order equation. Differentiating Eq.(6) with respect to time we obtain:

$$\frac{d^2\mathbf{y}}{dt^2} = \frac{d\mathbf{v}(\mathbf{x},t)}{dt}|_{\mathbf{x}=\mathbf{y}(t)} = \frac{\partial\mathbf{v}(\mathbf{x},t)}{\partial t}|_{\mathbf{x}=\mathbf{y}(t)} + (\mathbf{v}\cdot\boldsymbol{\nabla})\mathbf{v}(\mathbf{x}.t)|_{\mathbf{x}=\mathbf{y}(t)} \tag{8}$$

where the second term takes account of the variation in $\mathbf{v}$ in time δt due to the change in the value of $\mathbf{x}$, from $\mathbf{y}(t)$ to $\mathbf{y}(t+\delta t)$.Taking the divergence of (2) we obtain:

$$\left(\tfrac{\partial}{\partial t} + \left(\tfrac{1}{m}\right)\nabla S\cdot\nabla\right) = -\nabla(V+Q)$$

where Q, the 'quantum potential', is the term involving R in (2).

Using (8) we obtain the equation of motion, Eq.(6), as a second-order equation:

$$\frac{d^2}{dt^2}(m\mathbf{y})|_{t=t_0} = -\nabla(V(\mathbf{x},t_0) + Q(\mathbf{x},t_0))|_{\mathbf{x}=\mathbf{y}(t_0)}. \tag{9}$$

This has a form familiar from classical mechanics, but we must remember that the three components of the velocity at time t_0 are not constants of integration along with the position $\mathbf{y}_0$ at this time. Only those solutions $\mathbf{y}$ to Eq.(9) for which (6) holds at $t = t_0$ are allowed, given which, (6) then holds at all times; so the velocities are completely fixed. In NRQM, Eq.(6) is the fundamental equation. Nothing more is needed, for the pilot-wave theory, other than the Schr"dinger equation, and the use of the measure (7). In particular, measurements are automatically taken care of. There is no need for any further postulates, or mention of experiment or observation. The measurement problem is solved.

2 Relativistic Particles

Can we proceed analogously in the relativistic case? But we know that in orthodox quantum theory there is no uniform generalization of NRQM to the relativistic domain. Only in special circumstances can we carry over the usual prescriptions.

One sees this very simply in the case of the scalar (Klein-Gordon) wave equation, with external potential A_μ, $\mu = 1, 2, 3, 4$ (putting $\hbar = c = 1$):

$$g^{\mu\upsilon}(i\partial_\mu - eA_\mu)(i\partial_\upsilon - eA_\upsilon)\psi = m^2\psi. \tag{10}$$

From this we deduce that there exists a divergence-free 4-vector j_s with components (writing $\varphi\overleftrightarrow{\partial_\upsilon}\psi$ for $\varphi\partial_\upsilon\psi - (\partial_\upsilon\varphi)\psi$):

$$j^\mu = g^{\mu\nu}(\frac{i}{2m}\psi^*(i\overleftrightarrow{\partial_\upsilon} - 2eA_\upsilon)\psi \tag{11}$$

but in the general case the vector j_s ("s" for "scalar") is not everywhere timelike. As a result, whilst the spatial integral of its time-component is conserved, by Gauss's theorem, this integral need not be positive. So we cannot use (11) to define an invariant positive-definite inner-product on the space of solutions to (10), blocking the construction of a Hilbert space and a probability interpretation. In the free case, when the external potentials are zero, one can restrict the class of solutions to define a Hilbert space with (11) positive-definite (the Hilbert space of positive-frequency states). The same can be done for certain classes of slowly-varying external potentials.

In both cases the phenomenology is similar to that of NRQM; the creation and annihilation of particles, the processes characteristic of particle physics, involve strong fields and rapidly-changing potentials.

There are worse difficulties for the analogous 1-particle model in pilot-wave theory. The lack of a positive definite norm likewise means that there will be problems in the probability interpretation (cf. Eq.(7)), but instead of the problem of defining the Hilbert space, and the operator formalism that goes with it, there is a difficulty in defining the particle trajectories: the spatial part of Eq.(10) ought to give us the guidance condition, but j_s is not everywhere timelike. Unless the regions in which the integral curves of j_s are spacelike can be excluded, or shown to have measure zero, then particles can move at superluminal speeds, and even reverse their direction in time. This is so even in the free case, with $A = 0$, and even restricting ourselves to superpositions of states which have positive energy (in the usual sense), and even if these individually yield timelike j (Kyprianidis 1985).

Evidently the scalar 1-particle pilot-wave theory is in trouble, even in the kinematic limit, where the conventional 1-particle theory is free of any difficulty. This is a surprise; we expected that this naive approach would encounter difficulties in parallel to those of the standard formalism, but here we find them in the limit of free particles. Since this is the asymptotic limit of scattering theory in RQFT, routinely used in applications, the result is not encouraging. This conclusion is widely shared (Bohm and Hiley, 1993, Holland 1993): in the scalar case, no credible 1-particle pilot-wave theory is on offer.

What of the 1-particle fermion theory? Here too the conventional theory is in difficulties, but again there is a well-defined kinematic limit. Let us see how things stand with its pilot-wave analogue. We shall work with the Dirac equation. The scalar wave-function of NRQM is replaced by a 4-component function $\Psi : x \to C^4$, $x \in R^4$, transforming under a finite-dimensional (non-unitary) representation of the group $SL(2, C)$ (the covering group of the Lorentz group), with 4×4matrix generators γ^μ, $\mu = 1, 2, 3, 4$, obeying the relations:

$$\gamma^\mu \gamma^\upsilon + \gamma^\upsilon \gamma^\mu = 2g^{\mu\upsilon}.$$

In terms of the γ-matrices the Dirac equation for external potentials A_μ is:

$$\gamma^\mu (i\partial_\mu - eA_\mu)\Psi = m\Psi. \tag{12}$$

The adjoint representation of $SL(2, C)$ is in terms of bispinors $\overline{\Psi}$; referred to a Lorentz frame (where "†" denotes transpose and complex conjugation):

$$\overline{\Psi}(\mathbf{x}, t) = (\gamma^0 \Psi) \dagger (\mathbf{x}, t). \tag{13}$$

Using these representations, we can construct the divergence-free 4-vector j_D (summing over spinor indices) with components:

$$j_D^\mu(x) = \overline{\Psi}(x)\gamma^\mu \Psi(x). \tag{14}$$

This gives us a real 4-dimensional vector field which, in contrast to (10), is everywhere timelike. We can therefore define a conserved positive-definite inner-product:

$$< \Psi, \Phi >= \int \overline{\Psi}\gamma^\mu \Phi d\sigma_\mu. \tag{15}$$

This does not really solve the problems encountered in the scalar case, however; here, just as in the scalar case, the naive Hamiltonian, a determined by the wave-equation, has negative eigenvalues. The situation is not as bad as in the KG theory, where we cannot define the Hamiltonian as an operator on a Hilbert space at all, but it is bad enough: if there is no lower bound to the energy, there can be no ground state stable under strong couplings; at best there will only be a local minimum, and the most that could be hoped for is a perturbative treatment with respect to it.

What about the pilot-wave theory? It is clear that the previous difficulty, that afflicted the scalar guidance condition, is no longer a problem. The Dirac current j_D is everywhere timelike, unlike the scalar current j . Its integral curves are at least candidates for particle trajectories. First form the 4-velocity field with components:

$$u^\upsilon(x) = f(x)j_D^\upsilon(x)$$

(where $f(x)$ is a normalization factor ensuring that $u^\upsilon\ u_\upsilon = 1$ for non-zero j). Its spatial part $\mathbf{v}$, a 3-velocity field, has components $v^k = u^k\ /u^0$, $k = 1, 2, 3$:

$$v^k(x) = \frac{\overline{\Psi}(x)\gamma^k \Psi(x)}{\Psi^\dagger(x)\Psi(x)}. \tag{16}$$

The particle trajectories now follow from this as in NRQM, using Eq.(6). It is easy to check that $|v|^2$ is bounded by 1 (i.e. c, the velocity of light), as it should be. If, further, we use (15) to define a probability interpretation, we see that the ensemble average of the components of velocity are:

$$< v^k >= \int \Psi^\dagger(x)\alpha^k\Psi(x)d^3x \tag{17}$$

where $\alpha^k = \gamma^0\gamma^k$, from which we see that the expectation value of the speed is likewise bounded by c. The important point is that all of these favorable features hold whether or not the state is made up of positive or negative frequency parts. It seems, in fact, that the pilot-wave theory has done much better than the standard 1-particle theory. For example, whilst Eq.(17) has often been interpreted as the mean electron velocity, in the orthodox tradition, there has always been the problem that the matrices α^k do not commute; if these are the velocity operators, it would follow (by the usual arguments) that different components of the velocity are not simultaneously measurable. Their eigenvalues, moreover, are ± 1, giving rise to Schr"dinger's picture of "zitterbewegung", of particles trembling back and forth at the speed of light. These have remained puzzles within conventional thinking. One solution, adopted by many, is to give up on the concept of particle positions and velocities, and make do with momenta instead, which are perfectly simple and easy to define by self-adjoint operators (see my 1991 for references and further discussion). But they are not puzzles for the pilot-wave theory, which defines the positions and velocities by the guidance condition, independent of the Hilbert-space theory altogether. If there are no covariant position operators, then so much the worse for operators; the pilot-wave theory can make do without them. But success in this context brings with it a new difficulty. The 1-particle pilot-wave theory had better not be free of all internal difficulties, for if it is then it will be applicable whatever the external potentials. But we know that when the field strengths are large or quickly varying then the particle number is likely to change. We know this from the conventional theory, but also from experiment. The 1-particle pilot wave theory better had get into difficulties, given sufficiently strong couplings. If we look again to the history of the subject, the problem that Dirac and others were wrestling with was only indirectly related to the existence of position operators (which remains a problem to this day). Rather, he was concerned with the interpretation of the negative-frequency states; just what the pilot-wave theory appears to be blind to. Only on the standard interpretation does this notion of negative total energy appear so peculiar, and loom so large: one expects the potential energy to vanish in the asymptotic limit, but then if the total energy were negative, the kinetic energy would have to negative. We have no idea what that would mean. The theory cried out to be replaced by one better, and it was clear that the distinction between positive and negative frequency solutions was the key.

What seemed to a strength of the pilot-wave theory now looks to be its failing; in terms of the beables of pilot-wave theory, there can never be any question of negative kinetic energies; particle trajectories are always well-defined, independent of whether the state has negative frequency parts. The difficulty is more concealed, for it lies in the quantum potential. Appear it must, if we have the same unitary evolution of the state in the two theories, for things do go wrong at the level of the state, on conventional thinking. On the standard view, the absence of a lower bound to the energy will be catastrophic if the couplings are sufficient strong, for components of state with arbitrarily large negative energies will grow in amplitude at the expense of all others. That ought to make for unbounded growth in the (positive) kinetic energies recognized by the pilot-wave theory, those defined by Eq.(16). But it is noteworthy that this reasoning depends on the energy as defined by the state, and hence the operator formalism; if we are to reproduce it in the pilot-wave theory, it will be in terms of the quantum potential. Evidently the successes and the failings of the pilot-wave theory, in the 1-particle interpretation, are different in kind from those of the conventional approach. It is far from clear how the concept of anti-matter, wedded as it is to negative energies in the standard theory, is to make its appearance at the level of the beables. Of course, in view of the ubiquity of particle creation and annihilation processes, as actually observed, we already know that the dynamics should lead to change in particle number, a concept foreign to NRQM and, as we have seen, to the pilot-wave theory.

3 Alternative Particle Beables

Historically speaking, regarding the negative-energy difficulty, three strategies turned out to be productive. The first was Dirac's hole theory, of the early '30s. This led him to predict the existence of antimatter, specifically the positron. The second strategy, also built on Dirac's work, was to reformulate the theory as a theory of fields (RQFT), as was more common by the late '30s. The third proposal was due to Feynman, which again emphasised the particle aspect. Since for the moment we are considering particle interpretations, we are concerned with the first and third strategy. The basic idea of Dirac's hole theory applies only to fermions, particles subject to the Pauli exclusion principle. Dirac suggested that not only do negative-energy particles exist, but that they exist in such abundance that all the negative energy states are occupied. This is the Dirac negative energy sea. Since, according to the exclusion principle, no two fermions can occupy the same state, transitions to negative energy states would in general be prohibited. The only exception would be if a negative energy electron were to acquire sufficient energy so as to be ejected from the sea, acquiring positive total energy. Since the total energy includes the rest-mass energy, there is always a considerable threshold energy required for this to happen. But if it does happen, then there will be a "hole" in the negative-energy sea, and it will be possible for a positive energy particle to make a transition into the hole. Dirac showed that such a hole would behave as though it were a positive-energy particle of positive charge. So the creation of a hole would appear to be the creation of two positive-energy particles, of opposite electric charge; and the filling of a hole would correspond to the disappearance of the hole and the particle, so the annihilation of oppositely-charged particles. In this way Dirac was led to postulate the existence of annihilation and creation processes, involving oppositely-charged pairs. Since the mechanism obviously applies to any sort of particles, whether or not they are electrically charged (so long as they are fermions), we should really keep track of the number of holes and positive energy particles using a more universal notion of charge, positive for particles, negative for antiparticles. The hole theory makes it obvious that high-energy phenomena will involve variable particle number, despite the fact that the total number of positive and negative energy particles, all infinitely many of them, will be conserved, in accordance with the structure of NRQM. From a formal point of view his theory is conservative. That would seem to auger well for the pilot-wave theory, which works so well in NRQM. And Dirac's ideas can certainly get a foothold: one might hope to define change in particle number accordingly, at least in a perturbative sense, distinguishing positive and negative frequency parts by reference to the free Hamiltonian (with zero external potential). Ordinary QED can do no better, after all. But how the exclusion principle, or something analogous to it, would be related to a condition on the beables, is not so clear. This is at best a program of research.

Feynman's strategy was very different. He supposed that particles, or more properly probability amplitudes for particle processes, could be associated with paths that zig-zagged backwards and forwards in time. Such reversals in the path could be reinterpreted just as were Dirac's transitions into and out of the negative energy sea. A particle going backwards in time would appear to be an oppositely-charged particle moving forward in time; and on reversal back to the forward direction, it would appear that two particles had been created, one with opposite charge to the other. Conversely, a particle moving forward in time, reversing into the backwards direction, would appear as an instance of pair annihilation. Like the Dirac hole theory, it is clear that the apparent particle number can only change by multiples of two, that the mechanism applies quite generally, and that the real number of particles present (the number zig-zagging backwards and forwards in time) may be very different from the number observed. As Feynman said, it may be that there is only a single electron.

If Dirac's theory has too many things in it, Feynman's has too few. But the important question is whether the pilot-wave theory can make use of the idea. Of course Feynman's particles, though associated with trajectories (through the path-integral formalism), were not "beables" in any classical sense, no more so than were Dirac's. Neither of them offered a realistic theory which could clear up the problem of measurement. But his picture could surely apply to the trajectories of the pilot-wave theory, if only they have the appropriate form.

Alas, they clearly don't, if they are the integral curves of the Dirac current j ; what was a strength becomes its weakness. Should we work with the scalar KG current j instead? But we need a theory of pair creation and annihilation above all for fermions, and not for scalar particles. Alternative 4-vector fields have been proposed in the literature (Bohm, Schiller, and Tiomno, 1955), but they do no better in this regard, and worse in others (Bohm and Hiley, 1993, Sec.10.3). The root problem is that Feynman's methods, based as they are on the path-integral approach to quantization, have a more tenuous relation to the Schr"dinger equation, the point of departure of the pilot-wave theory, and give us no clues as to how they can be understood in canonical terms.

In point of fact, Dirac's methods were not as fruitful as those of field theory, whilst Feynman's, in the more fundamental applications, likewise appealed to Lagrangians defined by locally-interacting fields. Finally, we come on to field theory.

4 Fields as Beables

In quantum mechanics, the beables are particles; in quantum field theory, they should be fields. This is surely the most natural strategy. To make contact with the familiar methods of the standard formalism of field theory, we shall begin with the pilot-wave theory of normal modes. We take the field configuration space to be parameterized by a denumerable infinity of coordinates {q }, and rework the basic steps of the pilot-wave formalism using the state as a function on this space (that is, it is a "functional", since a complex function of infinitely-many coordinates is essentially a map from a Hilbertian function space to the complex numbers). The normal mode coordinates are essentially the real part of the complex amplitudes of the normal modes. Using box normalization in volume V:

$$\psi(x) = \frac{1}{V^{\frac{1}{2}}} \sum_k a_k \exp(i\mathbf{k} \cdot \mathbf{x}) \tag{18}$$

(here $\mathbf{k} \cdot \mathbf{x} = \sum_j k_j x_j$, and $k_j = 2\pi n_j / L$, $n_j \in Z^+$, $i = 1, 2, 3$, for a box of side L), we use the real c-numbers

$$q_k = |k|^{-1}(a_k + a_k^*) \tag{19}$$

as coordinates for the field configuration space. The phase space can be coordinatized by these and their canonical momenta. In the pilot-wave formalism, we write the the q 's as functions of the time, with time-derivatives given as before as tangents to the vector field (in configuration space) defined by the phase S, now a functional of the infinitely many parameters q (equivalently, of j). Formally we can follow through the steps (5), (6) to obtain:

$$\frac{dq}{dt}k^{(t)}|_{t=t_0} = \frac{1}{m}\frac{\partial S}{\partial q_k}[q_{1,\ldots,}q_k, \ldots]|_{q_k = q_k(t_0)}. \tag{20}$$

S is as before the phase of the state Φ, the solution to the Schr"dinger equation (not to be confused with the field equation for ψ):

$$H\Phi[\psi, t] = i\hbar \frac{\partial}{\partial t}\Phi[\psi, t] \tag{21}$$

where H is the Hamiltonian (involving the usual field operators), and square brackets indicate that the state Φ is a functional of the field ψ.

Eq.(20) specifies the k^{th} component of an (infinite- dimensional) vector field over an (infinite dimensional) space as a function of the time. Viewing S as a functional $S[\psi, t]$ over the space of classical field configurations ψ, this function of the time is the change in S in the "direction" $\exp(i\mathbf{k}.\mathbf{x})$ at time t.

This approach has been applied to some simple kinematical problems in pure electromagnetic field theory. As it stands we can see the problems we will encounter in trying to extend it to the interacting case: we will no more be able to solve (21) and express S as a functional in normal modes, in the interacting case, than we can make use of the plane-wave expansion, in the ordinary approach. But of course in the first place we should aim for a perturbative treatment, as in the usual theory.

Here we are trying to get an overview of the overall structure of the theory. To this end it will be helpful to reformulate it in more general terms, without relying on normal modes. We may use the field values themselves, parameterized by coordinates on R^3 . Formally we can view this as the replacement of an orthonormal basis (the normal modes) by delta functions. If we do this, we obtain the time derivative of the field ψ at a point $\mathbf{x}$ of R^3 (rather than the k^{th} component of the field) in terms of the functional derivative of the phase in the "direction" $\delta_{\mathbf{x}}$ (where $\delta_{\mathbf{x}}(\mathbf{y}) = \boldsymbol{\delta}(\mathbf{x} - \mathbf{y})$.

Let us make this precise. The functional derivative of a functional $A[\,]$ at χ in the direction ψ is

$$\tfrac{DA}{D\psi}[\chi] = \lim_{\varepsilon \to 0} \tfrac{A[\chi+\varepsilon\psi]-A[\chi]}{\varepsilon}.$$

In the case of the direction $\delta_{\mathbf{x}}$ we use the notation:

$$\tfrac{\delta}{\delta\chi(\mathbf{x})} A[\chi] \underset{def}{=} \tfrac{DA}{D\delta_{\mathbf{x}}}[\chi].$$

Equation (20) is then replaced by:

$$\frac{\partial\psi(\mathbf{x},t)}{\partial t}|_{t=t_0} = \frac{\delta}{\delta\chi(\mathbf{x})} S[\chi, t_0]|_{\chi(.)=} \tag{22}$$

This is the most natural generalization of the ideas sketched at Eqs.(5), (6), for the non-relativistic 1-particle case, to field theory.

In principle, if we can solve Eqs.(21), (22), we would have for each choice of beable at time $t = t_0$ - for each field configuration ψ at time t_0 , as a complex-valued function on R^3 - a trajectory through the space of classical field configurations. We would have the beable at every other time. In principle this problem is mathematically well-posed in the interacting case; in φ^4 theory, for example, in 1 or 2 spatial dimensions.

What of fermion fields? The standard field configuration space in this case is the space of bispinor functions whose values are not complex numbers but complex Grassmann numbers (see Berezin, 1966). These are c-numbers θ,η which *anticommute*, i.e.:

$$\theta\eta = -\eta\theta,\ \theta^2 = \eta^2 = 0.$$

It follows from this that functions $F(\theta)$ have a very simple form (consider the Taylor expansion!). Indeed, it is only integrals over sets of such numbers, the bispinor field configurations, that have physical significance. The anticommuting properties of such numbers have the effect of interchanging certain determinants of operators in Gaussian integrals, and automatically accounting for the various sign conventions relating Feynman diagrams for fermion fields. If one is prepared to work with Grassmann field configurations as beables, there would again seem to be no problem in principle; the method just sketched can, in principle, be applied to this case as well.

5 The New Problem of Measurement

Here is a clear mathematical program; but it is not so clear that we should embark on it. One objection is that the guidance equation, Eq.(20), or Eq.(22), is both non-local and non- covariant (the two claims are of course rather different). There has been plenty of discussion as to whether this will be an inevitable feature of any "realist" solution to the problem of measurement. It has often been argued that the world, the phenomenology itself, is non-local, so that a theory adequate to the phenomenology had better be non-local as well. Some are prepared to conclude from this that we should not bother about covariance either, at least at the level of the beables, so long as we can demonstrate that, at the macroscopic level, some version of a "no-signaling" theorem holds good (see, e.g., T. Maudlin, 1994). Similar claims are made on behalf of state-reduction theories.

But I will say nothing about these questions here, for there is a much more pressing problem. Call it the new problem of measurement: why is it that the field configurations are well-localized in space? And if they are not, in general, well- localized: what special considerations apply to suitable analogues of macroscopic bodies, to ensure that they are well-localized? If none, the measurement problem looms before us anew. It is no more use, here, to refer back to the non-relativistic limit, and to the particle beables that can be defined in that regime - local by definition of the concept of particle - than it would be to appeal to the classical limit, in the case of standard NRQM. Were we content with loose and formal arguments of that sort, there would have been no old problem of measurement, and no need to move to the pilot-wave theory in the first place,

An argument recently advanced is that indeed we can be sure that the field beables will be localized to within the non-relativistic wave-packet. It is due to Valentini (1992); he considered the real scalar field, adapting an argument to be found in Bohm et al, 1987. For simplicity, I shall state it in the non-relativistic case. Given box-normalization, the ground state as a functional of complex normal modes is:

$$\Omega[a_1, ...a_k, .., a_1^*, .., a_k^*, ...] = exp\left(-\sum_k k a_k^* a_k\right) \tag{23}$$

We can use Eq.(18) for the real scalar field as well. Inverting it we can write the c-numbers a_k^*, a_k in terms of the fields ψ, and then substitute in (23). We obtain:

$$\Omega[\psi, t] = exp\left(- \iint \psi(\mathbf{x}, t)\psi(\mathbf{x}', t) f(\mathbf{x} - \mathbf{x}') d^3x d^3x' \right) \tag{24}$$

where

$$f(\mathbf{x} - \mathbf{x}') = \tfrac{1}{\sqrt{V}} \textstyle\sum_k k \exp(\mathbf{k} \cdot (\mathbf{x} - \mathbf{x}').$$

In this way we can represent the vacuum state as a functional of real classical field configurations. We can extend this to n-particle states using the usual definition

$$|n >= \widehat{a}_k^* ... \widehat{a}_{k'}^* .... \widehat{a}_{k''}^* ... |0 >$$

(n creation operators in all). Using Eq.(23) to define the ground state, and representing the creation operators $\widehat{a}_k^*$ as derivatives, the 1-particle state of momentum $\mathbf{k}$ is:

$$\Psi_k[a_0, ..., a_k, ...] = a_k exp\,(- \textstyle\sum_k k a_k^* a_k)$$

Inverting (18) once again we can represent this state as a functional of field-configurations ψ. For a 1-particle state of the form:

$$\psi(\mathbf{x}, t) = \textstyle\int f_k \exp -i(\mathbf{k} \cdot \mathbf{x} + E_k t) d^3k$$

we find in this way:

$$\Psi_\varphi[\psi, t] = exp\left(- \int \varphi(\mathbf{x}, t)\psi(\mathbf{x}, t) d^3x \Omega[\psi, t] \right) \tag{25}$$

From this expression we can deduce that $\Psi_\varphi[., t]$ achieves its maximum value on configurations ψ whose support lies in the support of φ. It is obvious from Eq.(25) that configurations ψ for which sup(φ)$\cap$sup(ψ)= 0 have zero probability. We may allow that there is a rigorous argument somewhere in this neighbourhood, but it is obvious that it will fall short of its purpose. For let it be granted that the "most probable" classical field configuration can, in certain circumstances, be identified with the 1-particle non-relativistic state. From that it only follows that the problem of measurement will take the same form in pilot-wave RQFT that it takes in *standard* NRQM. What is needed is an argument to show that the classical field configuration will most likely be localized to regions small within the support of the non-relativistic state, in fact small on macroscopic length-scales, at least in measurement situations.

That part of the field representing the apparatus pointer had better be localized in this sense. Otherwise we are no better off than in standard quantum mechanics. How does the the pilot-wave theory guarantee this result in the non-relativistic case? The answer is not reassuring: there is nothing specific to sub-systems with a large number of degrees of freedom (the macroscopic pointer), or to the dynamics, or to the low-energy limit; the locality of the beables is ensured by the simple proviso that they are point-like by definition, that they are point particles in the classical sense. If this is the method used in the non-relativistic case, what reason is there for thinking that classical field beables, which by definition are not point-like, will turn out to be localized where it counts? So far we have no indication at all that this will be so; *no reason at all* has been given to suppose that solutions to Eq.(22) will, in appropriate circumstances, be localized to regions small on the macroscopic scale. This is the new problem of measurement.

Valentini's argument is so far from what is required that, on grounds of charity, we should ask whether he had some other point in mind. Indeed, it might seem that his aim was more modest:

> A realistic field description of particles may at first sight seem untenable....one may ask how a field distributed over all space can account for the highly localized massive particles seen in the laboratory. However, exactly the same query may be put to standard quantum field theory; for say the scalar case, the basic "observable" is surely the field operator í(x,t), whose eigenvalues are the set of definite field configurations í(x), associated with eigenstates |í(x)>. How do "particles" localized in space emerge? (Valentini, 1992, p.50).

True enough, the standard field theory is no better in this regard; but then, for most of us, the only virtue of the pilot-wave theory is that it resolves the measurement problem. If it is not better than the standard theory in this respect, then it has nothing to commend it.

6 Whither Hidden-Variables?

Failing a demonstration that solutions to Eq.(22) will, in the right circumstances, be localized - and no reason has been given to suppose that they will be - the pilot-wave theory had better make do with the mechanism it relies on in NRQM: the beables had better be particles. With that, whatever else might be wrong with the theory, there will be no problem of measurement.

Obviously it may be possible to come up with something entirely new. But as things stand nobody has; we had better consider again the options of Section 3. Of the two considered, it would seem that the best strategy is Dirac's. What is needed is the "negative-energy sea", and the restoration of a principled distinction between particles (fermions) and fields (bosons). In fact this fits much better with particle physics today, than it did almost fifty years ago, when Bohm first suggested that the pilot-wave theory should make use of Dirac's ideas. Now, but not then, we know that the constituents of ordinary matter are fermions, and that bosons are invariably associated with gauge fields (and hence with forces; the Higgs boson is the exception, but this is hardly a constituent of any ordinary matter). There is no cause for alarm if only fermions are sure to be localized, for they are all that we ordinarily see. Yet the hole theory has few advocates among those who support the pilot-wave theory. Thus Valentini (speaking of QED): "Not only is the [hole] theory very inelegant, it also creates an unsatisfactory dualism: particle description for the massive case versus field description for the massless case." (Ibid, p.50.) And thus Bohm, Hiley, and Kaloyerou: "Although this theory provides a consistent interpretation, it is somewhat ad hoc and consequently is not likely to provide a great deal of further insight." (Bohm et al, 1987, p.374.) So whither hidden variables in relativistic quantum physics? And whither hidden variables in the non-relativistic case, if there is no such insight to be had in the relativistic domain? But according to Bohm et al, it seems this is not a problem:

> However, it is our view that, at this stage, it is premature to put too much emphasis on the interpretation of relativistic quantum mechanics. This is because we feel that the theory in its current stage of development is probably not consistent enough to be given an overall coherent interpretation. First of all, there are the infinities which make it difficult even to see what the theory means. For example, the dressed particles are said to be in a different Hilbert space from which the theory starts. (Ibid, p.374.)

The Copenhagen interpretation has long been a refuge for obscurantists; it would be a pity if advocates of the pilot-wave theory were to resort to similar evasions. There are mathematical problems aplenty in renormalization theory, but that should not be allowed to hide the fact that relativistic quantum physics, in the case of special relativity, is a detailed and successful physical theory, as precise and systematic as anything in physics (more so; but the lesser point is enough). It is no more "premature" to seek to put this theory on a principled footing than it is to put NRQM on a principled footing. If, notwithstanding the remarkable success of the standard model, nothing less than a full-blown theory of quantum gravity will do (the quoted passage goes on to cite difficulties with reconciling quantum theory and general relativity), it is hard to see why we should bother with NRQM in the first place - hence neither with the pilot-wave theory. There is an honourable tradition, well known through Penrose's writings (Penrose 1989), according to which the problem of measurement is to be resolved by a proper marriage of quantum theory with gravity; but that is not what advocates of the de Broglie-Bohm theory are appealing to. Failing something radically new, or a proof that Eq.(22) yields localized field configurations, we are left with particles as beables, and with the approaches of Dirac and Feynman. I have already indicated my own opinion, that Dirac's offers the better prospects for the pilot-wave theory, but in truth both have their drawbacks. The hole theory is not, contrary to what is sometimes claimed (Bohm and Hiley, 1993, p.276), unitarily equivalent to conventional QED. It is simply not true that "in most cases the wave function will factorize, so that it will be sufficient to consider a limited number of particles and ignore the rest" (Bohm et al, 1987, p.375), or that the difficulty is "similar ...to what happens with the boson fields for which likewise the whole universe must be considered in principle, while in practice a limited number of Fock states may be adequate" (ibid, p.375). On the contrary, in any pair creation or annihilation process the negative energy sea had better play a dynamical role. Any calculation of the probability amplitudes will have to take into account the infinite number of electrons already in situ. Since there is no unitary mapping between the finite-particle states of conventional QED, and the infinite-particle states of the hole theory, we must work from the outset with particle interactions, and with the negative-energy sea.

There is, however, an historical precedent: progress, albeit limited, was made with the hole theory in the early '30s. But there is a final consideration, that may yet tell in favour of Feynman's approach. For at bottom the pilot-wave theory is only of interest insofar as it provides a believable interpretation. Unlike the original Dirac hole theory, whose interest lay for the most part in what it had to say about new phenomena, and in the combining of principles of relativity theory and quantum mechanics, the virtue of the pilot-wave theory is that it solves the problem of measurement. But can we really believe in the literal existence of an infinity of point-particles, in the classical sense, all with negative energy and charge, in every non-zero volume of space? To paraphrase Putnam, speaking of a very different interpretation: what is the point of an interpretation of quantum mechanics that one cannot believe? (Putnam 1990, p.10) But that was not Dirac's view. He, along with most others of the time, took an instrumentalist view of the theory. Only experiment and questions of elegance and mathematical simplicity really mattered to him. It was in that climate of opinion that the hole theory was seriously entertained. That is hardly an outlook available to the pilot-wave theory.

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