Review of: "On Quantum Superposition"

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Potential competing interests: No potential competing interests to declare.

The author reviews some basic concepts of quantum physics, represents them in a very obscure and uninformed way, and draws wrong conclusions from that about quantum experiments, quantum superposition, and quantum entanglement. This paper should not be published in any form in any journal except maybe as a lesson to the reader what a scientific paper should not be like.

Already in the abstract, and very often throughout the manuscript the author claims that it is a "well established mathematical fact" that it is impossible to "obtain precise coordinates". What the author means by that becomes a bit clearer in a later on page 5 of the manuscript where the author seems to "explain" that "well established mathematical fact" they like to mention. Essentially his argument is that if there is some non-zero precision γ with which one can know a coordinate, then one cannot know the position $\r$ with arbitrary precision. That seems to be a very circular definition. It seems the problem of the author with quantum physics originates from some problem the author has with using multi-dimensional real numbers as a coordinate space.

Of course, no physicist and nobody working in quantum physics would claim that one can measure positions or times with arbitrary precision. There also have been many works considering a fundamental uncertainty in how well we can define a position in space-time or metric fluctuations, a granularity of spacetime etc. The author's approach however oversimplified, and they draw very questionable conclusions resulting from their oversimplified arguments.

On page 6, the author introduces the superposition principle in a strange way, they first refer to it in the context of waves and the solution of some differential equation. Probably the author refers to the Maxwell equations of so. Quantum states, of course, can also be the energy eigenstates or similar. This depends on the situation one is dealing with. Superpositions are, effectively, just a result from choosing different basis states. For example, the 45-deg polarized state is a superposition of horizontal and vertical polarization in the horizontal/vertical basis, but it is a basis state in the diagonal/anti-diagonal polarization basis. Any superposition state is again a pure state, and one can define a basis in which it is not a superposition. In collapse theories or similar, preference is given to the position basis, and for that reason such collapse theories then often assume that there will be some kind of "collapse" to a position state. In the context of quantum Darwinism and similar approaches, it has been argued that the position basis may indeed be preferred because it is more resilient against the dominant decoherence mechanisms.

The central argument that the rest of the paper is based on is presented on page 7, but that whole argument is fundamentally flawed. After equation (3), the author emphasizes that it is an "assumption" that all the ψ_i states refer to the same particle. Since those states are the solutions for a wave-equation describing the evolution of one particle, obiously, these states refer to the same particle. It is quite useless to call that an assumption. The states would not be in the same Hilbert space otherwise.

The author then claims that "the same single quantum object can at most be measured only once". Of course we can measure a quantum object as many times as we want, as long as the object is not destroyed. For example, if we do a photoelectric detection of a photon, the photon will be gone afterwards. If we are dealing with a bead of material or similar, that bead will usually still be around after a measurement, and we can measure (for example) the position of the bead however many times we want. However, the quantum state describing the position of our bead will be different after each measurement because our knowledge is updated continuously by the measurements. Maybe the author confuses quantum states with quantum objects. The quantum state represents the experimenter's prior knowledge about a quantum object. If one performs a measurement, one will gain information, and therfore one will have a different prior knowledge before a succeeding measurement. To get the quantum object in the same quantum state again, one would wither need to project the experimenter on the same state again, or the experimenter has to repeat the experiment with an identical object under the same conditons. This is how experiments usually work.

In addition to the things I noted above already, the author (without any clear justification) says that Born's rule does not include an uncertainty in time. Well, that is because the author presented Born's rule here for a precisely defined moment in time. If there is uncertainty in time, one has to take that properly into account. However, is the uncertainty in time is much smaller than the time scale on which the quantum state evolves in time, one can usually ignore that to very high precision. Instead of doing that, the author immediately draws wild conclusions about the superposition principle not making sense because a time uncertainty is missing in the author's representation of Born's rule.

A very bold statement of the author is that the "majority of physicists ... believe, without any evidence, decoherence making effects of quantum superposition disappear beyond the level of quantum objects". Did the author make a world-wide poll about the inner beliefs of quantum physicists? Who says that this is "without any evidence"? I have been working in experimental quantum physics for a long time, and I have seen tons of evidence that we can realize macroscopic superpositions. For example, take the many experiments by the group of Markus Arndt, the increasingly macroscopic experiments using quantum optomechanics, etc. Quite in contrast to what the author claims, an incredible number of experiments have confirmed the quantum superposition principle for increasingly massive test objects over the last century.

On page 8, the author continues drawing audacious conclusions based on the wrong argument on page 7. Once again, the author claims that we can only detect each particle once. Like above, the author seems to confused quantum states with quantum objects, but they now switched to talking about individual particles. The author states "all instants t_k" are unknown to us, but t_k are (by their own definition) the times at which particles k are measured. Of course, one cannot measure times to arbitrary precisison. One can see that as a consequence of time-energy uncertainty, which is closely related to Fourier transforms. This is very similar to not being able to measure points in space with arbitrary precision, but time and space are treated in very ditinc ways in non-relativistic quantum physics.

In the remainder of the paper the author considers spin superpositions and their measurement as well recent Bell tests etc in the light of their flawed arguments. For example, the author provides a round-about argument on page (9) that spins are also not perfectly defined (just like he argued for positions and times), and therefore one should not be surprised if one sometimes gets varying outputs in an experiment. Indeed, no experimenter would be surprised. But just because one cannot perfectly prepare a spin state does not explain all results one could possibly have in experiments with superpositions of spin states - for example the interference in a Ramsey-Bordé atom interferometer or similar.

Later, on page 13, the authour also aims to "explain" quantum randomness by their approach of times and position not being arbitraily well defined. Well, yes, like I said earlier: everybody knows that, and we also always take it into account in experiments. But just because we do not know our measurement time better than 1e-21s or something, does not mean that a quantum state thay maybe only evolves noticably over a timescale of a ms or so (depending on the system one considers), will then suddenly give other outcomes and therefore "intuitively" explain quantum randomness. For example, a kHz-bandwidth photon will not randomly take one or the other output of a beam splitter because I do not know the arrival time of the photon at the beam splitter with arbitrary precision. In short, the author's arguments are flawed, and even if they were less flawed, they would still explain nothing.