Editor's Comment:

The author tries to use the gauge transformation, which he has applied to the Lagrangian formalism, also to the Hamiltionian one. The Lagrangian (1) is written with wave functions ψ^* and ψ. The Hamiltonian density (6) is written without wave functions ψ^* and ψ. The wave function in (6) is absolute different that from that in (1). So, the basic operators – (1) and (2) - are very different. It is obvious from the beginning that the application of his approach to the Hamiltonian formalism can be successful or unsuccessful. The researcher has to try. The author tried and did not receive a positive result.

It is known from the classical field theory that the Lagrange formalism is the most fundamental, and the Hamilton's is not so basic.

The correct conversion of one formalism to the other one is possible only if

$\partial^2 L/\partial \dot{q}_i \partial \dot{q}_l$ is not degenerated, i.e. it is not equal to zero (here $\dot{q}_i$ is the velocity's component).

I do not see any paradox. The author did not take into account that each of the methods – Lagrangian and Hamiltonian – is functioning in its own framework of rules. It will be more paradoxical, if suddenly the author has reached a success applying the Lagrangian transformation to Hamiltonian's method.

Authors Feedback:

I answer below arguments of the reviewer in the order of their appearance. My answers show that the Reviewer's complaints are unjustified and that the paper is correct. In particular, it is proved in item 5 that the paradox is also found in the Lagrangian formalism. This issue shows that a discussion about the importance of the Hamiltonian is irrelevant to the paper. The manuscript is reviewed as mentioned in item 5.

On top of everything, my paper presents a new quantum paradox. Nobody has shown that this paradox has been published earlier. The paper aims to draw the readers' attention to this problem and stimulate them to try finding a good solution. This is the very essence of publishing papers in a scientific journal.

I kindly ask the Editors to publish the paper.

Response to specific points of the Reviewer.

1. Considering the paper, unlike the Reviewer's statement, the Dirac Hamiltonian density is not (6) but (4). Due to fundamental requirements, (4) is written in terms of $\psi^\dagger$, ψ. As usual, the Dirac Hamiltonian density is derived from its Lagrangian density by means of the Legendre transformation (see (3)). Hence, the function ψ of the Hamiltonian density (4) is the same as that of the Lagrangian density (1).

2. The Dirac Hamiltonian is a vital element of the Dirac theory because it stands on the right hand side of the particle's equation of motion. Furthermore, the equation of motion is a fundamental element of any quantum theory. The paper's analysis abides by this matter.

3. There is no doubt that the Hamiltonian is an important quantum operator. For example, its eigenfunctions describe quantum states. It also plays a crucial role in the Fermi Golden Rule which describes a transition between quantum states, which is a primary quantum effect. Therefore, an analysis of the Hamiltonian properties is widely used in articles that discuss quantum properties. My paper deserves the right to do that.

4. The Reviewer says: "The correct conversion of one formalism to the other one is possible only if …". As a matter of fact, the transformation from the Dirac Lagrangian density to the corresponding Hamiltonian density can be found in textbooks (see e.g. J. D. Bjorken and S.D. Drell, Relativistic Quantum Fields (McGraw-Hill, New York, 1965), eqs. (13.42), (13.44) on pp. 54, 55)). No restriction whatsoever is mentioned on the validity of the Dirac Hamiltonian density. Evidently, textbooks make an undeniable basis for a discussion in manuscripts.

5. By definition, a gauge transformation of a Dirac system affects the 4-potential *and* the function ψ (see (2)). The 4-potential is a part of the dynamics of the system and is a term of the Lagrangian density and of the Hamiltonian. The function ψ is a primary element of any quantum theory. The paper proves *two* kinds of discrepancies: The transformed Hamiltonian of a free electron takes the form of a bound electron of the hydrogen atom Hamiltonian (see (12) and the text that follows it). On the other hand it proves that an application of the operator $i\partial/\partial t$ demonstrates that the gauge transformation of the function ψ casts it into a form that is not an energy eigenfunction (see (14) and the text that follows it). The function ψ is an element of the Lagrangian formalism and of the Hamiltonian formalism as well. Therefore, the paper's analysis applies also to the Lagrangian formalism. This issue answers the primary complaint of the reviewer. A short paragraph that contains this matter is added at the end of section 2, on p. 8 of the revised version. Few sentences are added to the last section.

6. The Reviewer says: "I do not see any paradox". I really wonder to read these words. Indeed, the paper proves that the transformed Hamiltonian of a free motionless Dirac particle is that of a bound electron of the hydrogen atom (see (12)). Moreover, the transformed wave function does not represent a state that has a well defined energy (see (14) and the text that follows it).

7. Two Reviewers have already stated explicitly that the paper contains no technical error. I also do not see any statement that mentions an explicit error in the last Reviewer Report (namely, an eq. number of an error, together with a proof that it is erroneous).