Global weakness of the paper: "Mass-energy equivalence Extension onto a Superfluid Quantum Vacuum"

i.e. why the so-called "model" introduced is in this form not viable

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Abstract

With these comments, I wish to focus on the problems encountered in the paper: Šorli, A.S. Mass - Energy Equivalence Extension onto a Superfluid Quantum Vacuum. Sci Rep 9, 11737 (2019). https://www.nature.com/articles/s41598-019-48018-2. The paper denotes problems of conceptual, technical and aesthetic nature, and brings as support of a possible "introduced model", as indicated there, extremely poor mathematical concepts, confused ideas and concepts in general, inadequate qualitative figures, everything presented in a questionable English. It follows an analysis of the critical found points.

Keywords: (Superfluid Quantum) Vacuum; Space; Mass; Energy; Mathematical modelling; Theoretical physics.

Premise

We could define *theoretical physics* the speculative part of physics which, starting from basic assumptions and hypotheses, develops them using *primarily* the mathematical language and formalism, in order to obtain physical laws in the form of mathematical equations and conditions.

The power of the mathematics that is used demonstrates all its value in relation to how much *universal* are the introduced (new) laws; the corresponding theories will be the more important and universal the more the domain of validity of the involved mathematics is broad.

Theoretical physics includes both the inductive and the deductive aspects; in particular the latter allows the construction of models on a purely mathematical basis,

with general physical assumptions at the base, which allow an interesting and fruitful quantitatively accurate prediction. The mathematical aspect of a theory that wants to be defined as a theory of "theoretical physics" is therefore doubtless essential.

What I have said must be connected to the "Subjects" indicated by the author for the paper, namely "Physics" and in particular "Theoretical physics".

Analysis of the content

In the introductory paragraph, presented without a name, it is stated that "The vacuum is timeless in the sense that time is not its fourth dimension", not explaining further and not providing mathematical indications on the meaning (the essence) of what it is indicated.

The author says that "On a stellar object's surface, the density of the vacuum is at the minimum (ρ_{min})", but he does not explain why, and indeed in the continuation of the article it is clear that he applies inadequately the "Newton shell theorem".

In Figures 1, 2 (and so in 3, 5, 6) the letter "P" is indicated in the place of " ρ ". Figure 2 is inadequate and proves his misinterpretation of the Newton shell theorem. The theorem states indeed that "a spherically symmetrical body affects external objects at the gravitational level as if all its mass were concentrated in a point at its center". The author should therefore explain why he indicates the minimum density on the surface of the object rather than in its center.

Eq. (4) seems to be not supported by the Newton shell theorem, and then the considerations of the following are doubtful; due conditions of validity should be connected to Eq. (3), always in relation to the Newton shell theorem that the author claims to use.

Further on, he writes that "These vacuum fluctuations are characteristic from the macro scale of the stellar objects to the micro scale of the proton"; it is a generic sentence, not supported by the correct orders of magnitude, since at the atomic and subatomic level, with dimensions of order of 10^{-10} m or less, the laws of classical mechanics, which describe the motion of macroscopic system, have problems.

The author aims to present a model ("In the model presented here ...") and this raises the question about what model, considering that he cites only parts of articles by other authors. Below he only indicates in words that "the Casimir effect and van der Waals

force can be described by vacuum fluctuations", but he does not add any mathematical support, considering that this should be a (new?) model.

The author talk about "Gravity force from the macro- to the microscale (proton)"; he does not explain why the model would arrive at the "microscale" indicating the proton, nor does he explain what he means with "microscale", considering that the dimensions of the proton are much smaller than the microscale.

He writes that "The gravity force between physical objects is immediate". On a relativistic level, the speed of gravity refers to the speed of a gravitational wave; in this sense, general relativity provides that the speed is the same as the speed of light c. This appears to be confirmed by the observation of the GW170817 neutron star merger.

The author does not explain why he uses Eq. (7) within a stellar object.

Eq. (8) is the rewriting of Eq. (1), and he adds: "where E is the energy of the vacuum that is incorporated in a given physical object, m is the mass of the object, $\rho_{\max}$ is the density of space in the intergalactic area, $\rho_{\min}$ is the density of space on the surface of the physical object and V is the volume of a given physical object". He seems to confuse/mix the energy of the vacuum incorporated in a physical object with the energy deriving by its mass, not distinguishing between total energy (of the vacuum and relative to the mass of the object) and energy relative only to the mass of the object.

In Eq. (9), the Lorentz factor γ disappears in the last step and it appears the term $\rho_{\min R}$, defined as "density of the vacuum at the relativistic proton surface". He adds:

"The proton, when accelerated, is interacting with the vacuum and additionally incorporating some of its energy", but only in words, he does not offer adequate mathematical support.

The author uses then Eq. (4) which would derive, as previously underlined, from a misinterpretation of the Newton shell theorem. The considerations that follow are repeated in identical sentences, instead of being correctly and better visible in a table, as the visualization of scientific data requires. In the further part of the paragraph, unclear qualitative considerations are written, not supported by mathematical information.

The author generically writes that "In General Relativity, the gravitational time dilation is calculated using Eq. (10)", not indicating that such relation is derived by

the Schwarzschild metric, which is not the only usable metric, despite being the most general spherically symmetric vacuum solution of the Einstein field equations.

He does not explain the derivation of Eq. (11), and extensively adds elementary calculations, without using the scientific notation and without summarizing them in helpful tables. Most of the calculated data are not linked to known scientific existing data for a possible constructive comparison.

He uses Eq. (13) concerning, as by him indicated, "a simple case of a real scalar field H". It follows a series of qualitative considerations, already mentioned by the authors indicated in the references, but it does not follow a mathematical approach that introduces and clarifies his improvements.

Concluding remarks

The article aims to present a model concerning the variable density of the vacuum in relation to gravity. This is not a new concept, variously addressed and connected to the superfluid quantum vacuum model. Some sentences are indicated, but they are not supported by an adequate mathematical analysis, as it would like to be in the author's intention an "introduced model".

The paper is not clearly written, it does not technically sound; claims are not fully supported by experimental data and not appropriately discussed in the context of previous literature.

There is no basic mathematical structure and the few introduced formulas are not adequately supported by strong mathematical/logical reasons. The author does not touch any fundamental questions, such as the possible connection between the curvature of general relativity and the variable density of the vacuum. There is no connection, even minimal, to tensor analysis, which is the basis of the theory of relativity.

The author seems to not fully grasp the meaning of the Newton shell theorem and seems to be not familiar with general relativity, as well as with the substantial differences between special and general relativity.

The used English does not help in the comprehension of paper and there is a deficit of aesthetic nature, such as the lack of tables in the place of data added in the text, as well as errors and imperfections in the figures.

The article seems therefore to require a complete conceptual and aesthetic review, and must be completely (re)-built in mathematical terms. The article, as it is now, should therefore be removed.

In a situation with me as reviewer, the article as presented would have been rejected. Why it has been accepted and published, is instead a question to ask to the reviewers, and maybe not just to them.