<u>An Emerging Debate over How to Construct a Quantized Theory of Tachyon Fields</u>

from Charles Schwartz, schwartz@physics.berkeley.edu, December 18, 2021

The two documents below come from the peer review process of my latest research paper, “Tachyons with any Spin”, which was submitted to IJMPA on June 9, 2021, and finally accepted for publication on December 7, 2021. The final version of my manuscript may be found at arXiv:2105.03017v3 .

The first document below is the report from Reviewer #2 (identity unknown), sent to me by the editors on September 8. The second document is my response, sent to the editors on September 10.

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Review of "Tachyons with any Spin", by Charles Schwartz

September 8, 2021

1 Introduction

In this article, henceforth [3], the author reviews an idea expressed in his previous papers on tachyons, namely the introduction of an indefinite metric in the one-particle vector space of states of a spin 1/2 tachyon (in the context of building a quantum field theory), extends the idea to higher half-integral spins, and develops a wave equation for such (integer plus one-half) spin values. Also in the paper, the author suggests a connection between a projection of the symmetric bilinear algebra of the $j = 5/2$ angular momentum matrices and the Gell-Mann matrices of the SU(3) representation.

2 Comments

Although I have found the paper to contain some interesting ideas or conjectures, the article seems to be sketchy or unclear in some details which, if clarified, would greatly help the reader understand the subject matter in general and what the author is doing in particular. I shall start with a general introduction to what I think are the two most important points about the subject of half-integral spin tachyons, and then continue with suggestions of where and how I think the author may improve and clarify his paper, referring back to the two points as necessary.

(1) The first important point about developing tachyonic quantum field theory, in my view, is that in the free quantum field theory, spontaneous Lorentz symmetry breaking must occur. This is necessary to maintain a weak form of causality (namely that messages can't be sent back into one's past light cone via, say a tachyon relay system), as well as to guarantee renormalization if interactions are introduced. Note, however, that the classical wave equation from which the second quantization process starts may well respect Lorentz covariance. Lorentz symmetry breaking is implemented in the second quantization when the one-sheeted mass hyperboloid, representing the set of the allowed energy-momenta of all particles and antiparticles in the model, is cut in half by a space-like hyperplane through the origin, and only the upper half kept. This is analogous (in the regular massive case) to the upper sheet of the two-sheeted mass hyperboloid being taken to represent the spectrum of all allowed energy-momenta of all particles

and anti-particles. The negative energy particles (moving back in time) have been reinterpreted as positive energy anti-particles (moving forward in time) with the opposite values of certain quantum numbers such as charge. There is a preferred frame in which the cutoff plane of this one-particle spectrum is simply the $E = 0$ hypersurface. In all other frames, this spectrum would stay within the full one-sheeted hyperboloid, but its cutoff hyperplane would shift to allow negative energies states on one side, and forbid the states with opposite energies from existing on the other side. Furthermore, although Lorentz symmetry is manifestly broken, a symmetry subgroup conjugate to SO(3) is preserved in such models.

(2) The second important point concerns tachyons with spin 1/2 and possibly other half-integral spin values. To my knowledge, this was first brought up in the paper by Chodos et al. [1], namely that the one-particle vector space for a spin 1/2 model has an indefinite inner product. (The author is correct that this should technically not be called a Hilbert space at the moment, because the metric is not initially positive definite.) Although it might first appear to pose an insurmountable problem, this indefinite inner product turns out to be a redeeming feature of such models since it points directly to the parity breaking of the neutrino. The idea is to simply project out the subspace of states with negative norms, leaving one with only left-handed particles and right-handed anti-particles. On the remaining quotient space, the inner product becomes positive definite, and the vector space, assumed complete with respect to the induced norm from this inner product, may then legitimately be called a one-particle Hilbert space. A summary of such a spin 1/2 model of this kind is given in [2].

Concerning the title, since the author clearly deals with $2j$ odd and gives formulae for H_1 and H_2 only for this case (p.3, l. -9), the title really should say something like "Tachyons of any half-integral spin." Furthermore, the title for Section 3 should be "Generalize to any half-integral spin".

From the "Author's response to Reviewer's Comments about IJMPA-D-21-00257", second paragraph: As I stated above in point (2), with the projecting out of states with negative norm (using essentially $\frac{H+1}{2}$ as projection operator, since $H^2 = 1$), the space of one-particle states becomes a Hilbert space, negative normed states and zero normed states are eliminated. The overall sign of H may be chosen so that the remaining states represent left-handed particles and right-handed anti-particles. Again, this is done once and for all in the preferred frame, and properties are Lorentz transformed appropriately when the system is observed in other frames.

Third paragraph: there is a well-defined Hamiltonian operator in this formulation, which is bounded below due to the cut-off in the one-particle spectrum. The lower bound of energy in the preferred frame [mentioned in (1)] is zero for all momentum directions.

Section 1, first paragraph: please insert the italicized words in "...and this does not allow *finite dimensional* unitary representations, except for the one-dimensional case."

One issue that doesn't seem to be addressed in Section 2 is that of the possible inconsistency between the choice of allowed states (namely left-handed particles and right-handed anti-particles), and O(2,1) symmetry, or, more generally, Lorentz symmetry. Eq. (2.6) expresses this inconsistency due to the negative signs in the last two equations of that line.

Section 3: Seems ok; the definition of H appears consistent with the later notion that the spin j equation will decompose into $j + \frac{1}{2}$ spin 1/2 tachyons. E.g., the $j = 5/2$ equation decomposes into 3 spin 1/2 tachyons. The subspace corresponding to negative eigenvalues of H is projected away, leaving the left-handed particles and right-handed anti-particles for each tachyon.

Section 4: I'm not clear where this section is heading, but it all looks correct.

Section 5: I didn't follow the first paragraph very well. Is the upper left block for the $j = 3/2$ case going to be a symmetric algebra that is to be compared with the Lie algebra of SU(2)? I checked paragraphs 2 to 5 and they seem to be all correct. As for paragraph 6, is the author perhaps suggesting that there may be a natural Lie product that can be defined on the symmetric bilinear algebra of J matrices (restricted to upper left 3x3 blocks), making this algebra into a genuine Lie algebra, which permits an isomorphism between these two Lie algebras? I suppose there might be, but it may not be very natural. Besides, it seems a bit off the topic here. It could be pursued as a separate math research project, and one could see if there are similar examples for other dimensions. But without a Lie bracket defined for the symmetric bilinear algebra, and a way to ensure that all elements are traceless, it seems difficult to find a useful correspondence between the two objects, but, of course, I could be proven wrong about that.

General comment: It appears that the author takes Lorentz symmetry as a primary starting point and endorses Wigner's approach as in [5], or in [4]. Since O(2,1), the little group for the tachyonic case, $p^2 = -m^2 < 0$ is not compact, one expects the unitary irreducible representations of the Lorentz group to be infinite dimensional. The author seems to claim that the introduction of H effectively changes the group generators J_i back to those of the compact group O(3), allowing a finite-dimensional unitary representation of the Lorentz group. However, it appears that the little group symmetry must be broken in the sense that the projection to the positive-normed subspace of one-particle states doesn't commute with all the generators of the little group. This brings one full circle back to the original idea that Lorentz symmetry is broken down to a subgroup conjugate to O(3) in order to arrive at a sensible QFT. Hence it seems reasonable to either anticipate this from the beginning, or look for invariant subspaces of finite dimension within Wigner's infinite-dimensional tachyonic representations.

Section 6: I wonder, did the author consider the equation $D\Psi = i\mu\Psi$ where D is defined as

$$D \equiv i\partial_t\gamma_0 - i\mathbf{J}\cdot\nabla\gamma_{05}\ ? \tag{1}$$

In this case, one would get

$$\begin{pmatrix} \omega & -kU \\ kU & -\omega \end{pmatrix} \Psi = i\mu\Psi\ , \tag{2}$$

and a different dispersion relation, namely

$$\omega^2 - k^2U^2 = -\mu^2\ , \tag{3}$$

which also may be of interest.

Section 6, paragraph at bottom of p.8: The details should be filled in, but is the end result the same if one broke Lorentz symmetry down to O(3) at the start, and then used the $j = 5/2$ representation in which J_2 and J_3 really do represent the generators of the rotation group about the y and z axes?

Finally, a comment on the connection between the symmetric $j = 5/2$ bilinear algebra and $su(3)$: was the author possibly seeking something like the strong interaction symmetry among the 3 flavours of neutrino?

Concluding comments: It seems of interest to promote consideration of the tachyonic neutrino hypothesis (TNH), especially since recently, KATRIN's initial results show a negative mass squared to one sigma for the electron neutrino coming from tritium beta decay. Hence, generally speaking, although I think there are a number of unclear statements, or points of confusion in this paper, and although I hope the author will reconsider his apparent stance against Lorentz symmetry breaking in tachyonic theories[1], I would nevertheless encourage publication of this paper, once the various issues raised above are satisfactorily dealt with or further clarified.

References

[1] A. Chodos, A.I. Hauser, and V.A. Kostelecký. The Neutrino as a Tachyon. *Phys. Lett. B*, 150:431–435, 1985.

[2] M.J. Radzikowski. A Quantum Field Model for Tachyonic Neutrinos with Lorentz Symmetry Breaking. 2010. In: Proceedings of the Fifth Meeting on CPT and Lorentz Symmetry, Bloomington, IN, 28 June – 2 July 2010. See arXiv:1007.5418.

[3] C. Schwartz. Tachyons with any spin. 2021. IJMPA-D-21-00257R1.

[1]Note that I have tried to help him deal with at least one objection from the previous reviewer, that of the infinite-dimensional unitary spin 1/2 representations of the Lorentz group [4]; this objection is rendered harmless because the full symmetry group has been broken down to O(3).

[4] H. van Dam, Y.J. Ng, and L.C. Biedenharn. *Phys. Lett.*, 158B.

[5] E.P. Wigner. pages 161–184, Berlin-Heidelberg. Springer-Verlag, 1997.

Author's Response to Reviewer #2
re IJMPA-D-21-00257R1, “Tachyons with any Spin”
by Charles Schwartz   September 10, 2021

I have made several minor changes to the paper responding to comments by this Reviewer and those will be itemized later in this response. I must start, however, with a vigorous complaint against the main thrust of this reviewer’s report.

How might one create a quantum Field Theory for Tachyons? The first two pages of this Reviewer’s report contain a recitation of what is “in [the reviewer’s] view” the only correct approach; and the later segments of the report are filled with more of the same dogma. I find this objectionable, if not outright prejudiced.

There are a number of authors who have written about quantizing a tachyon field, following mostly the habits learned from textbooks about how to quantize fields for ordinary - slower than light - particles. That means “canonical quantization”, leading - for tachyons - to a problem with negative frequency wave functions that gets resolved by breaking Lorentz invariance. Various means have been taken to resurrect that mess; and those authors seem delighted when they can achieve “causality” for tachyons mimicking what they know for ordinary particles. The Reviewer aligns with that camp.

I have taken a very different approach. My 2016 paper (cited as reference #1 in the present manuscript) starts out by explaining why “canonical quantization” is the wrong way to treat tachyons. I maintain a strict adherence to Lorentz invariance. My inner products of tachyon wave functions do not use d^3x. A central point of my approach is the definition of tachyons as stuff that always travels faster than light: this means that I turn the conventional calculation of “causality” into its opposite. [My quantized tachyon fields commute/anti-commute for time-like intervals.] In my 2018 paper (cited as ref #2 in the present manuscript) I make a point of distinguishing my approach and my results (particularly with regard to the KATRIN experiment) from that of the other camp.

These two approaches are in competition with each other. So be it. However, I reject the Reviewer’s attempt to coerce my analysis into conforming with their preferred view.

My responses to specific suggestions of this reviewer

Section 1. In the first paragraph I have inserted the words suggested.

Changing the title of the paper and Section 3. I reject this suggestion. The original purpose of this study was to generalize the indefinite metric H, originally found for j=1/2, to all j values, integral and integral + 1/2. This is fully accomplished in Sections 1,2,3. In later Sections there is some preference for 2j odd; and in Section 6, following the Equation (6.1), I put aside the cases of j integral, for a good mathematical reason.

Section 5. The Reviewer seems confused about when and where the Lie algebra for SU(3) comes in. The numbered equations in this Section display the results of some numerical calculations: a complete isomorphism between these 3-dimensional matrices derived from the j=5/2 representation of O(3) and the Gell-Mann matrices. Those latter (the eight lambda matrices defined by Gell-Mann) are exactly the basic elements of the Lie algebra of SU(3).

Section 6. The Reviewer suggests some alternative to my modified Dirac equation. I have added an Appendix to the paper that acknowledges the possibility of many such alternatives and ties this in with a “fitting” of the mass formulas to experimental data about neutrinos.

I have also made two further additions to the manuscript, conceived before I received this Reviewer's report.

(1) An added paragraph near the end of Section 6 that goes into some (pedagogical) detail about the Poincare group. (I am surprised that the Reviewer's long report makes no mention of the two most exciting things in my paper: the multiple masses and the irreps of the Poincare group.)

(2) An added paragraph at the end of Section 7 that discusses future implications of the results achieved here. (This may connect with the Reviewer's second remarks re Section 6.)