Referee Report

**Nonlocal Rarita–Schwinger theory**

## Summary

The manuscript constructs free nonlocal extensions of the Rarita–Schwinger theory for spin-3/2 fields. The authors consider two related classes of form factors: scalar functions of the d'Alembertian, $f(\Box)$, and operator-valued functions of the Dirac operator, $f(\partial\!\!\!/)$. For the massless theory they introduce nonlocal kinetic terms and corresponding gamma-trace gauge-fixing terms, and they argue that the propagator in the physical spin-3/2 sector is obtained from the local one by inserting the form factor in the Dirac denominator. For the massive theory they argue that the usual Rarita–Schwinger subsidiary constraints, $\gamma \cdot \psi = 0$ and $\partial \cdot \psi = 0$, survive the nonlocal deformation at the free level, so that the unphysical spin-1/2 components do not become propagating degrees of freedom. They then derive modified pole equations and illustrate the $f(\partial\!\!\!/)$ case with exponential form factors, including a Lambert-$W$ description for one branch and an oscillatory example with complex dispersion relations.

The paper addresses a reasonable and potentially useful question. Free Rarita–Schwinger theory is constrained enough that it is not automatic that a nonlocal kinetic deformation preserves the spin-3/2 sector. The manuscript also has the merit of separating the scalar-form-factor and Dirac-operator form-factor cases, since these have different algebraic behavior. The appendices provide useful supporting material on the gamma-matrix identities, the constraint analysis, and the spin-projector basis. The scope is modest: the paper does not attempt to solve the known interaction and causality problems of spin-3/2 fields, and it correctly states that interactions are a separate and delicate problem.

In my view, however, the present version is not yet ready for publication. The main idea is plausible and likely publishable after revision, but several central claims are currently stronger than what is actually demonstrated. In particular, the ghost-free and no-new-pole statements require substantial qualification, the derivation for form factors $f(\partial\!\!\!/)$ needs more care because such form factors do not commute with individual gamma matrices, and the propagator formulae should be clearly distinguished between full gauge-fixed inverses and projected physical-sector propagators. I therefore recommend major revision.

## Assessment of Correctness and Logical Structure

The logical structure of the paper is clear. Section 2 reviews the local massive and massless Rarita–Schwinger theory, introduces the gamma-matrix notation, states the usual constraints, and quotes the standard propagator. Section 3 then introduces nonlocality first through scalar examples, next through a spin-1/2 Dirac-operator prescription, and finally applies these ideas to massless and massive Rarita–Schwinger fields. The appendices support the local gamma identities, the constraint derivations, and the projector language.

The constraint argument is the strongest part of the manuscript. For constant coefficient form factors, the divergence of the equations of motion still annihilates the antisymmetric derivative term, and the gamma-trace equation can be combined with the divergence relation to recover the usual massive constraints. Appendix A gives a useful derivation for $f(\partial\!\!\!/)$, including the even/odd decomposition needed because $\gamma^\rho$ does not commute with $f(\partial\!\!\!/)$. This is an important point, and it should be emphasized in the main text: the preservation of the constraints depends on the free, translation-invariant setting and on the analytic constant-coefficient nature of the form factor. It does not by itself establish anything about interacting or background-dependent theories.

The more serious issues arise in the propagator and pole analysis. The paper often states that if $f$ is entire and has no zeros, then no additional poles or ghostlike degrees of freedom are introduced. This statement is correct for the massless scalar-form-factor denominator in the limited sense that $\not{p} f(p^2)$ has the same zero at $p^2 = 0$ if $f$ is nonzero. It is not, however, sufficient for the massive equations considered later. For a massive scalar-form-factor model the pole equation is

$$p^2 f^2(p^2) - m^2 = 0,$$

and a nonvanishing entire $f$ does not prevent this equation from having additional real or complex roots. Likewise, for a Dirac-operator form factor the condition is essentially $\lambda f(\lambda) = m$ on eigenvalues $\lambda = \pm\sqrt{p^2}$, and a zero-free entire function can still lead to multiple Lambert-$W$ branches and infinitely many complex solutions. The manuscript partly acknowledges branch issues in the exponential examples, but this acknowledgement is not reconciled with the earlier and later no-new-pole claims. A journal article on nonlocal field theory should state precisely which analytic conditions are being imposed to avoid additional physical poles, complex Lee–Wick-type poles, tachyonic roots, or negative-residue states. Merely requiring $f$ to be entire and nonzero is not enough for the massive models as written.

A second technical concern is the handling of $f(\not{\partial})$ in the wave operator and propagator derivations. The appendix correctly notes that $f(\not{\partial})$ does not commute with a gamma matrix. But in the main text the expanded operator for the massless and massive $f(\not{\partial})$ theories is written as if the form factor can be moved through gamma matrices in several places. Starting from

$$\gamma^{\mu\rho\nu} \partial_\rho f(\not{\partial}) \psi_\nu,$$

the ordering of $\gamma^\nu$ relative to $f(\not{\partial})$ is not innocuous. Because $f(\not{\partial})$ is a matrix-valued differential operator, the expanded operator should be derived with the form factor kept in its original position, or the authors should show explicitly how the even/odd decomposition justifies each rearrangement. This is especially important because the later propagator for the $f(\not{\partial})$ case is obtained by the formal replacement $\not{p} \mapsto \not{p} f(\not{p})$ inside the physical spin-3/2 sector. That replacement may be valid after imposing $p \cdot \psi = \gamma \cdot \psi = 0$, but the manuscript should demonstrate this directly and should not rely on operator identities that would be invalid off the physical subspace.

Relatedly, the propagators need a sharper interpretation. In the massless case, the expression proportional to the spin-3/2 projector is best understood as the propagator between admissible transverse, gamma-traceless sources or as the inverse restricted to the physical projected sector. It is not obviously the full inverse of the gauge-fixed kinetic operator. A gamma-trace gauge condition alone may leave residual structure in the spin-1/2 and longitudinal sectors, and the projector itself contains $1/p^2$ factors. The authors should either provide the complete gauge-fixed inverse in the projector basis, including the spin-1/2 blocks and gauge parameter dependence, or state explicitly that only the physical projected propagator is being displayed.

The same issue appears in the massive scalar-form-factor propagator. The quoted expression has the tensor-spinor structure of the local massive Rarita–Schwinger propagator with the denominator deformed to $p^2 f^2(p^2) - m^2$. This may correctly describe the projected spin-3/2 sector, where the operator reduces to $\not{p} f(p^2) - m$. It is less clear that it is the full inverse in vector-spinor space, since the off-shell spin-1/2 blocks and the constraint-enforcing terms need not retain exactly the same coefficients once the kinetic term is multiplied by $f(p^2)$ while the mass term is not. The notation $S^{(3/2)}_{\mu\nu}$ suggests a projected object, but the surrounding prose calls it the propagator of the model. This should be clarified and, if the full inverse is intended, derived explicitly.

## Novelty and Significance

The paper is potentially suitable for a high-energy theory journal if revised. The novelty is moderate rather than dramatic: the construction is a free-field extension obtained by inserting analytic form factors into a known constrained system. Still, even a free-field analysis can be worthwhile for spin-3/2 fields, because the subsidiary constraints are central to the consistency of the theory. The distinction between $f(\Box)$ and $f(\partial\!\!\!/)$ is also useful, and the examples in the massive $f(\partial\!\!\!/)$ case give the reader a concrete sense of how the dispersion relations are modified.

The significance would be strengthened by a better connection to the existing literature on ghost-free infinite-derivative theories and nonlocal kinetic operators. The introduction cites early nonlocal field theory and recent spin-1/2 work, but it does not sufficiently explain how the present kinetic operators compare with the common ghost-free construction in which an entire nonzero function multiplies the whole local kinetic operator, leaving the pole set unchanged. In the present model the form factor multiplies only the derivative part while the mass term is left local, which generically deforms the mass shell and can create additional roots. That distinction is central to the interpretation of the paper and should be discussed explicitly.

## Recommendation

I recommend major revision. The topic is appropriate, the paper is readable, and the free constraint analysis appears to contain a useful result. However, the current form overstates the ghost-free conclusion and leaves important operator-ordering and propagator-inversion questions unresolved. I would be inclined to recommend publication after these points are corrected, because the paper could then serve as a clean starting point for later work on nonlocal spin-3/2 effective theories.

## Major Comments

1. The authors should revise the ghost-free and no-new-pole claims throughout the paper. For massive theories, the absence of zeros of $f$ does not imply that $p^2 f^2(p^2) - m^2$ or the corresponding Dirac-operator determinant has only the local pole. The exponential examples already show that branch structure matters. The paper should give a precise criterion for acceptable pole structure, including whether complex poles are allowed as effective-theory artifacts or excluded as ghosts/instabilities. If no general criterion is proved, the text should state that the paper derives the pole equations but does not establish ghost freedom for arbitrary entire nonvanishing form factors.

2. The $f(\partial\!\!\!/)$ derivation requires a more explicit treatment of operator ordering. Appendix A handles the noncommutativity of $\gamma^\rho$ and $f(\partial\!\!\!/)$ carefully, but the main-text expressions for the wave operator appear to move $f(\partial\!\!\!/)$ through gamma matrices without proof. The authors should rederive the expanded kinetic operator with the correct ordering, probably using the even/odd decomposition $f(\partial\!\!\!/) = f_{\mathrm{e}}(\Box) + f_{\mathrm{o}}(\Box)\ \partial\!\!\!/$, and then show exactly how the physical projected equation follows. This is not a cosmetic issue: the validity of the propagator and determinant condition depends on it.

3. The status of the displayed propagators should be clarified. Are they full inverses of the gauge-fixed quadratic operators, or are they projected propagators between transverse, gamma-traceless external currents? If they are only physical-sector propagators, the paper should say so consistently and avoid claiming a full inverse. If they are intended as full propagators,

the authors should present the inversion in the spin-projector basis, including the spin-1/2 blocks and the dependence on the gauge-fixing parameter in the massless case.

4. The massive scalar-form-factor propagator should be checked carefully. The local tensor-spinor numerator contains terms proportional to $1/m$ and $1/m^2$ that enforce the correct off-shell vector-spinor inverse in the local theory. Once $\not{p}$ is replaced by $\not{p}f(p^2)$ only in the derivative part, it is not evident that the full local numerator remains unchanged. The authors should either prove the quoted expression by direct multiplication with the nonlocal kinetic operator or restrict the statement to the projected spin-3/2 sector.

5. The dimensions and sign conventions of the form factors should be made consistent. In the scalar example the exponent is written schematically as $z/\Lambda$ or $p^2/\Lambda$, even though $z = \Box$ has mass dimension two if $\Lambda$ is called a scale. Later, $f(\not{\partial})$ uses $\not{\partial}/\Lambda$, where $\Lambda$ has mass dimension one. The paper should distinguish these cases, for example by using $\Lambda^2$ in functions of $\Box$. The Fourier-space sign convention should also be fixed consistently: $f(\Box)$ becomes $f(-p^2)$ or $f(p^2)$ depending on conventions, and the text currently shifts between these notations.

6. The relation to the literature should be expanded. The paper should explain how its kinetic deformations differ from standard infinite-derivative "ghost-free" choices that multiply the full local kinetic operator by an entire function. It should also make clear whether the goal is a fundamental nonlocal free theory, an effective field theory below $\Lambda$, or a preparatory construction for interactions. This will help readers evaluate the physical meaning of the modified mass shells.

## Minor Comments

1. There are inconsistencies in factors of $i$ in the propagator conventions. For example, the massive local propagator is displayed without the same factor of $i$ that appears later in Appendix B. The authors should choose one convention and apply it throughout.

2. The manuscript contains several encoding problems in the affiliation and introduction, such as corrupted accents and dashes. These should be fixed before publication.

3. Some statements in Section 2 are pedagogically useful but should be tightened. For instance, the massless propagator written with the covariant spin-3/2 projector contains $1/p^2$ through the transverse projector, so the authors should be precise about how it is used on the light cone and with admissible sources.

4. The notation $a_1(p)$ and $a_2(p)$ in the dispersion discussion is somewhat misleading, since these are scalar functions of $p^2$ or of $\rho = \sqrt{p^2}$ after a branch has been chosen. The dependence and branch choice should be stated explicitly.

5. The statement that the determinant condition has multiplicity corresponding to the four massive spin states should be separated from the spinor determinant calculation. The determinant shown is a spinor-space determinant; the vector-spinor constraints account for the spin-3/2 polarization multiplicity.

6. The reference list should be checked for bibliographic completeness and accuracy. In particular, the cited 2026 spin-1/2 reference should be verified, and the paper would benefit from additional references on infinite-derivative/nonlocal ghost-free criteria.

7. The paper would read more professionally if the final remarks avoided claiming that a “ghost-free” deformation has been shown in full generality. A more accurate formulation is that the free constraints are preserved and that ghost freedom depends on the detailed pole structure of the chosen form factor.