Referee Report

Birman–Schwinger Formulation of the Faddeev–Popov Zero-Mode Problem

arXiv:2607.14425

Summary and recommendation

The manuscript proposes a normalized fixed-background operator

$$K_A = H_0^{-1/2} V_A H_0^{-1/2}, \qquad H_0 = -\partial^2,$$

on the mean-zero adjoint scalar sector of a torus. Its central exact statement is the form congruence in equation (13),

$$\mathcal{M} = H_0^{1/2}(1 + K_A)H_0^{1/2},$$

from which Proposition 1 and equation (12) identify a Faddeev–Popov zero mode with the spectral threshold $-1 \in \operatorname{spec} K_A$. Equation (14) then relates the inertia and nullity of the two forms. The manuscript supplements this observation with a Sobolev bound and proposed weak-Schatten estimates, a principal-symbol calculation for the crossing count, a representation of the ghost dressing as a diagonal resolvent, and a periodic transverse $SU(2)$ background that reduces to a Mathieu/Jacobi problem. The latter is used to compare an exact threshold with a Born estimate and with finite Feshbach truncations, and to motivate volume and action-counting laws.

There is a useful paper within this material. For a smooth real transverse background at fixed finite volume, the congruence and zero-mode criterion are sound, and the normalized operator is a convenient way to organize the horizon problem. The $Q = m = 1$ Mathieu threshold and the five-site Feshbach formulas are also internally consistent after setting $g = 1$. In particular, the value $a_{\mathrm{crit}} \simeq 1.905316$ following equations (55)–(56), and the rapid improvement supplied by equations (63)–(66), are credible and potentially useful as a solvable reference example.

However, the manuscript is not suitable for publication in its present form. There are significant domain and scope problems in the infrared discussion, a central color-averaging error in the advertised comparison with Gribov's no-pole form factor, an invalid inference from small Faddeev–Popov eigenvalues to eigenvalues of K_A approaching -1, and several novelty claims that do not survive a like-for-like comparison with standard Birman–Schwinger theory. The more substantial weak-Schatten and signed Weyl statements are not proved under the rough hypotheses being advertised. The physical interpretation of the independent-mode "entropy" is also much stronger than the calculation.

Recommendation: major revision. I would reconsider the paper only after the no-pole comparison is corrected quantitatively, the finite-volume and infrared prescriptions are made mathematically consistent, proved results are cleanly separated from conjectural or semiclassical statements, and the novelty and ensemble-level conclusions are substantially narrowed. A revised paper centered on the normalized operator and the periodic Mathieu/Feshbach example could be publishable, but the present headline claims are not yet established to the standard expected for PRD or JHEP.

Assessment of correctness and logical structure

The logical core in Section 2 is persuasive under stronger hypotheses than are currently stated. On a fixed compact torus, with A smooth, real, and transverse, H_0 has a gap after removal of the constant adjoint modes, $H_0^{-1/2}$ is bounded on that subspace, and K_A is a compact self-adjoint pseudodifferential operator of order -1. The kernel map $\eta = H_0^{1/2}\omega$ then proves Proposition 1 at the level of closed forms, and the negative index and nullity agree as asserted in equation (14). Likewise, the Hermiticity argument leading to equation (15), the $d \geq 3$ Sobolev estimate in equations (17)–(18), and the symmetrized Riesz-transform identity (19) are essentially correct, provided the sum over spatial indices is understood in the commutator argument.

The principal-symbol algebra in Section 4 also appears consistent. For $SU(2)$ the color symbol has branches 0 and $\pm|v|/|\xi|$ as in equation (28), and the formal phase-space calculation gives the coefficient $1/(8\pi)$ in equation (34); counting both nonzero branches doubles it in equation (36). For nonzero momentum/color states the resolvent identity (37) and the pole formula (42) are correct. Finally, after making the convention $g = 1$ explicit, equations (53)–(58) and (62)–(66) correctly describe the even Mathieu chain and its first Feshbach corrections. Equations (68)–(70) also give the stated volume and action powers for the tuned periodic family.

These correct components do not resolve the issues below, several of which affect the main quantitative and physical conclusions rather than only the presentation.

Novelty and significance

The specific packaging of the Landau-gauge problem in terms of K_A is useful, but the manuscript presently overstates what is new. Positivity of the Faddeev–Popov operator in the first Gribov region, loss of positivity through a zero mode at its boundary, and Hermiticity on the Landau slice are standard. Equation (13) is an effective preconditioning or form normalization of those facts; it does not replace an approximate horizon criterion by the first exact operator characterization. This is especially important because the Introduction itself cites the all-order equivalence of the no-pole and horizon conditions due to Capri, Dudal, Guimaraes, Palhares, and Sorella.

The most distinctive part is therefore the combination of the normalized operator with the explicit periodic $SU(2)$ Mathieu chain and its Feshbach corrections. To establish the claimed advance, the paper should compare this part directly with earlier one-mode treatments of the Gribov horizon, including L. J. Carson, Nucl. Phys. B **266**, 357 (1986), and should explain what the K_A representation adds to existing spectral-overlap analyses of the ghost propagator. In particular, Cucchieri and Mendes already relate the ghost to low Faddeev–Popov eigenvalues and plane-wave overlaps and explain why small eigenvalues need not imply infrared enhancement. The manuscript's discussion around equations (38), (41), and (42) needs a direct comparison with that work.

There is also a missed opportunity in Section 4. Signed spectral asymptotics for smooth negative-order pseudodifferential operators are part of the Birman–Solomyak framework. The author should determine whether the smooth matrix-valued operator used here is already covered. If it is, the applicable theorem should replace the current heuristic status. If it is not, a precise new theorem explaining the obstruction would materially strengthen the paper. As written, the potentially most substantial analytic claims are left as open tasks, while the exact part is largely elementary.

Major comments

1. **State the operator theorem with complete hypotheses and delimit its finite-volume scope.**

Proposition 1 currently says only that the constant modes have been removed on T^d. It should specify that A is real and transverse, its regularity, the complex adjoint Hilbert space, the operator domain of $\mathcal{M}$ (normally $H^2 \cap \mathcal{H}_\perp$ for smooth coefficients), and the common form domain $H^1 \cap \mathcal{H}_\perp$. The proof should first derive the weak equation for $\omega = H_0^{-1/2}\eta$ and only then invoke elliptic regularity. Self-adjointness does not follow from transversality in isolation; reality, boundary conditions, and regularity are also being used. In equation (14), the meaningful statements are equality of the finite negative index and the nullity, since both positive indices are infinite.

The exact construction is intrinsically finite-volume as presented. On a fixed torus, deleting the constant mode leaves a gap. On $\mathbb{R}^d$, deleting one formal constant state does not remove zero from the continuous spectrum and does not make $H_0^{-1/2}$ bounded. The abstract and conclusion should either restrict the theorem explicitly to compact finite volume or provide a separate infinite-volume form construction. This distinction is essential because Sections 5, 8, and 9 repeatedly use infrared or large-volume language.

2. **Separate smooth pseudodifferential results from rough endpoint claims, and correct the spectral asymptotic statement.**

   Section 2 uses smooth coefficients to call K_A a classical compact operator of order -1. Section 3 then moves to $A \in L^d$ and writes equations (20)–(21) "under the expected endpoint hypotheses," while Sections 4 and 10 acknowledge that the relevant endpoint and signed Weyl theorems remain unproved. These are distinct analytic regimes. The revision should give one precise theorem for smooth A, a separate form-based extension for rough L^d fields where available, and an explicitly labeled conjecture for any unresolved $d = 2$ Lorentz endpoint. Equations (31)–(36) should be supported by an applicable signed Weyl theorem or described only as a formal semiclassical calculation.

   There is also a direct error immediately after equation (23): the negative spectrum of a bounded compact K_A cannot accumulate at $-\infty$; its nonzero eigenvalues accumulate at zero. The relevant regime is $\mu_n \to 0^-$, $\varepsilon \downarrow 0$, or equivalently large amplitude $\lambda \to \infty$. Consequently, equation (35) is an asymptotic count of many crossings far beyond the first Gribov horizon, not a law for the first crossing. This interpretation should be stated explicitly. Figure 1 should also be accompanied by mesh sizes, cutoff dependence, fitting windows, and a continuum convergence test; agreement between two diagonalizations at the same finite cutoff is not such a test.

3. **Repair the infrared and constant-mode inconsistency in the no-pole discussion.**

   The constant ghost state is removed before $H_0^{-1/2}$ and K_A are defined, yet equation (49) writes

$$\sigma(0, A) = \langle 0|K_A^2|0\rangle.$$

   This expression is not defined on $\mathcal{H}_\perp$. On an expanding torus or in the continuum, a fixed-background $k \to 0$ limit can moreover depend on the direction of k for an anisotropic field. The author must choose and justify a specific prescription: a lowest-nonzero-momentum sequence, an angular average, a supremum over directions, or an ensemble-averaged infinite-volume limit. Figure 2's $k = 0$ is evidently the chain index at fixed $m = 1$, not zero total momentum; the notation should make this distinction explicit.

   The same projection must remain visible in Proposition 2. In equation (45), the term $q = k$ has intermediate momentum $k - q = 0$ and is excluded from the Hilbert space. As printed, it produces 0/0. The sum needs a prime or an explicit projected inverse. In addition, the assertion following equation (48) that $\sigma(k, A) \leq \sigma(0, A)$ follows because the loop kernel is largest at zero momentum is not valid for a general frozen anisotropic background. Such monotonicity requires additional rotational or ensemble averaging and must be proved under the adopted prescription.

Finally, the Neumann series (43) converges only when $\|K_A\| < 1$, unless it is being used merely as a formal small-amplitude expansion. Interior membership $\lambda_{\min}(K_A) > -1$ does not control large positive eigenvalues and hence does not imply this norm condition. The convergence domain and the status of the truncation should be stated.

4. **Distinguish the exact no-pole form factor from its quadratic Born coefficient.**

   Proposition 2 and equations (45)–(48) identify the color-averaged second-order Born term with the standard quadratic expression. This does not establish that the exact scalar no-pole form factor is merely a "diagonal-dominance approximation" obtained by discarding off-diagonal spectral weight. If the exact diagonal dressing is parameterized as $d(k, A) = 1/[1 - \sigma_{\text{exact}}(k, A)]$, then σ_{exact} itself contains all orders in the background. Odd terms and irreducible return paths in higher powers of K_A are not represented by equation (49).

   The paper should consistently call equation (45) $\sigma^{(2)}$ or the quadratic Gribov estimate, and should rework Sections 5.2–5.3, the abstract, and the conclusion accordingly. This will also resolve the apparent tension with the all-order equivalence cited in the Introduction. The relation to the explicit third-order expansion of Gomez, Guimaraes, Sobreiro, and Sorella (arXiv:0910.3596) should be discussed rather than implying that the full no-pole condition is exhausted by a second spectral moment.

5. **Correct the color average in the central periodic no-pole comparison.**

   This is a quantitative error, not a matter of convention. Proposition 2 and equation (45) define a normalized adjoint-color trace. Equation (59), by contrast, computes the norm in one charged sector of the periodic $SU(2)$ background. The color-three sector is free and contributes zero, while the two charged sectors give equal contributions. Therefore, in the $g = 1$ convention used in Sections 6–7,

$$\sigma_{\text{ch}} = \frac{a^2}{2(Q^2 + m^2)}, \qquad \sigma_{\text{avg}} = \frac{1}{3}(\sigma_+ + \sigma_- + \sigma_3) = \frac{2}{3}\sigma_{\text{ch}} = \frac{a^2}{3(Q^2 + m^2)}.$$

   The standard color-averaged saturation is consequently

$$a_{\text{np}}^2 = 3(Q^2 + m^2),$$

   not equation (61). At $Q = m = 1$ it gives $a_{\text{np}} = \sqrt{6} \simeq 2.44949$, about 28.6% above $a_{\text{crit}} \simeq 1.905316$, rather than the advertised 5%. The manuscript may retain the value $a_{\text{np}} = 2$ only by renaming it a charged-sector diagnostic and clearly distinguishing it from the color-averaged form factor of Proposition 2. A matrix-valued or largest-color- channel definition would be another consistent choice. Every statement based on the five-percent difference, including the relative size of the two regions, must then be revised.

6. **Remove the claimed special amplitude meromorphy and make the pole statement precise.**

   The argument following equation (39) attributes fixed eigenvectors and simple amplitude poles to the first-order nature of the Faddeev–Popov perturbation and claims that this structure is absent for a multiplicative Schrodinger potential. This is not a like-for-like comparison. At fixed spectral energy, any linear pencil $H(a) = H_0 + aV$ has a normalized operator $aH_0^{-1/2}VH_0^{-1/2}$, fixed eigenvectors, and a meromorphic coupling-constant resolvent. The text instead compares variation of amplitude here with variation of spectral energy in the Schrodinger problem. The asserted distinction should be removed or replaced by a genuinely invariant difference.

Equation (41) should also isolate the full critical spectral projection. Its claim that the second sum remains finite assumes that the lowest eigenvalue is simple; if it is degenerate, additional terms in that sum diverge. Finally, Figure 2 calls $R(k) = |\langle k|\eta_{\min}\rangle|^2$ the residue, whereas the actual residue in the complex a plane is $-a_{\text{crit}}R(k)$ according to equation (42). The former is the coefficient of $1/(1 - a/a_{\text{crit}})$ and should be labeled as such.

7. **Do not infer proximity of K_A to -1 from small numerical eigenvalues of $\mathcal{M}$.**

   Section 5.3 states that the observed drift of low Faddeev–Popov eigenvalues toward zero as the volume grows is the statement that negative eigenvalues of K_A crowd against -1. This contradicts the manuscript's correct warning that equation (13) is a congruence, not a similarity. The free field is an immediate counterexample: on a box of side L, $\lambda_{\min}(-\Delta) \sim L^{-2} \to 0$, while $K_A = 0$ remains a unit distance from the horizon value. Small eigenvalues of $\mathcal{M}$ can reflect the changing kinetic scale without any approach of K_A to -1.

   Any lattice claim therefore requires direct measurement of the generalized or preconditioned spectrum, with a volume-dependent normalization controlled explicitly. The proposed direct computation of K_A could be valuable, but it is a future test rather than a consequence of existing low-mode data. Related statements that refined Gribov–Zwanziger dynamics simply "shifts" a pole of the same frozen-background resolvent should also be removed: that framework modifies the ensemble/effective action and its correlators, which is not shown here to be an eigenvalue shift of a fixed K_A.

8. **Complete the periodic-sector proof and document the numerical evidence.**

   The algebra of the selected even sector is good, but several global statements are not proved. After equation (54), the residue classes $r + Q\mathbb{Z}$ are bilateral Jacobi chains, not automatically half-lines; a half-line arises only after a justified parity reduction in the appropriate sectors. The statement that the exact global first crossing occurs at $|m| = 1$ is supported only by a three-site estimate. That approximation cannot establish the ordering of the exact thresholds. A variational comparison or a controlled scan with error bounds should cover all integer m, residue classes, parity sectors, and charged-color sectors. Numerical values support the claimed ordering, but a proof or a precisely qualified numerical statement is needed.

   Likewise, the inequality $a_{\text{crit}} \le a_{\text{np}}$ drawn from equations (63)–(64) assumes existence of the tail resolvent, convergence of the continued fraction, and positivity of its denominators up to the first crossing. These properties should be established using Schur complements, interlacing, or a positivity argument rather than the phrase "positive subtraction near threshold." The manuscript should state explicitly that $g = 1$ in equations (51)–(66), or restore g throughout; the reported number 1.905316 is a threshold for the dimensionless combination ga in the stated units.

   All numerical statements should be reproducible. The paper should give Fourier cutoffs, parity and Bloch sectors, convergence tables, error estimates, fit windows for Figure 1, and the data or code used for Figures 1–3. The stated fifteen-digit agreement presently checks two implementations at a finite mesh, not the continuum truncation error.

9. **Substantially narrow the entropy/action and four-dimensional claims.**

   The tuned-family laws (68)–(70) are algebraically correct and are a worthwhile observation. Equation (72), however, is not derived from the gauge-fixed Yang–Mills measure. It treats modes as independent, assigns each one a Boltzmann weight at its own threshold, and omits the Faddeev–Popov determinant, the measure along the horizon, interactions among modes, gauge-copy selection, and nonlinear color effects. Moreover, equations (71)–(73) pull a single coefficient c into β, although Section 6 itself shows that $a_{\text{crit}}(Q)/Q$ varies with Q before tending to $\sqrt{2}$. Thus c is mode-dependent at finite wave number. The power-law exponent may survive asymptotically, but the partition sum is not exact as printed.

At $d = 4$, the two displayed powers merely become volume independent; no dynamical balance or dominance in the ensemble has been demonstrated. This should be presented as a dimensional toy estimate, not as a principal physical conclusion. The BPST comparison also needs a consistent coupling convention: equation (70) gives $S_{\text{crit}} = 4\pi^4(1.905316)^2/g^2 \simeq 1415/g^2$, about 17.9 times $8\pi^2/g^2$, so calling the two simply "comparable" is misleading. The abstract's entropy/action claim should be narrowed accordingly.

10. **Reframe the paper's central novelty and literature claims.**

    The most defensible contribution is a useful normalized-operator reformulation combined with a solvable periodic example. The abstract should not state that Zwanziger's horizon function and Gribov's no-pole condition encode positivity only approximately while the new construction supplies the first direct operator statement. The exact definition of the first Gribov region was already positivity of $\mathcal{M}$, and the all-order no-pole/horizon relation is part of the cited literature. Similarly, Hermiticity of the Landau-gauge Faddeev–Popov operator is standard; the modest new observation is that the same transversality makes the normalized K_A self-adjoint.

    For a high-energy theory journal, the paper would be substantially stronger if it supplied at least one nontrivial new result beyond the algebraic congruence: for example, a rigorous signed Weyl theorem with the coefficient in equation (34), a rough-coefficient endpoint theorem, or a proof of the global periodic threshold and Feshbach bound. Otherwise, the novelty claim should be limited to organization and the illustrative Mathieu model, with a correspondingly more modest assessment of physical significance.

Minor comments

1. Section 5 should define the ghost object with color indices before taking a normalized trace. At fixed A, $\mathcal{M}^{-1}$ is the conditional ghost two-point function or Green operator, not an ensemble-averaged vacuum expectation value. Equations (37)–(42) otherwise switch silently between a fixed-color state and a scalar dressing.

2. In equation (19), transversality gives $\sum_i [\partial_i, \mathrm{ad}(A_i)] = 0$. The prose should not suggest that each component commutator vanishes separately.

3. The statement near equation (49) that overlap with the critical mode approaches one "typically" near an isolated crossing is unsupported. Under amplitude scaling the eigenvector is fixed, so a small overlap remains small all the way to the crossing.

4. The bibliography entry currently associated with Cucchieri and Mendes mixes two publications. Phys. Rev. D **88**, 114501 (2013) is "Ghost sector and geometry in minimal Landau gauge: further constraining the infinite-volume limit," arXiv:1308.1283. "Crossing the Gribov horizon" is arXiv:1311.4699 and should be cited separately.

5. The Fourier and plane-wave normalizations, the allowed integer values of Q and m, and every projected momentum sum should be stated explicitly. This will also make clear where volume factors and the omitted zero mode enter Proposition 2.

6. The notation in the first sentence of Section 7 contains a malformed charged-sector kernel. It should be rewritten after the convention for the $\omega^{\pm}$ basis and for g is fixed.

7. Sections 9 and 10 repeat much of the abstract and earlier caveats. Once the claims are corrected, these sections could be shortened and used to give a sharper list of proved results, numerical observations, and open problems.

Conclusion

The manuscript contains a correct and useful finite-volume form congruence and a largely sound periodic Mathieu/Feshbach calculation. Those ingredients merit further development. The present version nevertheless draws several central conclusions that do not follow: the zero-momentum no-pole functional is not defined on the chosen Hilbert space, the periodic comparison uses the wrong color normalization, the small-eigenvalue lattice interpretation ignores the difference between congruence and similarity, and the four-dimensional entropy/action claim is only a model-dependent dimensional estimate. Correcting these points will change both the quantitative conclusion and the paper's stated novelty. I therefore recommend a comprehensive major revision before the work is considered further.