Referee Report

Nonadditivity in Quantum Field Theory: Replica Energies, Scaling Filters, and the Renormalization Group

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Summary

The paper develops a thermodynamic and field-theoretic diagnostic for violations of extensivity and additivity based on the "replica energy"

$$\beta\mathcal{E}(L,\beta) = \left(1 - \frac{1}{d}L\partial_L\right) W(L,\beta), \qquad W = \log Z.$$

The basic observation is that this differential operator annihilates the leading volume term in the canonical free energy and therefore isolates subleading, non-homogeneous contributions. The authors then use this idea in several settings: finite-size corrections in local QFT, a proposed infrared criterion for genuine nonextensivity due to unscreened long-range mediators, topology-dependent constants in gapped $(2+1)$-dimensional phases, subextensive ground-state degeneracy in fracton phases, and filtered sphere partition functions related to c, F, and a data along RG flows.

The manuscript has a clear unifying point. It is useful to view surface terms, topological degeneracies, fracton degeneracies, long-range interactions, and conformal anomaly data through the same family of scaling filters. The sections on topological order and fracton phases are particularly transparent: once the large-size form of W is assumed, the replica-energy filter gives immediate and physically interpretable answers. The discussion of the three-dimensional bulk sphere filter versus the defect/entanglement filter is also worthwhile, because it explains why a filtered sphere partition function can have the correct fixed-point values without being an RG monotone.

In my view the paper is potentially publishable in a high-energy theory journal, but not in its present form. The central construction is sound as a kinematic diagnostic, and the range of applications is interesting. However, several of the stronger claims require sharper hypotheses and a more complete argument. The most important issues concern the ensemble and density assumptions in the long-range examples, the exact assumptions behind the Ward-identity derivation, the status of the Newtonian/nonextensive model, and the interpretation of the four-dimensional filtered quantity as related to the a-theorem.

Recommendation

I recommend publication after major revision.

The requested revisions are not, in my judgment, fatal to the project. They mostly require the authors to state assumptions more carefully, moderate a few claims, and fill in missing derivational steps. At the same time, they are substantial enough that I would not recommend acceptance after only cosmetic changes. The paper's best version should make completely clear which conclusions are exact identities, which are finite-size scaling consequences, which are perturbative or mean-field arguments, and which are interpretive connections to known RG monotonicity theorems.

Evaluation of Correctness and Logical Structure

The core algebraic construction is correct. If W contains an extensive term proportional to L^d, then $(1 - \frac{1}{d}L\partial_L)W$ removes it. If a subleading term scales as L^s, the filter returns $(1 - s/d)L^s$ times its coefficient. This simple observation justifies much of the later classification table. The derivations for topology-dependent constants and for X-cube-like linear degeneracy are also straightforward once the assumed asymptotic forms of Z are granted.

The broader logical structure is less uniformly secure. The paper moves between canonical thermodynamics, zero chemical potential, nonzero thermal densities, finite-size QFT, lattice fracton systems, and sphere partition functions. These are related topics, but the assumptions are not identical. A reader needs clearer signposts about the ensemble, the scaling limit, and the quantities held fixed in each application. In particular, the early statement that the rest of the work is at "$N = 0$ (or zero chemical potential)" is not equivalent to the later use of a nonzero density one-point function in the long-range criterion. This is not a merely verbal issue: the sufficient criterion for nonextensivity explicitly requires a nonzero one-point function, while in many charge-conjugation-symmetric zero-chemical-potential ensembles the relevant current one-point function vanishes.

The Ward-identity derivation is plausible but should be made more precise. The derivation is written for a flat thermal manifold $\Sigma_L \times S^1_\beta$ and uses the trace of the improved stress tensor. This is enough for the stated flat-space formula if boundary terms, virial-current divergences, finite-volume contact terms, and possible anomaly terms are absent or controlled. But later parts of the paper discuss curved spheres and topological manifolds. The authors should explicitly separate the flat thermal Ward identity from the curved-space sphere-filter constructions, and they should state the conditions under which total derivatives and boundary contributions vanish. The notation also obscures the formula: the same symbol β is used both for inverse temperature and for beta functions. A notation such as β_g or B_g would make the central equation much easier to parse.

The finite-size scaling discussion is useful but somewhat too sweeping. The expansion involving perturbations with RG eigenvalues y_a is a reasonable schematic near a fixed point, but the interpretation of relevant perturbations as producing superextensive corrections needs qualification. In a standard massive flow, a relevant coupling introduces a correlation length, and at sufficiently large L the free energy again becomes extensive, with exponentially small finite-size corrections, unless one is considering a special scaling regime near criticality. The paper should distinguish finite-size scaling at fixed $g_a L^{y_a}$ from the thermodynamic limit at fixed physical mass or coupling.

Major Comments

1. **Clarify the ensemble and the role of nonzero one-point functions.**

   The paper states early on that it specializes to $N = 0$ or zero chemical potential, but the nonextensivity criterion and the long-range examples require a local operator with a nonzero expectation value. This tension appears in the QED discussion and again in the Newtonian example. At zero chemical potential in ordinary QED, the net charge density is expected to vanish by charge conjugation symmetry, so the criterion fails already because $\langle j^\mu \rangle = 0$, before one invokes Debye screening. If the authors instead have in mind a finite-density plasma, then the ensemble includes a chemical potential or a fixed conserved charge density, and the earlier specialization should be relaxed.

   Similarly, in the Newtonian model the mass density $\bar{\rho}$ is central to the argument. The authors should state whether the system is canonical at fixed particle number, grand canonical at a chosen chemical potential, or treated in a mean-field density ensemble. The scaling $W_{\text{int}} \sim \beta G \bar{\rho}^2 L^5$ is a fixed-density statement; it is not naturally an $N = 0$ statement. Since the main criterion depends on this point, the paper should revise the definitions and notation so that the density assumptions are consistent throughout.

2. **The sufficient nonextensivity criterion needs a more careful proof and statement of scope.**

   The "Nonextensivity criterion" is physically reasonable: an unscreened static mediator with nonintegrable spatial Green function, coupled to an operator with nonzero one-point function, can generate superextensive terms. However, the statement as written is not yet a theorem. It should specify the sign and stability assumptions, the ensemble, whether the disconnected part of the two-point function is the only relevant contribution, and how the thermodynamic limit is regulated. The phrase that a negative sign of the integral signals lack of Hilbert-space positivity also needs qualification; the sign of a Euclidean static kernel depends on conventions, gauge constraints, and on whether the field being integrated out has a positive-definite action.

   The Newtonian example is the natural place to make the criterion concrete, but the argument currently appears incomplete. The section writes down the nonrelativistic action, discusses anti-screening and the Jeans pole, and notes a logarithmic divergence near the pole. What the reader needs is an explicit derivation connecting this discussion to the criterion and to the claimed scaling of the replica energy. In particular, the paper should show the effective density-density interaction after integrating out Φ, fix the sign conventions, explain the treatment of the zero mode, and derive the L scaling of the resulting contribution to W in the same notation used in the rest of the paper.

   There is also a conceptual tension in calling the Newtonian model a controlled counterexample while emphasizing the Jeans instability of the homogeneous saddle. The instability may be precisely the physical reason the usual thermodynamic limit fails, but then the status of perturbation theory around a homogeneous state should be described modestly. A revised version should either provide a regulated stable setting in which the scaling calculation is controlled, or explicitly present the Newtonian example as a diagnostic mean-field illustration of why the usual hypotheses behind the thermodynamic limit fail.

3. **Strengthen the Ward-identity derivation and state all assumptions.**

   The trace formula for $\mathcal{E}$ is one of the paper's central claims. It should therefore be stated with enough precision that readers can see exactly what is included. The trace of the improved stress tensor may contain total derivatives, virial-current terms, curvature terms, boundary terms, and contact terms depending on the geometry and boundary conditions. The manuscript includes a footnote about a four-divergence, but the later applications make this issue more important. The authors should spell out the class of manifolds and boundary conditions for which the flat-space formula is valid, and then separately explain how the sphere partition-function filters are related but not obtained by simply applying the same flat-space identity.

   The derivation also uses derivatives with respect to renormalized couplings and masses. The authors should specify whether operator mixing, additive vacuum counterterms, and mass-independent versus mass-dependent schemes can affect the formula. Since one of the paper's main messages is that filters remove local counterterms, this is an opportunity to make the RG argument more robust.

4. **Moderate and refine the RG-monotonicity language.**

   The manuscript correctly emphasizes in several places that scaling filters are kinematic projectors, not monotonicity theorems. This is an important point and should be made consistently. The section on S^3 is convincing in this respect: the double filter extracts the fixed-point sphere free energy but is not monotone for the massive scalar, while the entropic F-function is associated with a defect free energy and strong subadditivity.

The four-dimensional discussion needs similar care. The quantity $\mathcal{A}_{\mathcal{E}} = D\mathcal{D}^{(4)}_{\mathrm{pow}} W_{S^4}$ extracts $4a$ at a conformal fixed point, and the free massive scalar calculation gives a negative derivative as a function of mR. However, the same calculation gives $\mathcal{A}_{\mathcal{E}} \to -\infty$ in the large-mass limit, rather than approaching the trivial IR value 0. Thus it should not be presented as an a-function, even in this example. The conclusion currently says that the quantity is finite, monotonically decreasing, and equal to $4a$ at conformal fixed points; this is technically true at finite mR, but it risks suggesting endpoint fidelity that the paper itself says is absent. The authors should state explicitly that the four-dimensional filter is an anomaly extractor at fixed points and a useful diagnostic away from them, but not an interpolating version of the a-theorem.

5. **Clarify the status of universality in the topological and fracton applications.**

   The topological section is largely convincing. The statement that $\beta\mathcal{E}$ isolates $\log\dim\mathcal{H}(\Sigma)$ follows directly from the factorized low-temperature partition function. The assumptions should nevertheless be stated more carefully: the temperature must be low compared with the bulk gap, the system size must be large compared with the correlation length, and possible edge modes or thermal anyons change the subleading terms. The discussion of parity anomaly effects is interesting but brief; it should be integrated more smoothly with the claim that the result is a topological Hilbert-space dimension.

   The fracton section is also a good application of the filter, but the word "universal" should be used cautiously. X-cube ground-state degeneracy is robust within a foliated phase but depends on lattice size, foliation data, boundary conditions, and microscopic regularization in a way that is different from a continuum topological invariant such as $\dim\mathcal{H}(\Sigma)$ in a TQFT. The authors do note UV/IR interplay, but the report would be clearer if the coefficient of the linear term were described as robust within a specified lattice/foliation structure rather than universal in the same sense as a CFT central charge or topological Hilbert-space dimension.

6. **Improve organization and remove draft remnants.**

   The manuscript contains material that reads as if it is still in draft form. There are commented blocks, colored-text remnants in the source, repeated statements, and some places where the text refers to a result more strongly than the visible derivation supports. The paper would benefit from a cleaner separation between: (i) definitions and exact identities, (ii) finite-size scaling consequences, (iii) examples of subextensive additivity violation, and (iv) RG fixed-point filters. The introduction is helpful but long; it could be shortened after the main text is made more self-contained.

Novelty and Significance

The individual ingredients are connected to known literatures: replica energies in nonadditive thermodynamics, finite-size scaling, trace Ward identities, topological degeneracy, fracton ground-state degeneracy, sphere free energies, and entanglement monotonicity. The paper's novelty lies in organizing these topics through a common filtering language. This is a worthwhile contribution if the authors clearly distinguish what is new from what is being reinterpreted or reviewed. The relation between the bulk S^3 double filter and the defect/entanglement filter is especially useful, because it gives a clean conceptual explanation of why identical fixed-point data do not imply identical monotonicity properties.

The significance is therefore mostly conceptual rather than computational. The paper is unlikely to change known numerical values of c, F, a, or fracton degeneracies, but it offers a coherent language for comparing finite-size nonadditivity, global topological sectors, and RG data. That is sufficient for publication, provided the claims are stated with the appropriate precision.

Minor Comments

- The notation $\beta(g)$ for a beta function is easily confused with inverse temperature β. I strongly recommend changing it to $\beta_g(g)$, $B_g(g)$, or similar.
- The definition of additivity should specify the assumptions about boundary interactions and correlations between the two subsystems. In local systems, exact additivity at finite volume is generally false unless boundary couplings are removed or neglected.
- The statement that additivity implies extensivity is true under standard replication assumptions, but the paper should indicate those assumptions, since long-range constrained systems can be subtle.
- The phrase "$N = 0$ (or zero chemical potential)" should be corrected. Fixed particle number zero and zero chemical potential are not equivalent.
- In the finite-size scaling subsection, relevant perturbations should be discussed in terms of a scaling regime near a fixed point, not as a generic large-volume thermodynamic conclusion.
- The QED example should mention that at zero chemical potential the one-point function of the electric current vanishes in a charge-neutral ensemble.
- The Newtonian action should include the chemical potential or fixed-density constraint if the later discussion assumes a finite matter density.
- In the discussion of the Jeans pole, the prescription for treating the pole should be specified. A principal-value integral, an infrared regulator, or an instability interpretation lead to different mathematical statements.
- The X-cube formula uses lattice linear sizes. The paper should say explicitly whether L_i are dimensionless numbers of unit cells or physical lengths measured in units of a lattice spacing.
- The subsection on $1 + 1$-dimensional CFTs should specify the low-temperature limit $\beta \gg L$ before interpreting the extracted quantity as the central charge.
- The statement that the entanglement-entropy replica continuation is "always possible" is too strong as written. The spectral expression gives analyticity in a half-plane for a density matrix, but QFT replica path integrals involve UV subtleties and analytic continuation is usually assumed rather than constructively proven.
- The footnote on the Pontryagin term in the four-dimensional Weyl anomaly should be phrased cautiously. This issue is somewhat tangential and should not distract from the round-sphere extraction of a.
- Several grammar issues should be corrected, for example "Let ξ the correlation length" should be "Let ξ be the correlation length."
- The conclusion should avoid saying or implying that $\mathcal{A}_{\mathcal{E}}$ is an interpolating a-function. It is an extractor of a at fixed points and, in the free scalar example, a monotone but endpoint-nonfaithful diagnostic.

- The table in the conclusion is useful. It would be even clearer if it separated subextensive but thermodynamically harmless nonadditivity from genuine nonextensivity by listing the behavior of both W/L^d and $\mathcal{E}/L^d$.

Final Assessment

The manuscript contains a publishable idea and several nice applications. The scaling-filter point is simple but powerful, and the paper uses it to connect communities that do not always use the same language. I would support publication after the authors revise the long-range/nonextensive argument, clarify the Ward-identity assumptions and ensemble choices, temper the four-dimensional monotonicity language, and clean up the presentation. With those changes, the paper would make a useful conceptual contribution to the literature on thermodynamics, finite-size QFT, and RG data.