Editor's Comment:

I think the author have to fulfill the responses of the reviewers

Author's Reply:

The attached file contains my response to this reviewer. It proves that his claims are just wrong.

FINAL EVALUATOR'S comments on revised paper (if any)	Authors' response to final evaluator's comments
I agree with the author that, in the notation of Ref. [14] one can take the Dirac spinor w^1(0) = (1,0,0,0) as a state with a definite spin (as given in Eq.(3.2) of [14]). Than the action of the operator (1+-\gamma_5) as shown in Eq.(2) on page 6 of the manuscript produces a mixed state of w^1(0) and w^3(0) (note that unitarity here can be restored by an adjustment of the operator normalization, i.e. factor 1/2). So one gets a quantum mixture of two states with different spins. Such quantum mixed states are well known. The mixed state consists of solutions of the Dirac equation(s) which are at rest and have the initial mass m. So this mixed state doesn't have infinite energy-momentum, since the operator (1+-\gamma_5) acts only in the spinor space and doesn't affect 4-momenta in the Minkowski space. So the critics of the author of the standard (1+-\gamma_5)/2 projection operators is completely wrong. Note that the standard treatment of electroweak interactions is both justified theoretically and verified experimentally (up to certain but very good precision). But the main problem of the present paper is not the faults in the critics of the Standard Model. The problem is that the suggested alternative is not elaborated. Observable consequences of the new model have not been confronted to experimental data on week processes, e.g. for decays of Z and W bosons. Meanwhile the standard approach describes these decays in the perfect agreement with experimental data. Moreover as I noted in the first report, the suggested model is obviously non-renormalizable and violates unitarity.	1. Let us examine 2 cases: A. An application of (1+-\gamma_5) to a single spinor state yields a single spinor state. Here components of the result are (1,0,λ,0). This spinor represents a particle that moves in the z-direction (see here: https://en.wikipedia.org/wiki/Dirac_spinor#For_particles). Hence, the operation is unacceptable because it violates momentum conservation. B. An application of (1+-\gamma_5) to a single spinor state yields two spinor states. Evidently, the second state is the anti-particle of the original Dirac particle (see the above link). This is totally unacceptable, due to conservation of charge and of lepton number. . 2. The fact that the operation of (1+-\gamma_5) does not alter the spatial component of the spinor is another error of case A and does not rectify case B. 3. Conclusion: in all cases, the paper's claim is right and this reviewer is wrong. 4. It is very well known that a paper that is published in a Journal differs from a comprehensive textbook, because the former presents some scientific information but it is quite short and it does not discuss all aspects that may be relevant to the subject which is discussed in the paper. Therefore, since the paper is not too short, the reviewer complaint that "the suggested alternative is not elaborated" does not provide any reason for a delay of the paper's publication. 5. As stated in my first rebuttal, there are no

	renormalization or unitarity problems with my paper. 6. Here I wish to point out that four reviewers have found the paper suitable for publication. 7. Last but not least. I see that this reviewer objects the paper. Therefore, I propose that he write a paper of his own where he explains his point of view. In this case, I kindly ask the right to respond to his paper. These papers will certainly help readers access a better opinion on the subject.