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On the Impossible Pilot Wave

J. S. Bell[1]

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The strange story of the von Neumann impossibility proof is recalled, and the even stranger story of later impossibility proofs, and how the impossible was done by de Broglie and Bohm. Morals are drawn.

1. INTRODUCTION

When I was a student I had much difficulty with quantum mechanics. It was comforting to find that even Einstein had had such difficulties for a long time. Indeed they had led him to the heretical conclusion that something was missing in the theory(1): "I am, in fact, rather firmly convinced that the essentially statistical character of contemporary quantum theory is solely to be ascribed to the fact that this (theory) operates with an incomplete description of physical systems."

More explicitly,(2) in "a complete physical description, the statistical quantum theory would ... take an approximately analogous position to the statistical mechanics within the framework of classical mechanics ... ."

Einstein did not seem to know that this possibility, of peaceful coexistence between quantum statistical predictions and a more complete theoretical description, had been disposed of with great rigor by J. von Neumann.(3) I myself did not know von Neumann's demonstration at first hand, for at that time it was available only in German, which I could not read. However I knew of it from the beautiful book by Born,(4) *Natural Philosophy of Cause and Chance*, which was in fact one of the highlights of

[1] CERN, Geneva.

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my physics education. Discussing how physics might develop Born wrote: "I expect ... that we shall have to sacrifice some current ideas and to use still more abstract methods. However these are only opinions. A more concrete contribution to this question has been made by J. v. Neumann in his brilliant book, *Mathematische Grundlagen der Quantenmechanik*. He puts the theory on an axiomatic basis by deriving it from a few postulates of a very plausible and general character, about the properties of "expectation values" (averages) and their representation by mathematical symbols. The result is that the formalism of quantum mechanics is uniquely determined by these axioms; in particular, no concealed parameters can be introduced with the help of which the indeterministic description could be transformed into a deterministic one. Hence if a future theory should be deterministic, it cannot be a modification of the present one but must be essentially different. How this could be possible without sacrificing a whole treasure of well established results I leave to the determinists to worry about."

Having read this, I relegated the question to the back of my mind and got on with more practical things.

But in 1952 I saw the impossible done. It was in papers by David Bohm.[5] Bohm showed explicitly how parameters could indeed be introduced, into nonrelativistic wave mechanics, with the help of which the indeterministic description could be transformed into a deterministic one. More importantly, in my opinion, the subjectivity of the orthodox version, the necessary reference to the "observer," could be eliminated.

Moreover, the essential idea was one that had been advanced already by de Broglie[6] in 1927, in his "pilot wave" picture.

But why then had Born not told me of this "pilot wave?" If only to point out what was wrong with it? Why did von Neumann not consider it? More extraordinarily, why did people go on producing "impossibility" proofs,[7–12] after 1952, and as recently as 1978[13,14]? When even Pauli,[15] Rosenfeld,[16] and Heisenberg,[17] could produce no more devastating criticism of Bohm's version than to brand it as "metaphysical" and "ideological?" Why is the pilot wave picture ignored in text books? Should it not be taught, not as the only way, but as an antidote to the prevailing complacency? To show that vagueness, subjectivity, and indeterminism, are not forced on us by experimental facts, but by deliberate theoretical choice?

I will not attempt here to answer these questions. But, since the pilot wave picture still needs advertising, I will make here another modest attempt to publicize it, hoping that it may fall into the hands of a few of the many to whom even now it will be new. I will try to present the essential idea, which is trivially simple, so compactly, so lucidly, that even some of those who know they will dislike it may go on reading, rather than set the matter aside for another day.

2. A SIMPLE MODEL

Consider a system whose wavefunction has one discrete argument, a, and one continuous argument, x, as well as time, t:

$$\Psi(a, x, t)$$

$$a = 1, 2, \ldots N$$

$$-\infty < x < +\infty$$

It might be a particle free to move in one-dimension and having an "intrinsic spin." Consider "observables" O which involve only the spin, and so can be represented by finite matrices:

$$O\Psi(a, x) = \sum O(a, b)\, \Psi(b, x)$$

To "measure" such an observable, suppose that we can contrive an interaction, with some external field, which is represented by the addition to the Hamiltonian of a term[3]

$$gO(\hbar/i)(\partial/\partial x)$$

where g is a coupling constant. Suppose for simplicity that the particle is infinitely massive, so that this interaction Hamiltonian is the complete Hamiltonian.[3] Then the Schrödinger equation is readily solved. It is convenient to introduce the eigenvectors of O

$$\alpha_n(a)$$

and corresponding eigenvalues

$$O_n$$

defined by

$$O\alpha_n(a) = O_n\, \alpha_n(a)$$

Then the initial state can be expanded

$$\Psi(a, x, o) = \sum_n \Phi_n(x)\, \alpha_n(a)$$

and the solution of the Schrödinger equation is

$$\Psi(a, x, t) = \sum_n \Phi_n(x - gO_n t)\, \alpha_n(a)$$

That is to say, the various wavepackets Φ move apart from one another, and after a sufficiently long time, whatever may have been the case initially, overlap very little. Then any probable result of a position measurement on the particle will correspond to a particular eigenvalue O_n, a particular O_n being obtained with probability given by the norm of the corresponding wavepacket Φ_n, i.e., by the strength of the corresponding eigenvector in the expansion of the initial state. We have here a model of something like a Stern–Gerlach experiment. Conventionally the process is said "to measure observable O with result O_n."

To complete this picture, *a la* de Broglie and Bohm, we add to the wavefunction Ψ a particle position

$$X(t)$$

If a position measurement is made at time t, then the result is $X(t)$, but even when no measurement is made $X(t)$ exists. The particle, in this picture, always has a definite position. The time evolution of particle position is determined by

$$(d/dt)\, X(t) = j(X(t), t)/\rho(X(t), t)$$

where

$$\rho(x, t) = \sum_a \Psi^*(a, x, t)\, \Psi(a, x, t)$$

$$j(x, t) = \sum_{a,b} \Psi^*(a, x, t)\, gO(a, b)\, \Psi(b, x, t)$$

Note that the Schrödinger equation implies the continuity equation

$$(\partial/\partial t)\rho + (\partial/\partial x)\, j = o$$

It is assumed that, over many repetitions of the experiment, various $X(o)$ occur with the probability distribution

$$\rho(X(o), o)\, dX(o)$$

where ρ is given as above in terms of the initial wavefunction. Then it is a theorem that the probability distribution over $X(t)$ is

$$\rho(X(t), t)\, dX(t)$$

This is the conventional quantum distribution for position, and so we have the conventional predictions for the result of the Stern–Gerlach experiment. For the experiment, despite all the talk about "spin," is finally about position observations.

Note that in this theory probability enters once only, in connection with initial conditions, as in classical statistical mechanics. Thereafter the joint evolution of Ψ and X is perfectly deterministic.

Note that in this theory the wavefunction Ψ has the role of a physically real field, as real here as Maxwell's fields were for Maxwell. Quantum mechanics students sometimes have difficulty with the fact that in the pilot wave picture the particle position X and the argument of the wavefunction x are separate variables. But the situation, in this respect, is just that of Maxwell. He also had fields extending over space, and particles located at particular points. Of course the field at the particular point is that most immediately relevant for the motion of the particular particle.

Although Ψ is a real field it does not show up immediately in the result of a single "measurement," but only in the statistics of many such results. It is the de Broglie–Bohm variable X that shows up immediately each time. That X rather than Ψ is historically called a "hidden" variable is a piece of historical silliness.

Note that from the present point of view the description of the experiment as "measurement" of "spin observable" O is an unfortunate one. Our particle has no internal degrees of freedom. It is guided however by a multicomponent field, and when this suffers the analogue of optical multiple refraction, the particle is dragged one way or another depending only on its initial position. We have here a very explicit illustration of the lesson taught by Bohr. Experimental results are products of the complete set-up, "system" plus "apparatus," and should not be regarded as "measurements" of preexisting properties of the "system" alone.

3. THE HOLES IN THE NETS

It is easy to find good reasons for disliking the de Broglie–Bohm picture. Neither de Broglie[18] nor Bohm[19] liked it very much; for both of them it was only a point of departure . Einstein also[20] did not like it very much. He found it "too cheap," although, as Born[20] remarked, "it was quite in line with his own ideas".[21,22] But like it or lump it, it is perfectly conclusive as a counter example to the idea that vaguenes, subjectivity, or indeterminism, are forced on us by the experimental facts covered by nonrelativistic quantum mechanics. What then is wrong with the impossibility proofs? Here I will consider only three of them, the most famous (incontestably), the most instructive (in my opinion), and the most recently published (to my knowledge). More, and more details, can be found elsewhere.[9,23–25]

It will be useful to denote by

$$R(O, \Psi(o), X(o))$$

the result of "measuring" O in the above way, for given initial X and Ψ. This function can be calculated in principle by solving first the Schrödinger equation for Ψ and then solving the guiding equation for X. For some cases this has even been done explicitly.[26,27] Note well that the values taken by R are the eigenvalues of O.

The vital assumption in the famous proof of von Neumann is that, for operators connected by a linear relation,

$$O = pP + qQ$$

the results R are similarly related:

$$R(O, \Psi(o), X(o)) = pR(P, \Psi(o), X(o)) + qR(Q, \Psi(o), X(o))$$

Now this must certainly hold when averaged over $X(o)$ to give quantum expectation values. But it cannot possibly hold before averaging, for the individual results R are eigenvalues, and eigenvalues of linearly related operators are not linear related. For example let P and Q be components of spin angular momentum in perpendicular directions

$$P = S_x, \qquad Q = S_y$$

and let O be the component along an intermediate direction

$$O = (P + Q)/\sqrt{2}$$

In the simple case of spin-1/2, the eigenvalues of O, P, Q, are all of magnitude 1/2, and the von Neumann requirement would read

$$\pm 1/2 = (\pm 1/2 \pm 1/2)/\sqrt{2}$$

—which is impossible indeed. Because the de Broglie–Bohm picture agrees with quantum mechanics in having the eigenvalues as the results of individual measurements—it is excluded by von Neumann. His "very general and plausible" postulate is absurd.

More instructive is the Gleason–Jauch proof. I was told of it by J. M. Jauch in 1963. Not all of the powerful mathematical theorem of Gleason[28] is required, but only a corollary which is easily proved by itself.[9] (The idea was later rediscovered by Kochen and Specker[11]; see also Belinfante[24] and Fine and Teller[29].) Jauch saw that Gleason's theorem implied a result like that of von Neumann but with a weaker additivity assumption—for commuting operators only

$$[P, Q] = o$$

Since the eigenvalues of commuting operators are additive, additivity of the "measurement" results is not manifestly absurd. Perhaps it seems particularly plausible when the commuting "observables" involved are "measured" at the same time. So let us go immediately to that case. It is sufficient to consider a complete set of orthogonal spin projection operators P_n, i.e., a set such that

$$P_n P_m = P_m P_n = P_n \delta_{nm}$$

and

$$\sum_n P_n = 1$$

The eigenvalues of such projection operators are all either zero or unity and, because the operators add to unity, the additivity hypothesis for "measurement" results means simply that on "measurement" one and only one of the operators will give unity, the others giving zero. It is easy to model this situation by an adaptation of the model described above. In the interaction Hamiltonian, gO is replaced by

$$\sum_n g_n P_n$$

The solution of the Schrödinger equation goes through as before in terms of simultaneous eigenvectors α of all the P_n. The various final wavepackets are displaced by distances g_n. The particle is found finally in one of these wavepackets; and, if the g_n are all different, this singles out one of the operators P_n as that for which the result of the "measurement" is unity rather than zero. However the Gleason–Jauch argument depends also on another assumption. For a given operator P_1 it is possible (when the dimension N of the spin space exceeds 2) to find more than one set of other orthogonal projection operators to complete it:

$$\begin{aligned} 1 &= P_1 + P_2 + P_3 \ldots \\ &= P_1 + P_2' + P_3' \ldots \end{aligned}$$

where $P_2' \ldots$ commute with P_1, and with one another, but not with $P_2 \ldots$. And the extra assumption is this: the result of "measuring" P_1 is independent of which complementary set, P_2... or P_2'..., is "measured" at the same time. The de Broglie–Bohm picture does not respect this. Even though the two sets of operators have P_1 in common, the eigenvectors α are different, and the particle orbits $X(t)$ are different, as well as $\Psi(t)$, for given $X(o)$ and $\Psi(o)$. There is nothing unacceptable, or even surprising, about this. The Hamiltonians are different in the two cases. We are doing a different experiment when we arrange to "measure" P_2'... rather than P_2... along with P_1. The apparent freedom of the Gleason–Jauch argument from implausible assumptions about incompatible "observables" is illusory. In denying the Gleason–Jauch independence hypothesis, the de Broglie–Bohm picture illustrates rather the importance of the experimental set-up as a whole, as insisted on by Bohr. The Gleason–Jauch axiom is a denial of Bohr's insight.

The proof of Jost[13] concerns unstable "identical" particles. He remarks that if decay times of similar nuclei were somehow determined in advance, by some parameters additional to the quantum wavefunction, then the nuclei would not be really identical and could not show the appropriate Fermi or Bose statistics. But again the difficulty disappears in the light of the pilot wave picture. The existing nonrelativistic version could not cope with beta decay. But it has no difficulty with alpha decay or fission (or even gamma decay[5]) when the unstable nuclei are regarded as composites of stable protons and neutrons. There is no problem in generalizing the de Broglie–Bohm picture to many particle systems.[5] The wavefunction is just that of ordinary quantum mechanics, and respects the usual symmetry or antisymmetry requirements. The added variables (in the simplest version of the theory[9,30,31]) are just particle positions, and the measured probability distributions of these will be those of quantum mechanics. Recognizing that it is always positions that we are in the end concerned with, all the statistical predictions of quantum mechanics are reproduced. This includes those phenomena associated with "identity of particles".[5] The anticipated difficulty does not arise.

3. MORALS

The first moral of this story is just a practical one. Always test your general reasoning against simple models.

The second moral is that in physics the only observations we must consider are position observations, if only the positions of instrument pointers. It is a great merit of the de Broglie–Bohm picture to force us to consider this fact. If you make axioms, rather than definitions and theorems, about the "measurement" of anything else, then you commit redundancy and risk inconsistency.

A final moral concerns terminology. Why did such serious people take so seriously axioms which now seem so arbitrary? I suspect that they were misled by the pernicious misuse of the word "measurement" in contenporary theory. This word very strongly suggests the ascertaining of some preexisting property of some thing, any instrument involved playing a purely passive role. Quantum experiments are just not like that, as we learned especially from Bohr. The results have to be regarded as the joint product of "system" and "apparatus," the complete experimental set-up. But the misuse of the word "measurement" makes it easy to forget this and then to expect that the "results of measurement" should obey some simple logic in which the apparatus is not mentioned. The resulting difficulties soon show that any such logic is not ordinary logic. It is my impression that the whole vast subject of "Quantum Logic" has arisen in this way from the misuse of a word. I am convinced that the word "measurement" has now been so abused that the field would be significantly advanced by banning its use altogether, in favor for example of the word "experiment."

There are surely other morals to be drawn here, if not by physicists then by historians and sociologists.[32,33]

Of the various impossibility proofs, only those concerned with local causality[34–37] seem now to retain some significance outside special formalisms. The de Broglie–Bohm theory is not a counter example in this case. Indeed it was the explicit representation of quantum nonlocality in that picture which started a new wave of investigation in this area. Let us hope that these analyses also may one day be illuminated, perhaps harshly, by some simple constructive model.

However that may be, long may Louis de Broglie continue to inspire those who suspect that what is proved by impossibility proofs is lack of imagination.

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I am sorry to have missed, before writing the above, an early paper by E. Specker [*Dialectia* **14**, 239 (1960), or in C. A. Hooker, ed., *The Logico-Algebraic Approach to Quantum Mechanics* (Reidel, Dordrecht, 1975), p. 135]. It announced already what I have called the Gleason–Jauch result.

Specker did not know of the work of Gleason, but mentioned rather the possibility of an "elementary geometrical argument"—presumably of the kind that I myself gave later[9] as a preliminary to criticism of the axioms.

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