Referee Report

"Ising the way into de Sitter"

arXiv:2607.14219

Summary and overall assessment

The manuscript studies the thermally deformed two-dimensional Ising model on rigid dS_2 by using its fermionisation to a free massive Majorana field. This is a well-chosen solvable laboratory for questions that are otherwise difficult in interacting QFT in de Sitter space. The paper first evaluates the massive sphere determinant and matches it to conformal perturbation theory (CPT), including a nontrivial finite order-ν^4 integral. It then derives the exact energy two-point function, gives a small-mass expansion in multiple polylogarithms, and studies its Lorentzian late-time limit. Finally, it uses the Doyon–Fonseca differential system for the spin and disorder two-point functions, derives an antipodal ratio, solves the system numerically, and compares the resulting late-time exponents with CPT.

There is substantial good work here. In particular, the agreement between the Majorana determinant and the integrated Ising four-point function at order ν^4, the independent exact/CPT comparison for the energy correlator at order ν^2, and the agreement between the Ward-identity and CPT treatments of the spin correlator are strong checks. The antipodal spin/disorder ratio is also an elegant and useful input. Subject to correction and better documentation, these results should be interesting to the de Sitter QFT and integrable-QFT communities.

However, a central formula in the advertised late-time analysis is algebraically incorrect. Equations (5.26) and (5.27) are not equal, and the error propagates directly to equation (5.29), which gives the supposedly exact leading oscillatory mixed correlator. The good news is that equation (5.26), the original hypergeometric correlator, and the CPT calculation are mutually consistent; the qualitative conclusion that secular logarithms resum into bounded oscillations therefore appears to survive. In addition, the numerical spin/disorder analysis is not presently reproducible at the level needed to support the finite-coupling claims, and the order-ν^2 renormalisation matching is formulated only through a formal use of $\zeta(1)$.

Recommendation: major revision. I would be favorable toward publication after the authors correct the late-time formulas, recheck the associated figure and claims, and provide a sufficiently explicit and controlled account of the spin/disorder numerics and renormalisation scheme.

Correctness, novelty, and significance

The logical structure of the paper is effective: it proceeds from observables that are directly accessible in the free-fermion frame to nonlocal Ising observables for which the fermionisation map supplies less direct but still powerful information. Most of the main body before the second equality in equation (5.26) is internally coherent. The Dirac spectrum and Pfaffian counting give the stated determinant, the massless limit of the energy correlator has the correct Ising normalization, and the finite CPT checks are impressively nontrivial.

The paper's most distinctive contributions appear to be the explicit Ising-frame CPT integrals, the

Lorentzian use of the sphere spin/disorder equations, the antipodal ratio, and the extraction of coupling-dependent late-time exponents. At the same time, several important ingredients are already in the cited literature: the massive determinant is standard in spirit, the exact energy correlator is acknowledged to agree with earlier work, and the spin/disorder differential equations were derived by Fonseca–Zamolodchikov and Doyon–Fonseca. The revised manuscript should identify its new results more sharply. With that clarification and the corrections below, the work is novel and interesting enough for a journal such as JHEP or PRD.

Major comments

1. **The subleading late-time energy correlator and the mixed correlator are incorrect.**

   The second line of the large-X expansion, equation (5.27), does not follow from the gamma-function expression in equation (5.26). This can be seen without making any choice of special-function conventions. Define

   $$P(\nu) = \frac{2\pi\nu}{\sinh(2\pi\nu)}, \qquad D(\nu) = \frac{\Gamma(1+\mathrm{i}\nu)^2\Gamma(1-2\mathrm{i}\nu)^2}{\Gamma(1-\mathrm{i}\nu)^2} = P(\nu)e^{\mathrm{i}\phi(\nu)}.$$

   The modulus equality in the last expression follows directly from $|\Gamma(1+\mathrm{i}y)|^2 = \pi y/\sinh(\pi y)$. Equation (5.26) then implies

   $$G_\varepsilon(X) \sim \frac{P(\nu)}{4\ell^2}\left\{\frac{1}{X} + \frac{1}{2X^2}\left[1 - \frac{\cos(2\nu\log X - \phi(\nu) + \arctan(2\nu))}{\sqrt{1+4\nu^2}}\right]\right\}.$$

   Thus division by $1+2\mathrm{i}\nu$ necessarily produces a relative amplitude $1/\sqrt{1+4\nu^2}$. In addition, the phase contains the nontrivial gamma-function phase $\phi(\nu)$; it cannot in general be replaced by the elementary phase printed in equation (5.27). The manuscript has therefore changed both the amplitude and the phase in passing from equation (5.26) to equation (5.27).

   There is a useful independent check already contained in the paper. Expanding the correct gamma-function form for small ν gives

   $$G_\varepsilon(X) = \frac{1}{4\ell^2}\left[\frac{1}{X} - \frac{2\pi^2\nu^2}{3X} + \frac{\nu^2}{X^2}\left(\log^2 X + 2\log X + 2\right) + \cdots\right],$$

   which agrees with equation (5.49). By contrast, expanding equation (5.27) yields a different polynomial in $\log X$, involving $[\log(4X)+1]^2$. Hence the inconsistency is visible even before comparing with any external result.

   Equation (5.29) must consequently also be corrected. Applying $\mathcal{D}_X = 1 + X\partial_X$ to the gamma-function form gives

   $$\langle\varepsilon_T(x)\varepsilon(y)\rangle^{\mathrm{c}} \sim \frac{P(\nu)}{8\ell^2X^2}\left[\cos(2\nu\log X - \phi(\nu)) - 1\right].$$

   Its small-ν limit begins with $-\nu^2\log^2 X/(4\ell^2X^2)$, in agreement with equation (5.50). The printed equation (5.29), including its factor $\sqrt{1+4\nu^2}$ and its $\log(4X)$ phase, fails this check.

   The authors should rederive equations (5.27) and (5.29), state the gamma-function phase and its branch convention, and verify every plot and conclusion that uses the simplified formula. In particular, the manuscript should say whether Figure 2 was generated from the full hypergeometric result (5.7), the correct expression (5.26), or the erroneous equation (5.29). The qualitative oscillatory resummation remains compelling, but the paper currently advertises an incorrect exact amplitude and phase.

2. **The spin/disorder late-time relation is conditional, and the numerical analysis needs to be reproducible.**

   Equation (6.8) follows after assuming in equation (6.7) that each complete correlator is dominated by a single nonzero pure power. This is not a consequence of conformal covariance alone: as the manuscript itself explains in Section 2, a generic de Sitter two-point function is a sum of late-time powers. Equal-real-part complex conjugate terms can produce oscillations, repeated roots can generate logarithms, and a leading amplitude can vanish at special parameters. The authors should either prove from equations (6.3)–(6.5) and the physical boundary conditions that a unique leading real power is selected, or present equation (6.8) explicitly as a conditional relation supported over a specified numerical range. Possible constant/disconnected contributions and logarithmic or oscillatory leading behaviors should be discussed.

   The actual numerical construction is described too briefly. The ratio (6.9) is valuable, but the point $u = 2$ is a singular point of the written ODE system, and a ratio alone is not an immediately transparent set of initial data. The regular antipodal expansion should be displayed. For example, if $S = G_\sigma(2)$, $M = G_\mu(2)$, and $c = (4\nu^2 + 1)/4$, regularity of equations (6.4) and (6.5) gives

   $$SG'_\sigma(2) = MG'_\mu(2), \qquad MG'_\sigma(2) + SG'_\mu(2) = -\frac{c}{2}MS.$$

   The revision should explain how these and the higher Taylor coefficients are used, how the common normalization is fixed by the short-distance condition (6.6), and how the solution is continued from the Euclidean interval through $u = 2$ to the Lorentzian spacelike region $u > 2$.

   For the exponent extraction, the paper should give the working precision, integration and fit ranges, the form of subleading corrections included in the fit, effective-exponent plateaus or comparable stability diagnostics, and numerical uncertainties. A small data table and code or pseudocode would be very helpful. Figure 4 visibly covers only roughly $0 \leq \nu \leq 0.15$, while the text says "for any value of ν" and concludes that the spin correlator has no late-time oscillations. Either a substantially broader range must be demonstrated with error control, or these statements should be limited to the range actually tested. Finally, substituting a numerical solution of the same ODEs back into equation (6.8) is a consistency check on the numerics, not independent evidence for the physical ansatz.

3. **The order-ν^2 matching requires an explicit common regulator.**

   The determinant calculation correctly isolates the local m^2 ambiguity, and the finite order-ν^4 match is a strong scheme-independent result. At order ν^2, however, equations (4.9), (4.12), and (4.22), together with equations (B.5)–(B.6), assign the same symbol $\zeta(1)$ to two divergent quantities. Appendix B explicitly calls the equality formal. Saying that both manipulations can be regulated is not yet a demonstration that the spectral and coincident-point regulators have identical normalization and finite parts. This is especially important because the comparison is presented as the derivation of the precise map $\tau = m/(2\pi)$ in equation (4.24).

   The authors should use a common covariant regulator, such as a proper-time/heat-kernel or point-splitting regulator, and show the coefficient of the logarithm and its local cosmological-constant counterterm in both frames. The renormalization scale and the mapping of the finite parameter α should be stated. Alternatively, the universal logarithmic coefficients may be compared while the mass/operator normalization is derived independently from the fermionisation map. The revised discussion should also distinguish the radius dependence fixed by the Weyl anomaly from the absolute, scheme-dependent normalization of a sphere partition function. Likewise, a normalization convention is needed for the statement that the Ising and Majorana sphere partition functions simply agree; a finite gauge-volume or Euler-counterterm factor would cancel from the normalized ratios used here but not from an absolute equality.

4. **The representation-theory and analytic-continuation claims should be calibrated more carefully.**

   The paragraph entitled "UIRs inside $\langle\varepsilon\varepsilon\rangle$" rewrites the exact answer in terms of the constituent fermion parameters (Δ, s). It does not perform a Kallen–Lehmann decomposition of the scalar composite correlator into de Sitter UIRs; indeed, that decomposition is correctly listed as future work in Section 7. In the same vein, powers $X^{-2\pm 2\mathrm{i}\nu}$ in the subleading energy correlator are naturally related to two-particle/composite combinations of the fermionic modes, but their mere appearance does not by itself identify an exchanged scalar principal-series UIR. The title of the paragraph and the statements following equations (5.26)–(5.29) should be qualified unless the relevant spectral decomposition is actually supplied.

   The continuation in equations (2.12) and (5.21) is sufficient for the equal-time spacelike sector used in the paper, for which $X > 1$. It is not a complete definition of a general Lorentzian two-point function. For timelike separation, hypergeometric functions and polylogarithms encounter branch cuts, and Wightman, time-ordered, and other correlators depend on the $\mathrm{i}\epsilon$ contour. The claims should either be restricted explicitly to equal-time spacelike cosmological correlators or supplemented with the continuation path, branch choices, and operator ordering. The coordinate-dependent operator $\varepsilon_T = (1 - T\partial_T)\varepsilon$ should likewise be described as a late-time filter in the chosen conformal coordinates, rather than as an ordinary covariant local descendant; it is important to state which time is held fixed when its derivative acts.

5. **Clarify which results are new and the precise scope of "exactness."**

   The manuscript should compare the determinant directly with the massive-Majorana effective-action literature already cited later in the paper, explain which parts following the known energy correlator are new, and distinguish the new Lorentzian/numerical use of the Doyon–Fonseca equations from the equations themselves. The relation to the earlier Schwinger-model resummation should also be stated more explicitly: the conceptual lesson about secular terms is anticipated there, while the new value here lies in the Ising/Majorana realization, the explicit CPT integrals, and access to nonlocal spin fields. The assertion that this is the first solvable example with a direct Majorana role should be supported or softened.

   Relatedly, the exact closed-form result is in the energy sector. The spin and disorder correlators are characterized by known nonlinear ODEs, a new boundary ratio, and numerical solutions, with a conditional asymptotic relation. Phrases such as "exact spin and disorder two-point functions" and broad statements that perturbation theory is "generically" spoiled should be adjusted to match what has actually been demonstrated.

6. **The antipodal-ratio argument should make its factorisation assumptions explicit.**

   Appendix D gives an appealing derivation of $G_\mu(2)/G_\sigma(2) = e^{\pi\nu}$. Since this ratio is the central numerical boundary condition, the manuscript should explicitly show that the zero-mode Hamiltonian commutes with the oscillator evolution at unequal Euclidean times, that the order and disorder states have identical nonzero-mode components, and that endpoint Weyl factors and relative operator normalizations cancel in the ratio. These statements appear plausible from equations (D.19)–(D.23), but spelling them out would remove an otherwise consequential hidden assumption.

Minor comments

1. Equation (5.20) contains $\pi^3 \log^2 X/3$. Equations (5.10) and (5.17) instead give $\pi^2 \log^2 X/3$: since $p_1 = -\pi^2/3$ and the relevant term in f_1 is $-\log^2 X/(1-X)$, their product has π^2, not π^3. The adjacent "$+-$" should also be corrected, and the displayed low-order coefficients should be rechecked.
2. The counterterm action in equation (4.10) is integrated over S^1_ℓ although the calculation is on S^2_ℓ.
3. Section 3 states that each single relevant deformation flows to "a trivially gapped vacuum." This conflicts with the later, correct statement that one sign of the thermal deformation has two degenerate vacua. "A trivially gapped phase" would avoid the conflict, with the vacuum structure then specified by the sign of the mass.
4. Appendix B describes a generic Mobius transformation fixing three complex points as an SO(3) isometry. Such a map is generally in $\mathrm{PSL}(2,\mathbb{C})$, not the SO(3) isometry subgroup. The subsequent Weyl factors suggest that a general conformal transformation is intended; the wording and the associated measure decomposition should be corrected.
5. The range of convergence of the all-order small-ν expansion should be stated in Section 5, along with branch conventions for the multiple polylogarithms after continuation to $X > 1$.
6. If first- and second-order CPT contributions are called "one loop" and "two loop," the loop-counting convention should be defined; the terminology is not automatic for integrated CFT correlators.
7. The incomplete forthcoming bibliography entry keyed `ThirringScwhinger` has neither title nor year and should be completed or removed. The discussion of the magnetic deformation should also compare with the already listed work of Grinza and Magnoli on the Ising model on the sphere.
8. Several source strings display encoding corruption in names and words such as "Painleve," "ansatz," and "Mobius." These should be corrected in the published file.

Recommendation

The manuscript contains a promising and potentially publishable set of results, and the main exact framework appears sound. Nevertheless, equations (5.27) and (5.29) are central to one of the headline conclusions and are presently wrong, while the spin/disorder finite-coupling claims lack the numerical and asymptotic support required for independent assessment. I therefore recommend **major revision**. A revision that corrects the late-time phase and amplitude, verifies the figures, supplies controlled numerical details, and makes the regulator and novelty claims precise would merit serious consideration for publication.