I discuss a hypothetical historical context in which a Bohm-like deterministic interpretation of the Schrödinger equation could have been proposed before the Born probabilistic interpretation and argue that in such a context the Copenhagen (Bohr) interpretation would probably have never achieved great popularity among physicists.

Is this the real life
Is this just fantasy
Caught in a landslide
No escape from reality

Freddie Mercury, “Boh(e)mian Rhapsody”

I. INTRODUCTION

The Copenhagen interpretation of quantum mechanics (QM) was the first interpretation of QM that achieved a significant recognition among physicists. It was proposed very early by the fathers of QM, especially Bohr and Heisenberg. Later, many other interpretations of QM were proposed, such as statistical ensemble interpretation, Bohm (pilot wave) interpretation, Nelson (stochastic dynamics) interpretation, Ghirardi-Rimini-Weber (spontaneous collapse) interpretation, quantum logic interpretation, information theoretic interpretation, consistent histories interpretation, many-world (relative state) interpretation, relational interpretation, etc. All these interpretations seem to be consistent with experiments, as well as with the minimal pragmatic “shut-up-and-calculate interpretation”. Nevertheless, apart from the minimal pragmatic interpretation, the Copenhagen interpretation still seems to be the dominating one. Is it because this interpretation is the simplest, the most viable, and the most natural one? Or is it just because of the inertia of pragmatic physicists who do not want to waste much time on (for them) irrelevant interpretational issues, so that it is the simplest for them to (uncritically) accept the interpretation to which they were exposed first? I believe that the second answer is closer to the truth. To provide an argument for that, in this essay I argue that if some historical circumstances had been only slightly different, then it would have been very likely that the Bohm deterministic interpretation would have been proposed and accepted first, and consequently, that this interpretation would have been dominating even today\(^1\). (In fact, if the many-world interpretation taken literally is correct, then such an alternative history of QM is not hypothetical at all. Instead, it is explicitly realized in many branches of the whole multi-universe containing a huge number of parallel universes.) For the sake of easier reading, in the next section I no longer use the conditional, but present an alternative hypothetical history of QM as if it really happened, trying to argue that such an alternative history was actually quite natural.\(^2\) Although a prior knowledge on the Bohm deterministic interpretation is not required here, for readers unfamiliar with this interpretation I suggest to read also the original paper\(^3\), or a recent pedagogic review\(^4\).

II. AN ALTERNATIVE HISTORY OF QUANTUM MECHANICS

When Schrödinger discovered his wave equation, the task was to find an interpretation of it. The most obvious interpretation – that electrons are simply waves – was not consistent because it was known that electrons behave as pointlike particles in many experiments. Still, it was known that electrons also obey some wave properties. What was the most natural interpretation of that? Of course, the notion of “naturalness” is highly subjective and strongly depends on personal knowledge, prejudices, and current paradigms. At that time, classical deterministic physics was well understood and accepted, so it was the most natural to try first with an interpretation that maximally resembles the known principles of classical mechanics. In particular, classical mechanics contains only real quantities, so it was very strange that the Schrödinger equation describes a complex wave. Consequently, it was natural to rewrite the Schrödinger equation in terms of real quantities only. The simplest way to do this was to write the complex wave function \(\psi\) in the polar form \(\psi = R e^{i\phi}\) and then to write the complex Schrödinger equation as a set of two (coupled) real equations for \(R(x,t)\) and \(\phi(x,t)\). However, such a simple mathematical manipulation did not immediately reveal the physical interpretation of \(R\) and \(\phi\). Fortunately, a physical interpretation was revealed very soon, after an additional mathematical transformation

\[
\phi(x,t) = \frac{S(x,t)}{\hbar},
\]
where $S$ is some new function. The Schrödinger equation for $\psi$ rewritten in terms of $R$ and $S$ turns out to look remarkably similar to something very familiar from classical mechanics. One equation looks similar to the classical Hamilton-Jacobi equation for the function $S(x, t)$, differing from it only by a transformation

$$ V(x, t) \to V(x, t) + Q(x, t), $$

where $V$ is the classical potential and

$$ Q \equiv -\frac{\hbar^2}{2m} \nabla^2 \frac{R}{R}. $$

The other equation turns out to look exactly like the continuity equation

$$ \frac{\partial \rho}{\partial t} + \nabla (\rho v) = 0 $$

for the density $\rho \equiv R^2$, with the Hamilton-Jacobi velocity

$$ v = \frac{\nabla S}{m}. $$

Thus, at that moment, the most natural interpretation of the phase of the wave function seemed to be a quantum version of the Hamilton-Jacobi function that determines the velocity of a pointlike particle. But what was $\rho$? Since one of the equations looks just like the continuity equation, at the beginning it was proposed that $\rho$ was the density of particles. That meant that the Schrödinger equation described a fluid consisting of a huge number of particles. The forces on these particles depended not only on the classical potential $V$, but also on the density $\rho$ through the quantum potential $\mathcal{Q}$ in which $R = \sqrt{\rho m^2}$.

Although the interpretation above seemed appealing theoretically, it was very soon realized that it was not consistent with experiments. It could not explain why, in experiments, only one localized particle at a single position was often observed. Thus, $\rho$ could not be the density of a fluid. It seemed that $\rho$ (or $R$) must be an independent continuous field, qualitatively similar to an electromagnetic or a gravitational field, that, similarly to an electromagnetic or a gravitational field, influences the motion of a particle. But why does $\rho$ satisfy the continuity equation, what is the meaning of this? They could not answer this question, but they were able to identify a physical consequence of the continuity equation. To see this, assume that one studies a statistical ensemble of particles with the probability distribution of particle positions equal to some function $p(x, t)$. Assume also that, for some reason, the initial distribution at $t = 0$ coincides with the function $\rho$ at $t = 0$. Then the continuity equation implies that

$$ p(x, t) = \rho(x, t) $$

at any $t$. But why should these two functions coincide initially? Although nobody was able to present an absolutely convincing explanation, at least some heuristic arguments were found, based on statistical arguments. This suggested that, in typical experiments, $\rho$ could be equal to the measured probability density of particle positions. Indeed, it turned out that such a prediction agrees with experiments. Since this prediction was derived from the natural assumption that each particle has the velocity determined by (4), it was concluded that experiments confirm (4). Thus, this interpretation became widely accepted and received the status of an “orthodox” interpretation.

However, not everybody was satisfied with this interpretation. In particular, Born objected that there was no direct experimental evidence for the particle velocities as given by (4), so this assumption was questioned by him. As an alternative, he proposed a different interpretation. In his interpretation, the equality (4) was a fundamental postulate. Thus, he avoided a need for particle velocities as given by (4). However, his interpretation has not been widely accepted among physicists. The arguments against the Born interpretation were the following: First, this ad hoc postulate could not explain why the probability density was given by $\rho$. Second, a theory in which the probabilistic interpretation was one of the fundamental postulates was completely against all current knowledge about fundamental laws of physics. The classical deterministic laws were well established, so it was more natural to accept a deterministic interpretation of QM that differs from classical mechanics less radically. Third, it was observed that if one used the arguments of Born to argue that QM is to be interpreted probabilistically, then one could use analogous arguments to argue that even classical mechanics should be interpreted probabilistically, which seemed absurd.

Although the Born purely probabilistic interpretation was not considered very appealing, mainly owing to the overwhelming mechanistic view of physics of that time, it was appreciated by some positivists that such an interpretation should not be excluded. The Born interpretation was quite radical, but still acceptable as a possible alternative. Indeed, his interpretation seemed to fit well with a mathematically more abstract formulation of QM (which started with the Heisenberg matrix formulation of QM proposed even before the Schrödinger equation, and was further developed by Dirac who formulated the transformation theory and von Neumann who developed the Hilbert-space formulation), in which Eq. (4) did not seem very natural. However, one version of the Born interpretation was much more radical, in fact too radical to be taken seriously. This new interpretation was suggested by Bohr. In fact, Bohr was already known in the physics community for proposing the famous Bohr model of the hydrogen atom, in which electrons move circularly at discrete distances from the nucleus. Now a much better model of the hydrogen atom (the one based on the Schrödinger equation and particle trajectories that it predicts) was known, so the Bohr model was no longer considered that important, although it still enjoyed a certain respect. Since the model by which Bohr achieved respect...
among physicists was based on particle trajectories, it was really a surprise when Bohr in his new interpretation proposed that particle trajectories did not exist at all. But this was not the most radical part of his interpretation. The most radical part was the following: he proposed that it did not even make sense to talk about particle properties unless these properties were measured. An immediate argument against such a proposal was the well-established classical mechanics, in which particle properties clearly existed even without measurements. Bohr argued that there was a separation between the microscopic quantum world and the macroscopic classical world, so that the measurement-independent properties made sense only in the latter. However, Bohr never explained how and where this separation took place. In his interpretation, he introduced no new equation. His arguments were considered pure philosophy, not physics. Although his arguments were partially inspired by the widely accepted Heisenberg uncertainty relations, the orthodox interpretation of the uncertainty relations (expressing practical limitations on experiments, rather than properties of nature itself) seemed more viable. Thus, it is not a surprise that his interpretation has never been taken seriously. His interpretation was soon forgotten. (Much later it was found that the mechanism of decoherence through the interaction with the environment provides a sort of dynamical separation between “classical” and “quantum” worlds, but this separation was not exactly what Bohr suggested.)

Another prominent physicist who criticized the orthodox interpretation of QM was Einstein. He liked the deterministicism of orthodox QM (despite the fact he made contributions to the probabilistic descriptions of quantum processes such as spontaneous emission and photoelectric effect), but there was something else that was bothering him. To see what, consider a system containing $n$ particles with positions $x_1, \ldots, x_n$ described by a single wave function $\psi(x_1, \ldots, x_n, t)$. The $n$-particle analog of (3) is a nonlocal function of the form $Q(x_1, \ldots, x_n, t)$. In general it is a truly nonlocal function, i.e., not of the form $Q_1(x_1, t) + \ldots + Q_n(x_n, t)$, provided that the system exhibits entanglement, i.e., that the wave function is not of the form $\psi_1(x_1, t) \cdot \psi_n(x_n, t)$. In the orthodox interpretation such nonlocal $Q$ is interpreted as a nonlocal potential that determines forces on particles that depend on instantaneous positions of all other particles. This means that entangled spatially separated particles must communicate instantaneously. Einstein argued that this is in contradiction with his theory of relativity, because he derived that no signal can exceed the velocity of light. Orthodox quantum physicists admitted that this is a problem for their interpretation, but soon they found a solution. They observed that the geometric formulation of relativity does not really exclude superluminal velocities, unless some additional properties of matter are assumed. Thus, they introduced the notion of tachyons, hypothetical particles that can move faster than light and still obey the geometrical principles of relativity. Einstein admitted that tachyons are consistent with relativity, but he objected that this is not sufficient to solve the problem of instantaneous communication. If the communication is instantaneous, then it can be so only in one reference frame. This means that there must be a preferred reference frame with respect to which the communication is instantaneous, which again contradicts the principle of relativity according to which all reference frames should enjoy the same rights. At that time orthodox quantum physicists understood relativity sufficiently well to appreciate that Einstein was right. On the other hand, the theory of relativity was also sufficiently young at that time, so that it did not seem too heretic to modify or reinterpret the theory of relativity itself. It was observed that with a preferred foliation of spacetime specified by a fixed timelike vector $n^\mu$ one can still write all quantum equations in a relativistic covariant form. It was also observed that, by using an analogy with nonrelativistic fluids, relativity may correspond only to a low-energy approximation of a theory with a fundamental preferred time. Thus, it was clear that the preferred foliation of spacetime does not necessarily contradict the theory of relativity (both special and general), provided that the theory of relativity is viewed as an effective theory. At the beginning, Einstein was not very happy with the idea that his theory of relativity might not be as fundamental as he thought. Nevertheless, he finally accepted that QM is irreducibly nonlocal when he was confronted with the rigorous mathematical proof that, in QM, the assumption of reality existing even without measurements is not compatible with locality.

A new crisis for orthodox QM arose with the development of quantum field theory (QFT). At the classical level, fields are objects very different from particles. As QFT seemed to be a theory more fundamental than particle QM, it seemed natural to replace the quantum particle trajectories with the quantum field trajectories (or more precisely, time-dependent field configurations). However, there were two problems with this. First, from the trajectories of fields, it was not possible to reproduce the trajectories of particles. Second, the idea of field trajectories did not seem to work for fermionic (anticommuting) fields. Still, the agreement with experiments was not ruined, as all measurable predictions of QFT were actually predictions on the properties of particles. Therefore, it seemed natural to interpret QFT not as a theory of new more fundamental objects (the fields), but rather as a more accurate effective theory of particles, in which fields play only an auxiliary role. Indeed, the divergences typical of QFT reinforced the view that QFT cannot be the final theory, but only an effective one.

As quantum physics made further progress, it became clear that many theories that were considered fundamental at the beginning turned out to be merely effective theories. This reinforced the dominating paradigm according to which relativity is also an effective, approximate theory. Nevertheless, some relativists still believed that the principle of relativity was a fundamental principle.
Consequently, they were not satisfied with the orthodox interpretation of QM that requires a preferred foliation of spacetime. Instead they were trying to interpret QM in a completely local and relativistic manner. To do that, they were forced to introduce some rather radical views of nature. In one way or another, they were forced to assume that a single objective reality did not exist. However, such radical interpretations were not very appreciated by the mainstream physicists. It did not seem reasonable to crucify one of the cornerstones not only of physics but of the whole of science (the existence of objective reality) just to save one relatively new theoretical principle (the principle of locality and relativity) for which there existed good evidence that it could be only an approximate principle. Therefore, the deterministic interpretation of QM survived as the dominating paradigm, while the probabilistic rules of QM, used widely in practical phenomenological calculations, were considered emergent, not fundamental. In fact, it has been found that, in some cases, the probabilistic rules cannot be derived in a simple way, so that one is forced to use the fundamental fully deterministic theory explicitly.

III. CONCLUSION

In this paper, I have argued that, in the context of scientific paradigms that were widely accepted when the Schrödinger equation was discovered, it was much more natural to propose and accept the Bohmian deterministic interpretation than the Copenhagen interpretation. I have also argued that, if that had really happened, then the Bohmian interpretation (or a minor modification of it) would have been a dominating view even today. In other words, the answer to the allegoric tongue-twisting question posed in the title of this paper is – probably no! This, of course, does not prove that the Bohmian interpretation is more likely to be correct than some other interpretation. But the point is that it really seems surprising that the history of QM chose a path in which the Copenhagen interpretation became much more accepted than the Bohmian one. I leave it to the sociologists and historians of science to explain why the history of QM chose the path that it did.

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1 A similar thesis with somewhat different arguments has been also advocated in J. T. Cushing, Quantum Mechanics: Historical Contingency and the Copenhagen Hege mony (University of Chicago Press, Chicago, 1994).
2 Remarks concerning the actual history of QM are given in references.
3 D. Bohm, “A suggested interpretation of the quantum theory in terms of “hidden variables”. I,” Phys. Rev. 85 (2), 166-179 (1952); D. Bohm, “A suggested interpretation of the quantum theory in terms of “hidden variables”. II,” Phys. Rev. 85 (2), 180-193 (1952).
4 R. Tumulka, “Understanding Bohmian mechanics: A dialogue,” Am. J. Phys. 72 (9), 1220-1226 (2004).
5 Such an interpretation was really proposed already in 1926: E. Madelung, Z. Phys. 40, 322-326 (1926).
6 These arguments might have looked similar to those in D. Dürr, S. Goldstein, and N. Zanghì, “Quantum equilibrium and the origin of absolute uncertainty,” J. Stat. Phys. 67, 843-907 (1992); A. Valentini, “Signal-locality, uncertainty, and the subquantum H-theorem,” Phys. Lett. A 156, 5-11 (1991).
7 In reality, this interpretation is known today as the Bohm interpretation, while the status of an “orthodox” interpretation is enjoyed by a significantly different interpretation. De Broglie has also proposed the same equation for particle trajectories much earlier than Bohm did, but de Broglie did not develop a theory of quantum measurements, so he could not reproduce the predictions of standard QM for observables other than particle positions, such as particle momenta. For more historical details see also G. Bacciagaluppi and A. Valentini, Quantum Theory at the Crossroads: Reconsidering the 1927 Solvay Conference (to be published by Cambridge University Press); quant-ph/0609184
8 Such arguments might have looked similar to those in H. Nikolić, “Classical mechanics without determinism,” Found. Phys. Lett. 19, 553-566 (2006). In this paper, it is shown that classical statistical physics can be represented by a nonlinear modification of the Schrödinger equation, in which classical particle trajectories may be identified with special solitonic solutions. A Bohr-like interpretation of general (not solitonic) solutions suggests that even classical particles may not have trajectories when they are not measured, while a measurement of the previously unknown position may induce an indeterministic wave-function collapse to a solitonic state.
9 For a review of the theory of decoherence with emphasis on the interpretational issues, see M. Schlosshauer, “Decoherence, the measurement problem, and interpretations of quantum mechanics,” Rev. Mod. Phys. 76, 1267-1305 (2004).
10 In reality, tachyons have been introduced in physics somewhat later, see O. M. P. Bilaniuk, V. K. Deshpande, and E. C. G. Sudarshan, “Meta” relativity,” Am. J. Phys. 30 (10), 718-723 (1962); O. M. P. Bilaniuk and E. C. G. Sudarshan, “Particles beyond the light barrier,” Physics Today 22 (5), 43-51 (1969).
It is well known that a wave equation describing the propagation of sound with the velocity \( c_s \) in a fluid has the same mathematical form as a special-relativistic wave equation describing the propagation of light with the velocity \( c \) in vacuum. Consequently, such a wave equation of sound is invariant with respect to Lorentz transformations in which the velocity \( c \) is replaced by \( c_s \). A fluid analogy of curved spacetime may also be constructed, by introducing an inhomogeneous fluid. For more details, see, e.g., M. Visser, “Acoustic black holes: horizons, ergospheres, and Hawking radiation,” Class. Quant. Grav. 15, 1767-1791 (1998).

This proof is now usually attributed to Bell, although other versions of this proof also exist. For a pedagogic review see F. Laloë, “Do we really understand quantum mechanics? Strange correlations, paradoxes, and theorems,” Am. J. Phys. 69 (6), 655-701 (2001).

Many of the current interpretations of QM mentioned in the introduction are of this form.

String theory also contains evidence against locality at the fundamental level. Although the theory is originally formulated as a local theory, nonlocal features arise in a rather surprising and counterintuitive manner. It turns out that string theories defined on different background spacetimes may be mathematically equivalent, which suggests that spacetime is not fundamental at all. Without a fundamental notion of spacetime, there is no fundamental notion of locality and relativity as well. It is believed that a more fundamental formulation of string theory should remove locality more explicitly, while known local laws of field theory should emerge as an approximation. See, e.g., G. T. Horowitz, “Spacetime in String Theory,” New J. Phys. 7, 201 (2005); N. Seiberg, “Emergent Spacetime,” hep-th/0601234

It is known that relativistic QM based on the Klein-Gordon equation, as well as QFT, do not contain a position operator. Therefore, the conventional interpretation of quantum theory does not have clear predictions on probabilities of particle positions in the relativistic regime. The fundamentally deterministic Bohmian interpretation may lead to clearer predictions, which means that it may be empirically richer than (and thus inequivalent to) the conventional formulation. For more details, see, e.g., H. Nikolić, “Relativistic quantum mechanics and the Bohmian interpretation,” Found. Phys. Lett. 18, 549-561 (2005); H. Nikolić, “Is quantum field theory a genuine quantum theory? Foundational insights on particles and strings,” arXiv:0705.3542

Unfortunately, experiments that could confirm or reject such a formulation have not yet been performed. It is also fair to note that today such a version of the Bohmian interpretation not empirically equivalent to the conventional interpretation is considered controversial even among the proponents of the Bohmian interpretation. Nevertheless, in an alternative history of QM in which the conventional probabilistic interpretation never became widely accepted, such a fundamentally deterministic Bohmian interpretation might have seemed more natural.