Referee Report

Nonlocal Four-Fermion Theory

Recommendation: Reject in the present form, with a new submission worth considering only after a fundamental reconstruction of the kinetic operator, determinant prescription, and spectrum. Several algebraic steps following the authors' chosen Euclidean prescription are correct, but the displayed pole equations contradict the claimed stable quasiparticle branch, the model used in the calculation has no demonstrated chiral symmetry to break or restore, and the finite-temperature gap equation does not follow from the grand potential that is written.

Synopsis and overall assessment

The manuscript proposes a four-dimensional large-N four-fermion model with a local scalar interaction and a nonlocal kinetic operator $i\not{\partial} f(\not{\partial})$. The two choices studied are $f_I(z) = e^{-z/\Lambda}$ and $f_{II}(z) = e^{-iz/\Lambda}$. A real Hubbard–Stratonovich field is introduced in equations (19)–(21), and a homogeneous expectation value is combined with the bare mass into $M = m + \bar{\sigma}$. Because the Euclidean Dirac determinant may be complex, the authors replace it in equation (27) by a paired determinant. They then derive the real cutoff potential (29), reduce the matrix inverse to the two-dimensional Clifford subalgebra generated by $\mathbf{1}$ and $\not{p}$, and obtain the hyperbolic and oscillatory kernels in equations (80) and (83). Exact one-dimensional cutoff integrals, an IR/UV matching expansion, and the critical couplings (98)–(100) are presented. Section V formally extends the construction to temperature and chemical potential through equations (102)–(114), discusses the continued pole equations (115) and (116), and gives four numerical illustrations at $\Lambda_{\text{UV}} = \Lambda$.

There is a potentially interesting question here. A matrix-valued form factor treats the two eigenspaces of the Dirac operator differently, so it can alter both the condensate equation and the particle spectrum in a way that is not captured by the more usual scalar form factors $F(\Box)$. The manuscript also correctly emphasizes that the matrix inverse must be performed before the Dirac trace. Conditional on accepting equation (27) as the definition of a new real functional, the Hubbard–Stratonovich normalization, the determinant identity (30), the kernels (80) and (83), the local limit (77), and the critical-coupling numbers in equation (100) are mutually consistent. The relation between the derivative of the logarithmic potential and the gap kernel is also correct within that prescription.

The difficulty is that the prescription is not shown to represent the model defined by equation (17). The Fourier and Wick-rotation conventions are not specified well enough to connect equations (10)–(14) to the Euclidean functions used later; the phase of the original determinant is discarded by an operation that is not generally a modulus square; and the finite-temperature section returns to an unpaired determinant. More decisively, the manuscript's own equations (115) and (116) do not support the claimed physical branch in the regime (117). Equation (115) has no real pole for generic $0 < M/\Lambda \ll 1$, while equation (116) produces more than one real positive pole even below the cutoff for the representative ratio $M/\Lambda = 0.2$. Thus the quantities interpreted as a dynamical mass, a critical coupling, and a thermal restoration temperature have not been tied to a consistent stable fermion spectrum.

The paper's novelty and significance are consequently difficult to judge. Relative to the nonlocal spinor theory cited as Ref. [1], the main new element is the scalar Hubbard–Stratonovich saddle and its cutoff gap equation. Much of Sections II and III is review, and Section V sets up a formal residue representation but does not calculate a nonlocal thermal potential, a critical temperature, or a phase diagram. The bibliography discusses general nonlocal field theory and standard local NJL/Gross–Neveu models, but it does not establish the relation to the extensive nonlocal NJL literature. That comparison is essential before the claimed contribution can be assessed for PRD or JHEP.

The presentation is readable at the level of individual formulas, but it is substantially longer and more repetitive than the result warrants. Statements about symmetry, stability, renormalization, and thermodynamics repeatedly go beyond what is derived. Because the central problems affect the definition of the theory rather than isolated technical details, they cannot be addressed by a conventional major revision of the current calculation.

Logical structure and principal assumptions

The manuscript's argument has the following structure.

- Equations (10)–(17) define a nonlocal kinetic operator and a local scalar four-fermion interaction. The analysis assumes that the entire functions in equations (12) and (13) lead to an acceptable real-time theory and do not add unacceptable poles.
- Equations (19)–(26) introduce the auxiliary field and integrate out the fermions. Up to this point the functional integral contains the ordinary, generally complex determinant of the original Dirac operator.
- Equation (27) replaces that determinant by a paired one. Equations (28)–(83) then consistently manipulate the paired expression, impose a four-dimensional hard cutoff, and define the gap integral.
- Equations (84)–(94) assume an overlap region $M \ll \Omega \ll \Lambda$ in which separate IR and UV expansions can be matched. Equations (95)–(100) infer a condensation threshold from the curvature of the paired potential at $M = 0$.
- Equations (102)–(114) assume that the zero-temperature kernel may be evaluated directly at complex Matsubara momenta and that the resulting meromorphic function admits the usual residue contour with no additional contribution at infinity. Equations (115)–(117) assume that a single stable pole continuously connected to the local mass remains in the stated effective-theory window.

The algebra after the third bullet is useful only if the first two physical assumptions and the last spectral assumption are established. At present they are not; in two cases they are contradicted by formulas in the paper.

Major comments

1. **The Minkowski operator, its momentum-space symbol, and its Euclidean continuation must be defined consistently.**

   Equation (10) contains $f(\not{\partial})$, whereas equation (14) simply writes $f(\not{p})$. On a plane wave, however, ∂_μ contributes a factor $\mp i p_\mu$; this factor of i is material for the two exponentials (12) and (13), because it interchanges hyperbolic and oscillatory behavior. A Wick rotation introduces a second continuation that must be fixed before equations (79), (82), (115), and (116) can be interpreted. The manuscript uses several versions of the symbol—$f(\not{p})$, $f(i\not{P}_n)$, and $f(\not{\partial})$—without deriving their relation.

   The same issue controls reality and unitarity. Under integration by parts, one must determine the adjoint of $i\not{\partial} f(\not{\partial})$ for each choice of f. An exponential that is unitary in one signature becomes non-Hermitian or exponentially growing in another. The assertion after equation (14) that an entire, zero-free f preserves the spectrum is not sufficient: poles are zeros of $\not{p} f(\not{p}) - M$, not zeros of f itself. Such transcendental equations generally have many Lambert-W branches even when f has no zero.

   The authors should start from a precise real Minkowski action, give the Fourier symbol including all factors of i, prove its adjoint and discrete symmetry properties, state a causal pole prescription, and derive the Euclidean operator by an explicit contour rotation. Only form factors for which this construction produces an acceptable spectrum should be carried into the gap calculation.

2. **The paired determinant in equation (27) is a change of the theory, not a consequence of equations (20)–(26).**

   Integrating the Grassmann fields gives $\det D_\sigma$ in equation (24) and $-i\,\mathrm{Tr}\ln D_\sigma$ in equation (26). Equation (27) then makes the replacement

   $$\ln\det D_E \longrightarrow \tfrac{1}{2}\ln\det(D_+D_-)$$

   solely to obtain a real potential. This is legitimate only if D_- is the appropriate adjoint or conjugate of D_+ and the discarded phase is independent of M. Neither statement is shown. For example, with $f_{II}(\not{p}) = e^{-i\not{p}/\Lambda}$ and Hermitian Euclidean gamma matrices, $f_{II}^\dagger = e^{+i\not{p}/\Lambda}$, so the stated $D_- = M - i\not{p}f_{II}(\not{p})$ is not $D_+^\dagger$. The pairing can therefore remove an M-dependent phase and alter the saddle equation.

   There is also an internal inconsistency at finite temperature. The grand potential (103) contains the unpaired determinant of $i\not{P}_n f(i\not{P}_n) - M$, but differentiating that expression does not produce the paired kernel on the right-hand side of equation (107) for a general matrix-valued f. At nonzero chemical potential, complex conjugation also maps μ and Matsubara modes nontrivially. Equations (103) and (107) cannot both follow from the construction as written.

   The authors must choose one functional integral and derive both the zero-temperature and thermal potentials from it. If a phase-quenched or doubled theory is intended, it should be stated as such and its physical field content and symmetries identified. If the original determinant is intended, its phase must be retained and the existence of a real saddle established rather than imposed.

3. **Equations (115) and (116) directly invalidate the claimed stability window.**

   The first pole polynomial can be rewritten exactly as

   $$D_I(q) = (q^2 - M^2)^2 + 4M^2q^2\sin^2(q/\Lambda).$$

   It is nonnegative for real q and vanishes only if both $q^2 = M^2$ and $\sin(q/\Lambda) = 0$. For generic $0 < M/\Lambda \ll 1$, including the illustrative value $M/\Lambda = 0.2$ used in Figure 1, there is therefore no real pole at all. In particular, $q = M$ is not a slightly deformed physical solution of equation (115); it gives $D_I(M) = 4M^4\sin^2(M/\Lambda) > 0$.

   The second equation factorizes as

   $$D_{II}(q) = \big(q^2 - M^2e^{2q/\Lambda}\big)\big(q^2 - M^2e^{-2q/\Lambda}\big).$$

   For $M/\Lambda = 0.2$, the positive roots include $q/\Lambda \simeq 0.169$ and $q/\Lambda \simeq 0.259$, both below the cutoff $\Lambda_{\mathrm{UV}} = \Lambda$ used in all figures; another root lies above that cutoff. Thus even the conservative domain (117) contains multiple real branches for model II, while model I has complex poles rather than a real local branch. The statement following equation (116) has the wrong qualitative conclusion.

   This is not a peripheral finite-temperature issue. It determines whether M is a mass of a stable excitation and whether the Euclidean saddle has a unitary real-time interpretation. The authors should solve the complete complex pole problem for both signs of energy, calculate residues, identify ghost or Lee–Wick branches, and specify the contour. The effective potential and all plots should then be restricted to a demonstrably consistent model. Merely placing a momentum cutoff at Λ does not remove the two low-lying roots just exhibited.

4. **The analyzed model has no demonstrated chiral symmetry.**

   The authors display the chirally invariant scalar–pseudoscalar interaction in equation (16) but explicitly choose the scalar interaction alone in equation (17). The latter is not invariant under a continuous axial rotation. In addition, the kinetic operator is chirally invariant only if it anticommutes with γ^5. For analytic f,

   $$\{\gamma^5, \not{\partial} f(\not{\partial})\} = \not{\partial}\big[f(\not{\partial}) - f(-\not{\partial})\big]\gamma^5,$$

   which vanishes when f is even, not for either exponential in equations (12) and (13). Setting the bare mass to zero does not restore this missing symmetry.

   Consequently, the repeated language of spontaneous chiral symmetry breaking, symmetry restoration, second-order transition, and crossover in Sections IV–VI is not justified for the theory actually analyzed. The stationary value of $\bar{\sigma}$ may still describe a scalar condensate in a model with explicit symmetry breaking, but equation (96) is then not a chiral critical coupling in the usual NJL sense.

   The authors should either adopt equation (16) and an even kinetic form factor, introducing both scalar and pseudoscalar auxiliary fields, or explicitly identify some other exact discrete or continuous symmetry and show how it acts on the nonlocal kinetic term. Otherwise all claims about spontaneous symmetry breaking and restoration must be replaced by the more limited language of solutions and stability of a scalar saddle.

5. **The relation among the bare mass, the auxiliary-field expectation value, and the physical order parameter is mishandled.**

   The paragraph following equation (19) proposes starting at $m = 0$, shifting $\sigma \to \sigma + m$, and discarding the resulting term linear in σ as irrelevant. It is not irrelevant. The auxiliary term becomes $-N(\sigma + m)^2/(2G)$ and hence contains the tadpole $-Nm\sigma/G$; this term changes the saddle equation and is precisely what distinguishes explicit from dynamical mass generation. It cannot yield only trivial contributions.

   More generally, equations (29)–(46) construct an even quantum contribution in M because of the pairing in equation (27), while the classical term is centered at the bare mass m. The paper should keep distinct notation for m, $\bar{\sigma}$, and any pole mass, derive the condensate relation by differentiating with respect to a source, and determine global rather than merely stationary minima. Figure 2 gives one nonzero solution of a gap equation but does not show that it is the thermodynamically preferred branch. This is especially important if the determinant phase or extra poles make the effective action complex.

6. **The finite-temperature analysis is only a formal placeholder and does not establish the thermal claims.**

   The residue identity (109) requires hypotheses on the decay of $n_F(z)F(z)$ on the closing contours and on the treatment of infinitely many poles. Exponential form factors have strong direction-dependent growth in the complex plane, so the usual large arcs need not vanish. Equation (114) then adds unspecified "complex-pole/cut contributions" even though those contributions are the essential new content of the model. No pole set, residue, convergence prescription, or numerical Matsubara sum is supplied.

   Regularization is also incomplete. The zero-temperature calculation uses a four-dimensional Euclidean hard cutoff, while equations (103) and (107) display an unrestricted three-momentum integral and Matsubara sum. It is not explained whether the cutoff acts on $|\mathbf{p}|$, on P_n^2, or only after subtracting the vacuum term, nor whether the result respects the same definition of the theory as equations (29) and (31). The local check (111) is necessary but does not validate the nonlocal contour deformation.

   To support Section V and the abstract, the authors should evaluate at least one complete nonlocal thermal potential with a stated regulator, include all admissible poles and residues, verify it against a direct Matsubara sum, and determine an actual $M(T, \mu)$ branch and its global stability. If this is deferred, the claims about thermal restoration, critical temperature, and transition order should be removed; only a prospective formal setup remains.

7. **The approximation, numerical evidence, and literature comparison are not sufficient for the quantitative claims.**

   The exact cutoff kernels (80) and (83) are a useful intermediate result, but the matched formulas (92) and (93) require $M \ll \Omega \ll \Lambda$. No value of Ω, residual scale dependence, or comparison between the truncated formula and exact quadrature is shown. The error symbols are not accompanied by uniform bounds, particularly near $M \to 0$ or when the hyperbolic UV factor amplifies the endpoint region. The four figures provide no data tables, code, solver tolerances, branch selection, or uncertainty estimates. Figure 2 omits model II, and Figures 2–4 do not state clearly which curves use exact integrals and which use the IR/UV approximation.

   The final remarks also call the equations "renormalized," although the manuscript performs no renormalization: the central results depend explicitly and strongly on the hard cutoff, as equations (98)–(100) and Figure 3 demonstrate. The authors themselves note after equation (101) that the hyperbolic model grows rapidly and would require further counterterms if the cutoff is raised. The correct description is a cutoff-defined effective model unless a counterterm analysis and regulator comparison are supplied.

   A revised paper should provide reproducible code and data, compare exact and matched expressions over the plotted domain, vary both Ω and the UV regulator, and distinguish regulator artifacts from effects of the form factor. It should also explain in detail how the construction differs from established nonlocal NJL models, rather than comparing only with general nonlocal-field-theory references, Ref. [1], and reviews of the local NJL model. Without this context, the novelty of the specific kernels is not yet demonstrated.

Minor comments

1. Equation (14) writes a matrix denominator as if it were a scalar fraction. Its inverse and determinant should be displayed explicitly, and the text should not infer pole counting from zeros of f alone.

2. The text immediately before equation (98) is rendered as "$Mo0$" rather than $M \to 0$.

3. The remainder in equation (56) is not written in a dimensionally transparent form. All error terms in the IR/UV expansion should retain their dependence on M, p, and the denominators needed for a uniform statement.

4. Equations (70), (71), (76), (89), (92), (93), and (112) need final punctuation. Equation (89) also lacks a label while the neighboring UV kernel is labeled, which makes later citation awkward.

5. The phrase "renormalized gap equation" in Section VI should be replaced by "cutoff-regularized gap equation" unless an actual renormalization prescription is added.

6. The Coleman–Weinberg analogy following equations (37)–(40) should be qualified. This is the standard cutoff fermion determinant for a composite NJL order parameter, not a demonstration of dimensional transmutation in a renormalized weakly coupled scalar theory.

7. Figure 4 should specify the horizontal variable and the value of M/Λ in its caption. Every plot should state the integration and root-finding procedure in enough detail to be reproduced.

8. The introductory review could be shortened considerably. Several paragraphs in Sections II–IV repeat the same statements about condensates, critical coupling, and the role of the effective potential, while the defining analytic-continuation and spectral issues receive only a few sentences.
9. The bibliography and prose require copyediting. Examples include “a interesting framework,” “A first step is compare,” the comma splice in the first sentence of the third paragraph of Section VI, inconsistent spacing in arXiv identifiers, and repeated or malformed author information in several entries.

Publication recommendation

The manuscript contains a correct and potentially useful matrix-inversion exercise once the paired Euclidean functional is taken as a definition. That is not yet enough for publication as a physical nonlocal four-fermion theory. The determinant used for the zero-temperature calculation is not derived from the stated model, the thermal calculation uses a different determinant, the claimed chiral symmetry is absent, and the exact pole equations refute the asserted stable local branch. These points undermine the correctness and significance of the principal conclusions rather than only their presentation.

I therefore recommend rejection in the present form. A new submission could be worthwhile if it begins from a real and symmetry-consistent kinetic operator, proves an acceptable pole and residue structure, derives one determinant prescription in both vacuum and thermal settings, and then repeats the mean-field and numerical analysis with controlled cutoff and matching errors. Such a paper should also contain a focused comparison with the nonlocal NJL literature and a genuinely evaluated thermal example if finite-temperature claims are retained.