Review of: "Emergent Quantum Mechanics – How the Classical Laws Can Replicate the Quantum Harmonic Oscillator"

Andrei Ludu[1]

1 Embry-Riddle Aeronautical University

Potential competing interests: No potential competing interests to declare.

The paper tries to present a model for some elementary particles from a perspective different from traditional quantum mechanics. Large parts of the paper contain text on history of science, and about models that have been abandoned long time ago, in a mixture with some more modern references. Some other parts of the paper refer to an original model of the authors, but unfortunately these two different approaches are mixed throughout of the paper and makes very difficult to understand if the paper is rather about history and teaching or it has some original contributions to science. Very confusing way of writing. We would suggest the authors to separate the manuscript in 3 different parts: one about the goal of the paper and what problems tries to solve and why there is a need to solve that problem, clearly and briefly stated. A second introductory part with history and motivations and showing what doesn't work at present and why is a need to change the existing models. And finally, a third part with the original model, explained along the lines of: hypotheses, equations, solutions, conclusions, comparison with other models and/or with experiments.

In the very beginning, the Abstract contains very strange wording and non-physical concepts, not to say straight wrong: there is no "Newton/Einstein dynamics", it is either Newton (classical) or Einstein (relativistic). Maxwell equations are included in Einstein theory and conversely. The wording is very unclear. Finally, the authors should rather keep on the scientific modesty side let the readers, and reviewers say if the so called "Em.QM" new revolutionary theory is indeed simpler and more transparent.

The paper is abundant in wrong physical affirmations, or at least very confusing and not founded ones. For example, and page 5 we read:

A. "The model has two degrees of freedom: the variable position of the midpoint ( ), and the variable elongation ( )…" and then
B. "… A quasi-static deformation mode is assumed…" and then:
C. "…stretching … takes place as in a spring without dynamic effects…" Finally, we read:
D. "…A final assumption is that the droplet has an elongated shape in the Z-direction…".

Are these affirmations hypotheses? Are them going to be proved or justified later? It is not clear. Why 2 DOF and not less or not more as is mentioned in (A)? Why not 3? Or in (B) is assumed something which is not even defined: quasi-static with respect to what? To what time scale? And so on. If the authors explain these hypotheses later, this should be

mentioned here.

The word “instationary” doesn’t exists in physics.

Does section 4.2 go anywhere in the text, or is just mentioned for historical motivation? If it is the later, this should be placed in some introduction, or removed.

What is delta(t) in eq. (6)?

What is the exact definition of this fictional “cross-coupling” force? There is no explanation of it.

In section 5.2 the electrostatic repulsion, which is a potential force, is introduced as generalized force. Why?

The first paragraph in section 5.3 has no explanation or motivation. The whole section 5.3 is anything but physics.

I will jump to my last comment, because the paper is so dense in historical facts combined with very loose and unjustified hypotheses that it makes the paper unreadable. Eq. (41) which seems to be the main original part comes from Eq. (28) where the non-homogenous terms is dropped. Why? It is not justified. At this point, the result is just a simple mathematical exercise and not a physical result.

The main result of the paper is supposed to be the quantification of the energy of some oscillating electrically charged drop. Energy, or at least what the letter E means here, because the authors change the concepts and juggles with definitions for energy in any direction in this paper. So, the author imposes and external parabolic potential such that the averaged motion of the drop is periodic and has angular frequency Omega. Next, another internal degree of freedom “delta s” is added and after several layers of simplifications and approximations, it is shown that this degree of freedom has also an oscillating motion, modulated by the external potential. The main central hypothesis here is that the complete motion should be periodic. We ask, why? Is there any reason, or is there any problem if the internal DOF of the droplet is not in phase with the average motion of its COM? Anyway, by asking this periodicity, of course the two frequencies of the two independent DOF should be commensurate, and hence their ration be integer, n. This is absolutely no quantization process.

Philosophically, this paper wants to substitute the (well established, very clear, correct, closed and exact, million of times proved real by experiments) quantum theory of harmonic oscillator with a very fuzzy fluid-relativistic-Maxwell-radiation complicated invention, just to show that the eliminated theory was right? I.e. the equidistant levels of quantization of energy of HO? It doesn’t make any sense.

I would suggest the authors the following:

1. Separate the historical and teaching and presentational part in a short introduction;
2. Eliminate from the present manuscript anything is not really used in deduction of the original result;
3. Present clearly what is the result. If there is any.

Our reviewer’s conclusion is to reject such a paper from publication.