Referee Report

Induced CFJ Term in nonlocal Lorentz-Violating QED: Natural Regularization from Entire Dirac Operators

Manuscript: arXiv:2607.13858

Recommendation: Major revision. The question is worthwhile and the nonlocal-vertex construction could support a useful specialist paper. At present, however, the claimed CFJ coefficients are not established: the two-photon contact contribution is discarded by an invalid symmetry argument, and the second model is evaluated with a free denominator inconsistent with its stated kinetic operator. The analytic continuation, pole structure, and interpretation of the finite loop term also require substantial correction.

Summary of the manuscript

The manuscript studies the parity-odd photon two-point function in a Lorentz- and CPT-violating extension of QED. The fermion action in equation (2) contains the axial background $b_\mu \bar{\psi}\gamma^\mu\gamma_5\psi$ and dresses the gauge-covariant Dirac operator with an entire function $f(\not{D})$. Two exponential choices are considered. In Case 1, $f_1(\not{D}) = \exp(-\not{D}/\Lambda)$; in Case 2, $f_2(\not{D}) = \exp(-i\not{D}/\Lambda)$. The ratio $\mu = m/\Lambda$ controls the departure from the local theory.

A useful feature of the paper is its attempt to retain the exact dependence on $\not{\partial}/\Lambda$ while expanding only in the photon field. The Fréchet–Duhamel expansion gives a one-photon vertex V_1 in equation (12) and a two-photon contact vertex V_2 in equation (13). Equation (15) accordingly contains both the loop with two V_1 insertions and the loop with one V_2 insertion, in each case with one axial insertion. The contact graph is then declared unable to contribute to the local CFJ structure, and only the first graph is retained. A derivative expansion leads to the radial integral (33) and, after Wick rotation, to the absolutely convergent deformation function (37) for Case 1. For Case 2, equations (44)–(48) instead give an oscillatory integral treated with an Abel factor $e^{-\eta\rho}$.

The intended physical conclusion is that entire-function nonlocality preserves the CFJ tensor structure while selecting a finite loop coefficient: exponentially in Case 1 and by Abel summation in Case 2. This is a potentially interesting application of the Dirac-operator nonlocal model. The calculation, however, currently has two defects at its core and several unresolved analytic and interpretive issues. Because corrections to the contact graph or to the Case-2 propagator can change the main quantitative results, the paper is not yet suitable for publication in PRD or JHEP.

Evaluation of correctness and logical structure

The logical plan of the paper is sensible: define a gauge-covariant nonlocal action, derive all vertices at the required order, compute the parity-odd two-point function, establish the validity of analytic continuation, and compare with the ambiguous local CFJ coefficient. The first two steps are also the most valuable aspect of the work. In a nonlocal gauge theory, the contact vertex is not optional; deriving it with the ordered Fréchet expansion is the right starting point.

The subsequent execution does not yet complete this plan. Bose symmetry cannot eliminate the contact graph, since the CFJ two-point kernel itself has precisely the required Bose symmetry. Moreover, the Case-2 kinetic operator does not have the Case-1 denominator, contrary to the statement preceding equation (41). Thus neither final deformation function is secure: Case 1 omits a potentially required contribution, and Case 2 is not derived from the displayed action.

There is also a distinction that should be maintained throughout the paper. A UV-finite value for a specified loop graph is not the same as a prediction of the renormalized CFJ coupling. Nor does exponential damping on the Euclidean axis by itself establish a legal Wick rotation or a unitary Minkowski theory. These points are central to the physical meaning of the result, not peripheral qualifications.

Novelty and significance

The paper extends the authors' recent Dirac-operator nonlocal fermion model to an axial Lorentz-violating perturbation and a parity-odd gauge two-point function. The exact photon expansion and the contrast between two analytic form factors could be of interest to researchers working on Lorentz violation and nonlocal field theory. The advance is nevertheless fairly narrow unless the authors can formulate general conditions on an entire $f(z)$ that guarantee a well-defined, gauge-consistent CFJ loop coefficient.

The novelty discussion is presently incomplete. Reference [39] (arXiv:2505.07483) already introduces the same Dirac-operator construction, the two representative form factors, and its $U(1)$ coupling. The manuscript should state precisely which parts of equations (12)–(18) are new and which are inherited from that work. More importantly, J. R. Nascimento, A. Yu. Petrov, and C. Marat Reyes, Phys. Rev. D **92**, 045030 (2015), arXiv:1505.04968, studied an induced CFJ term in a Lorentz-breaking theory with higher derivatives in the spinor sector and also emphasized a finite result. This is direct prior art involving two of the present authors. A detailed comparison of kinetic operators, additional poles, contact vertices, the low-energy limit, and the meaning of the word unambiguous is necessary.

Recent work on gauge-invariant entire-function regulators (J. W. Moffat and E. J. Thompson, arXiv:2511.11756) and on unitary nonlocal theories with CPT violation (M. M. Chaichian, M. A. Oksanen, and A. Tureanu, arXiv:2601.04037) is also directly relevant to the claims about poles, Wick rotation, and UV completion. These papers should be included in the discussion rather than merely alluded to, and the difference between a covariant-Laplacian form factor and the present Dirac-operator form factor should be made explicit.

Major comments

1. **The contact contribution is excluded by an invalid Bose-symmetry argument.**

   Equation (15) correctly identifies two classes of contributions at order bA^2: the graph with two V_1 insertions and the graph with one V_2 insertion. After equation (27), the manuscript argues that the Bose-symmetrized V_2 graph is even under $(\mu, k_1) \leftrightarrow (\nu, k_2)$ and therefore cannot project onto the local CFJ operator. This conclusion does not follow. The CFJ polarization tensor has the form

   $$\Pi^{\mu\nu}_{\mathrm{CFJ}}(k) = C\,\epsilon^{\mu\nu\rho\sigma} b_\rho k_\sigma,$$

   and it obeys $\Pi^{\mu\nu}_{\mathrm{CFJ}}(k) = \Pi^{\nu\mu}_{\mathrm{CFJ}}(-k)$. The minus sign from exchanging μ, ν is compensated by the minus sign from $k \to -k$. Thus the desired CFJ kernel is Bose symmetric under the *combined* exchange. Bose symmetry is a requirement on this term, not a reason for its absence.

   The ordered object in equation (16) also need not itself satisfy the equality written in equation (17). Rather, the sum of the two orderings is constructed to be symmetric. The authors must retain the full contact vertex, expand it explicitly to first order in the external momentum, and evaluate its parity-odd contribution. They should then verify transversality for the sum of the ordinary and contact graphs. In particular, the one- and two-photon vertices should be checked against the nonlocal Ward–Takahashi identities, beginning with

$$k_\mu \Gamma^\mu(p+k,p) = \mathbf{e}\,[S^{-1}(p+k) - S^{-1}(p)],$$

and the corresponding identity relating the two-photon contact vertex to differences of one-photon vertices. If the contact term is nonzero, equations (30)–(48), all numerical plots, and the conclusions must be recomputed.

There is an additional algebraic concern in equations (8) and (13). With $D_\mu = \partial_\mu - i\mathbf{e}A_\mu$, the first perturbation of $\exp(-\not{D}/\Lambda)$ contains $Y = i\mathbf{e}\not{A}/\Lambda$. Thus the linear term in the schematic equation (8) needs a factor i, and its quadratic term needs the corresponding negative phase. Multiplying the first Fréchet variation by the $\mathbf{e}\not{A}$ part of $i\not{D}$ gives

$$\frac{i\mathbf{e}^2}{\Lambda}\,\not{A}\int_0^1 ds\, e^{-(1-s)\not{\partial}/\Lambda}\not{A}e^{-s\not{\partial}/\Lambda},$$

whereas the first term of equation (13) lacks the factor i. The signs and phases of the momentum-space expression (18) must then be checked under the same Fourier convention used for equation (22). These vertices should be rederived directly from equation (2) before the contact graph is evaluated.

2. The propagator used for Case 2 is inconsistent with its kinetic operator.

The text preceding equation (41) states that the Case-2 denominator retains the form of equation (32). This is not compatible with equation (40). Using the same Fourier convention that leads to the Case-1 propagator in equation (22), the free Case-2 inverse propagator is

$$S_{0,2}^{-1}(p) = \not{p}\,e^{-\not{p}/\Lambda} - m = \cosh\rho\,\not{p} - (m + p\sinh\rho).$$

Its scalar denominator is therefore

$$\Delta_2 = p^2\cosh^2\rho - (m + p\sinh\rho)^2 = p^2 - m^2 - 2mp\sinh\rho,$$

not equation (32). The combination $m - ip\sinh\rho$ appearing in equation (41) mixes the two continuations and does not follow from equation (40).

This is not a cosmetic notation issue. Every propagator in the two V_1 graph, its derivative with respect to loop momentum, the pole locations, and the Wick-rotated denominator are affected. Equations (41)–(48) must be rederived from the Case-2 action without importing the Case-1 denominator. Until this is done, the sign change, Abel-summed coefficient, and comparison between the two models cannot be assessed.

3. The Wick rotation and the physical pole structure have not been established.

The argument around equations (37) and (38) shows that the final Case-1 integrand decays on the positive Euclidean axis. That fact is useful but insufficient to justify the contour deformation. One must also show that the integrand is controlled on the large arcs in the relevant complex quadrants and that no propagator poles are crossed. Entire functions may grow in some complex directions even when they damp on one ray.

There is a more immediate mismatch between the displayed Minkowski and Euclidean expressions. A direct substitution $\rho_M = i\rho_E$ in equation (33) does not yield equation (37): the mass combinations remain complex and the denominator does not become the positive sum of squares shown there. An operator-level continuation might define the Euclidean expression, but then the authors must derive that continuation, including the gamma-matrix convention and measure, and explain why it equals the original Minkowski determinant with a valid contour prescription.

The statement that an entire form factor introduces no new poles is too strong for the massive kinetic operator used here. The relevant eigenvalue is not $f(z)$ alone but $zf(z)-m$. For exponential choices, its zero equation is solved by multiple Lambert-W branches, so extra complex solutions are generic even though the exponential itself has no zeros. In the present conventions the Case-1 pole equation may be written $\lambda e^{i\lambda/\Lambda} = m$, while Case 2 gives $\lambda e^{-\lambda/\Lambda} = m$. Case 2 has two positive real roots for $0 < \mu < 1/e$, a double root at $\mu = 1/e$, and no positive real root above that value, in addition to complex branches. The authors should locate all relevant poles and residues, identify the physical mass, state the $i\epsilon$ prescription, and show which poles lie between the Minkowski and Euclidean contours.

The Minkowski reality properties also need attention. If $Q = i\not{D}$ is formally Dirac-Hermitian, the Case-1 kinetic operator is $Qe^{iQ/\Lambda} - m$, whose Dirac adjoint is $e^{-iQ/\Lambda}Q - m$, not the same operator. This agrees with the generically complex pole structure. The paper should establish an appropriate Hermitian formulation or state clearly that Case 1 is defined only through a Euclidean prescription. Claims of a UV completion are premature without an acceptable spectrum, residue structure, reality condition, and discussion of unitarity.

4. A finite loop term does not by itself fix the physical CFJ coefficient.

The local CFJ operator is gauge invariant for constant b_μ, up to a boundary term, and is therefore an allowed independent term in the effective action. Even if the nonlocal loop integral is absolutely convergent, the physical coupling has the schematic form

$$(k_{\mathrm{AF}}^{\mathrm{ren}})_\mu = (k_{\mathrm{AF}}^{\mathrm{bare}})_\mu + C_{\mathrm{loop}} b_\mu,$$

and a normalization or matching condition is needed to specify the first term. The nonlocal model may select a definite value for C_{loop} within a fully specified prescription; it does not, by finiteness alone, predict $k_{\mathrm{AF}}^{\mathrm{ren}}$.

Relatedly, $3\mathbf{e}^2/(16\pi^2)$ in equations (36) and (46) is one value obtained in the local literature, not a regulator-independent local result. The manuscript itself cites the literature on this ambiguity, but later language treats this value as a unique benchmark. The authors should specify the momentum routing and normalization condition that select it, distinguish a loop matching coefficient from an observable coupling, and explain how equation (39) and the analogous Case-2 limit differ from ordinary regulator dependence. Otherwise the noncommutation of the local limit and loop integration simply records that a UV-sensitive amplitude retains information about the chosen UV prescription.

5. The Abel analysis in Case 2 is incomplete and partly mischaracterized.

For generic fixed μ, the large-ρ kernel in equation (44) behaves as

$$K(\rho,\mu) = \frac{2\cos\rho\,\sin^2\rho}{\rho} + O(\rho^{-2}) = \frac{\cos\rho - \cos 3\rho}{2\rho} + O(\rho^{-2}).$$

The leading periodic function has zero mean, so the ordinary radial improper integral is expected to converge conditionally by the Dirichlet test, provided no finite-ρ singularity is present. If that is the intended integral, the authors should first evaluate its cutoff limit. Abel damping should then reproduce that limit, rather than define an otherwise arbitrary value. If a different multidimensional limiting procedure is intended, it must be specified and shown to preserve momentum routing and the Ward identities.

The only displayed numerical choice is $\eta = 0.03$ in Figures 4 and 5. This is not evidence for the limit $\eta \to 0^+$ in equations (45) and (48). The revision should show results for a controlled sequence of η, extrapolate to zero with errors, compare with the direct oscillatory cutoff integral, and demonstrate that the reported zero and sign change of $f(\mu)$ are stable. A radial factor $e^{-\eta|p|}$ is also not invariant under a loop-momentum shift. For a conditionally convergent CFJ amplitude, it may therefore conflict with the trace-cyclicity and momentum-routing assumptions invoked below equation (15). A regulator-level Ward-identity check is required. Because Case 2 needs this external summation factor, it should not be described as the same kind of natural regularization as the absolutely convergent Case-1 result.

There is also an overlooked finite-momentum singularity in the displayed formula. The denominator of equation (44) vanishes when

$$\cos\rho = 0, \qquad \mu + \rho\sin\rho = 0,$$

which occurs at

$$\mu = \rho = \frac{(4n+3)\pi}{2}, \qquad n = 0, 1, 2, \dots.$$

Near these points the displayed integrand behaves as an inverse fourth power of the distance from the zero, so Abel damping does not cure the singularity. The admissible parameter domain must be stated, the behavior near these values must be analyzed, and its relation to the propagator poles must be explained. This issue is especially relevant before a negative coefficient is interpreted as harmless oscillatory dynamics.

Finally, the small-μ statement below Figure 4 is not consistent with equation (48). At $\mu = 0$, that equation reduces to

$$f(0) = \frac{2}{3}\int_0^\infty \frac{\cos\rho - \cos 3\rho}{\rho}\, d\rho = \frac{2}{3}\ln 3 \simeq 0.7324.$$

For $\mu \to 0^+$, the region $\rho \sim \mu$ supplies an additional unit boundary-layer contribution, giving $\lim_{\mu\to 0^+} f(\mu) = 1 + (2/3)\ln 3 \simeq 1.7324$. Thus the local regime $f \simeq 1$ is not recovered by the displayed formula, even before the propagator inconsistency in comment 2 is fixed.

6. The Case-1 limiting behavior and numerical claims need to be rechecked.

The asymptotic coefficients quoted in equation (38) do not match equation (37). Including the overall 4/3, the fixed-$\mu > 0$ integrand behaves as $4\rho^3/\mu^4$ at small ρ, while at large ρ it behaves as $(8/3)e^{-3\rho}/\rho$. If the overall 4/3 is intentionally excluded, the corresponding coefficients are 3 and 2, not the pair displayed in equation (38). The convergence conclusion survives, but the stated asymptotics should be corrected.

More significantly, the $\mu \to 0$ limit is nonuniform. In equation (37) the region $\rho \sim \mu$ gives a finite boundary-layer contribution. Direct quadrature gives, for example,

$$f(0.01) \simeq 1.46996, \qquad f(0.02) \simeq 1.48375, \qquad f(0.03) \simeq 1.46379,$$

so the statement after Figure 2 that $f(\mu)$ decreases monotonically with μ is false near the origin. Moreover, $\lim_{\mu\to 0^+} f(\mu) = 1 + f(0) \simeq 1.353574$, whereas inserting $\mu = 0$ directly in equation (37) gives $f(0) \simeq 0.353574$. This nonuniformity is closely connected to the paper's central claim about the local limit and should be analyzed analytically, not hidden in a plot.

The revised paper should provide a table of values, numerical uncertainties, integration ranges, convergence tests, the location of extrema and zeros, and the small- and large-μ asymptotics for both cases. A reproducible numerical supplement would be appropriate.

7. The derivation from equation (29) to the final radial integrals is not sufficiently reproducible.

The manuscript passes from the schematic derivative expansion in equation (29) to the numerator (31) with very little intermediate algebra. This is precisely where momentum-routing surface terms, derivatives of the full nonlocal vertex, and the CFJ ambiguity enter. The authors should provide the exact momentum-space one-photon vertex, differentiate it with respect to the external momentum at $k = 0$, display the three axial orderings, and show the Dirac trace and angular reduction leading to equations (30)–(33). The same derivation must be repeated with the correct Case-2 propagator.

The replacement $p_\mu p_\nu \to (p^2/4)\eta_{\mu\nu}$ and cyclic rearrangements of the functional trace are legitimate only for a prescription that preserves translation invariance and trace cyclicity. This should be proved for the full Case-1 integrand before any shift of loop momentum. It is especially nontrivial in Case 2, where conditional convergence and Abel damping can generate exactly the surface terms at issue in an induced CFJ amplitude.

8. The scope and literature comparison should be sharpened.

The long discussion of BCH versus Fréchet expansions in Section III.A is mathematically standard and presently occupies more space than the physical checks needed to validate the result. It could be shortened or moved to an appendix. The main text should instead emphasize what is new relative to arXiv:2505.07483, compare in detail with the higher-derivative CFJ calculation arXiv:1505.04968, and discuss recent entire-function work that focuses explicitly on gauge invariance, poles, and Wick rotation.

If the authors wish to retain the broad conclusion that entire Dirac-operator nonlocality is a systematic UV framework, they should give sufficient conditions on a general $f(z)$ for Euclidean convergence, legal analytic continuation, absence or control of extra propagator poles, and preservation of the Ward identities. Otherwise the claims should be restricted to the two examples, with Case 2 described as a prescription-dependent oscillatory model. A specialist PRD contribution could result after the central recalculation; JHEP-level significance would likely require such a general classification or a genuine resolution of the CFJ matching ambiguity.

Minor comments

1. Equations (33), (44), and (48) typeset $\cos\rho$ as the product of three italic variables (`cos`) rather than as the trigonometric operator $\cos\rho$. The integration limits $0 \le \rho < \infty$ should also be written explicitly in equations (44), (45), and (48).

2. The two equations for the Case-2 vertices are numbered ambiguously: the labels associated with V_1 and V_2 both resolve to equation (43). They should receive distinct numbers and be referenced separately.

3. The sentence introducing equation (15) ends with the incomplete phrase *the contact term involving* and does not identify V_2. Several nearby sentences also need grammatical correction, including *The question as to whether* in the Introduction, *in the momenta space*, *imples*, and *deformation fuction*.

4. The quantities p_1 and p_2 below equation (19) are called *Euclidean magnitudes* even though the Wick rotation is introduced only after equation (33). The signature, Fourier convention, definition of $p = \sqrt{p^2}$, and branch used for complex p should be stated once and used consistently.

5. Figures 3 and 5 are described as plotting the CFJ coefficient as a function of ρ, but they plot an integrand; the coefficient is the integral. Axes, normalizations, and numerical integration details should be supplied. The phrase *universal UV damping* below Figure 3 should be restricted to the range of μ actually tested.

6. The bibliography should cite the original Jackiw–Kostelecký CFJ calculation and the renormalization and matching analyses directly, not only a later overview. Bibliographic formatting is inconsistent, and directly relevant recent references on nonlocal CPT violation and gauge-invariant entire-function regulators are absent from the rendered reference list.

7. The symbol f denotes both the nonlocal operator $f(\not{D})$ and the numerical deformation function $f(\mu)$. Distinct notation would improve clarity, particularly when the local limit is discussed.

8. The statement that the contact vertex has no analogue in local QED, where the only vertex is called *triple*, should say that minimally coupled spinor QED has only the fermion–fermion–photon vertex; the word *triple* is otherwise ambiguous.

Recommendation

I recommend **major revision**. The central idea is relevant and the ordered derivation of nonlocal gauge vertices is a promising starting point. Publication requires, at minimum, an explicit calculation of the V_2 contact graph, a Ward-identity check, a complete rederivation of Case 2 with its correct propagator, and a justified analytic-continuation and pole prescription. The authors must also distinguish a finite loop matching contribution from the renormalized CFJ coupling and substantiate the Abel and numerical limits. If those revisions leave a stable, gauge-consistent coefficient and the novelty is positioned against the closest literature, the work could become suitable for a specialist high-energy theory journal.