Review of: "Consistent Interpretation of Quantum and Classical Mechanics"

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Potential competing interests: No potential competing interests to declare.

Interpretation of Quantum Mechanics is a fully legitimate field of research; I personally understand the Author's (apparent) frustration with the 'orthodox interpretation' (for lack of a better word), a phrase which is usually taken to mean some variation of the Copenhagen interpretation. It is also a difficult field, not only because, to quote the Author:
"… it means explaining why and how the mathematics exploited to formulate wave-particle duality are related to observations or reality in classical physics (due to Newtonian mechanics, special relativity, gravity and entropy",
but for at least two additional reasons:

1. The existing literature on the subject is vast: the debate has been open since the first formalizations of Quantum Mechanics in the '20s of last century to our days;
2. It is often difficult to ascertain the relevance of an interpretation to the actual everyday practice of physics, that is, to actual experiments and data.

After this introduction, I must say I appreciate the earnestness of the Author in sharing his view on this complex topic, and I at least in part agree with some of his statements. However, unfortunately, I find multiple issues with the actual content of the paper, some of them fundamental.

Perhaps the main issue, from which I suspect most of the other ones follow, **is a serious lack of references**. As I already said, the literature on the problem is vast; however, the introduction only cites a very limited selection of papers and textbooks. This can be appropriate for an undergraduate course, but certainly not for a research paper, or even a review paper – especially it seems that not much of the material from recent and advanced sources – such as Jaeger's "Entanglement, Information, and the Interpretation of Quantum Mechanics", the paper's citation **[3]** has been included in the paper's text, despite the citation. In other terms, it seems to me that many of the Author's statements suffer from the classic strawman's flaw: he attacks views on Quantum Mechanics which are dated, misrepresented or oversimplified. An example of misrepresentation taken from the text is the comment: "This is the core Copenhagen-interpretation that was advocated, and *enforced to be the truth* by von Neumann and Dirac" (my emphasis). How did Dirac and Von Neumann exactly *enforce* a truth on the community? How many physicists today actually believe the Copenhagen interpretation to be "the truth"?

A further, related issue is language. In several points the Author's language appears too strong, too emphatic. There multiple instances of this: I'll highlight only a selection of them in order to make my point:

"… *proper and correct* physics and mathematics needed to take a firm stand…" (my emphasis): what is the need of stressing that the content of the paper is *proper and correct*? Surely this must be evaluated by the community?

"The above revised (1, 4, 5, 6 and 8) and Copenhagen (2, 3 and 7) postulates *strictly imply* that quantum mechanics is an ontic theory…": since these are *postulates*, and they are chosen by the Author in this very publication, their 'strict implications' are actually *assumptions* – their relevance to actual physics is yet to be established.

"The realist position is the only valid position in classical mechanics such that the agnostic position is meaningless, while the orthodox position is simply nonsensical.": this statement is both strongly-worded and ambiguous, since all of this positions are approaches to the interpretation of *Quantum Mechanics* – it is unclear what their relevance should be for *Classical* Mechanics.

Another important issue is the uneven level of mathematical rigor. I would expect a high level of precision in the statements and the definitions building up a paper which discusses the fundamentals of Quantum Mechanics. Yet the definitions in this paper are often unclear or addres specific examples rather than general cases.

For instance, the page-long discussion of the Uncertainty Principle is honestly puzzling, and after a couple of full re-reads I am left uncertain to what it adds to the current, well-established understanding of the Principle itself .

Another example is the Author's version of 'Postulate 1':

"The state function, Ψ(r, t), which is a function of time and space coordinates, is also known as the wavefunction. However, Ψ(r, t) for massive and charged quantum particles does contain all the observable or measurable information about a system except for the particle's mass, charge and the interactions in that system. We also demand that Ψ(r, t) to be single-valued (uniquely determines the eigenvalue), continuous and quadratically integrable. For continuum states, quadratic integrability should be excluded."

This parapraph intermingles specific statements ('charged and massive particles') with general ones with an unclear logic, introduces terms without definition (what are 'continuum states'? While I do know what they are, shouldn't they be introduced explicitly, and not taken for granted, in a reformulation of Quantum Mechanics? And is Postulate 1 really the most appropriate point of introducing them?), and is generally, in my personal opinion, a bit confusingly worded. Contrast this with – for instance – Jaeger's definition of Postulate 1 from citation **[3]**:

"Each physical system is represented by a Hilbert space and described by physical quantities and a state represented by linear operators in that space. The Hilbert space is usually taken to be complex projective Hilbert space. The treatment of the motion of quantum particles in space requires the use of an infinite-dimensional separable Hilbert spaces."

3 simple and general sentences, only using terms with a well-defined mathematical meaning, plus the basic physical

notions of ‘system’ and ‘particle’. Nothing else.

As a final example, consider the Author's ‘physical condition’ (C2):

“(C2) Whether the said quantum mechanical observables are also observable in classical physics. *Observable in classical physics* means a quantum mechanical observableis well-defined at any given time t_n.” (my emphasis)

This definition of “observable in classical physics” is puzzling to me. Consider a fully polarized (with any chosen polarization) beam of electrons, which can be produced in several ways in an actual laboratory. I would argue that the spin of a generic electron in the beam is well-defined at any time; however I wouldn't say that the spin of an electron is “observable in classical physics”. Again, it is not clear enough what is the actual point of the Author in making this definition.

Yet another issue are statements in the paper which are – as far as I can determine – **seriously misleading when not plainly incorrect**. These are the most important I could detect:

A minor mistake (surely a typo), yet important, in eq. (3) – Schroedinger's equation. Here the *reduced* Planck's Constant $\hbar$ should be used in place of h.

In ‘Postulate 2’: “Each linear Hermitian operator corresponds to a physical observable…” – this is actually the other way around; citing **[3]** again: “Each physical quantity of a quantum system is represented by a positive Hermitian operator O…”. As far as I know, not every Hermitian operator in the Hilbert space of a generic physical system needs correspond to an observable.

In ‘Postulate 5’: “If Ψ(r, t) is orthonormalized and well-behaved…” – a single state can be *normalized*, but how does one ortho-normalized it? Orthogonalization requires a *set* of states.

In ‘Postulate 6’: “The state function is separable into products…” and eq. (6) that follows. A generic wave function is most definitely not separable: eq. (6) should include a *sum* over the quantum numbers and the both the R function and the eigenvalue E will in general depend on the quantum numbers. While this can be implicit in the formula, it should at least be stated in the text.

I am left with the suspicion that a further thorough re-read of the paper may yield further similarly misleading/incorrect statements, and I urge the Author to double-check all his definitions, his formulas, and the specific wording of his postulates.

Finally, the new Postulates proposed in the paper have multiple issues in particular, they are not general enough, the need for their introduction is unclear and/or they are made up of generic statemens that are in need of further proof and motivation.

For instance: in the standard account, this paper's ‘Postulate 7’ is actually a *consequence* of the fact that electrons are mutually indistinguishable.

‘Postulate 8’ can similarly be derived by general considerations on angular momentum.

‘Postulate 9’ may possibly have interesting content, but it is unclearly worded and deals with examples (photon absorption or emission by bound-state electrons) rather than with a general situation.

‘Postulates 10-13’ resemble more generic statements on physics than actual postulates (as in: statements on the mathematical formulation of Quantum Mechanics and its relation with the description of physical reallity). Overall, they are neither fully condivisible nor fully understandable (at least in my view) without extensive further clarification. 'Postulate 13' in particular is basically a self-citation, which furthermore refers to a preprint.

In the end, I would strongly advise agains publication of this paper in its present form. I think the paper needs major and essential revision in order to be a useful contribution to the literature.

In particular it needs:

Additional references, especially recent ones dealing in depth with approaches to the measurement problems. Appropriate content from these citation should be incorporated in the text – especially the Introduction and Conclusion -- and the need and usefulness of the proposed new/revised principles and postulates should be weighed against it.

Strong wording such as ‘proper and correct’, ‘the only valid’, ‘nonsensical’ and so on should be avoided in favour of more open, tentative alternatives.

All formulas and definitions should be carefully revised and a double-checked for errors or imprecisions should be executed, especially in the reformulation of existing Postulates and in the writing down of well-known equations.

The wording of the Author's version of the Postulates should be extensively revised. The postulates should be as clear and concise as possible, clearly separating examples from general statements, and they should fully address (or at least hint at) the general cases where they don't already. The motivation for the new postulates should be clarified, and experimental consequences or possible limitations much more fully explored. ‘Postulate 7’ and ‘Postulate 8’ are most likely not needed.