
Report of Referee 2

Strengths

1. Computes previously unreported properties of the tricritical $O(N)$ model.

2. Discovers nontrivial surface ordering in d=3 in a less contrived scenario than those in Ref. 22.

3. Gives convincing argument why this ordering occurs.

We thank the referee for finding our work nontrivial and convincing.

Weaknesses

1. Level of grammar and precision in language is somewhat lacking.

2. Introduction does not sufficiently motivate the paper.

We appreciate the referee's constructive feedback. We have carefully revised the manuscript to improve grammar and language precision and have refined the introduction to better motivate the paper.

Report

This paper's content is well suited for the journal. However, the paper could use improvements in precision of language, uniformity of language, grammatical content, and motivation in the introduction. I have provided some unclear areas that the other referee did not mention (but both set of areas should be addressed). After making these changes, I would recommend the publication of this article.

We thank the referee for careful review and constructive suggestions. We have revised the manuscript to improve language precision, uniformity, and grammar, as well as to enhance the motivation in the introduction.

Requested changes

1. The sentence in the abstract "Then, by employing the technique of layer susceptibility, we solve the boundary operator expansion using the $\epsilon = 3 - d$ expansion" is unclear. Layer susceptibility is not a technique. Moreover, the sentence does not specify which BOE is computed.

We appreciate the referee's clarification. We have revised the sentence in the abstract as follows: "Then, using layer susceptibility, we solve the boundary operator expansion for the transverse and longitudinal modes within the $\epsilon = 3 - d$ expansion."

2. The introduction spends an entire paragraph on using layer susceptibility to compute

BOE. I would not state that this is one of the main innovations of the paper, so perhaps this calculation technique should not be presented as such.

We thank the referee for the suggestion. We have moved this part to the beginning of Sec. 2 "Mean-field correlation functions at extraordinary transition", before providing a detailed calculation of the layer susceptibility.

3. On the other hand, the introduction should provide physical motivation for studying the tricritical $O(N)$ model (e.g., the ϕ^6 term being marginal at $d = 3$ – this could affect the RG flow and boundary behavior substantially). The paper should also emphasize Ref. 22 more in the introduction rather than discussing their result in the context of Ref. 22 as an aside before the conclusion.

We thank the referee for the useful suggestion. We have revised the introduction to better motivate the context of our work. Specifically, we have placed greater emphasis on reference by Cuomo and Zhang in the introduction.

4. The sentence "Broadly, we believe... systems" likely belongs in the conclusion rather than the introduction. Moreover, the authors do not justify why this is true.

We have moved the text from p. 3 to the end of the Conclusions section and have rephrased it as "motivate further investigations into boundary criticality and its implications across diverse physical systems". For example, the nature of the extraordinary transition in chiral tricritical point in topological systems [S. Yin, S.-K. Jian and H. Yao, Phys. Rev. Lett. 120, 215702 (2018)] remains an open question. Moreover, while this work focuses on the weakly coupled regime, it would be interesting to explore strongly interacting BCFTs with nontrivial extraordinary behavior using holographic techniques.

5. The allowed values of Δ should be specified in equations 1 and 2 or in future mentions of the BOE.

As mentioned in point 2, we have moved this part to the beginning of Sec. 2 "Mean-field correlation functions at extraordinary transition". The allowed values of Δ cannot be determined a priori and must be obtained through explicit calculations on a case-by-case basis. In the manuscript, we explicitly specify the allowed values of Δ for our concrete model when discussing the BOE of the layer susceptibility.

6. Better notation for G^T on page 4 is $\langle \phi^i \phi^j \rangle = \delta_{ij} G^T$.

We have modified them accordingly.

7. The discussion of c_0 after equation 8 should be unified with the discussion of c_0 after equation 6 – we already take $c_0 \to -\infty$ after equation 6.

We have revised the manuscript accordingly to avoid this confusion.

8. What is k in Equation 18? Errors like these (that the other referee also brought up) should be proofed.

We thank the referee for the comment. We have now explicitly set $k = 4$ in corresponding equations and corrected the presentation accordingly.

9. Minor formatting point, the parentheses in Equation 28 and other places should fit the expressions.

We thank the referee for the suggestion. We have carefully reviewed and adjusted the parentheses in Eq. (28) and throughout the manuscript to ensure they properly fit the expressions.

10. The authors are not consistent with their use of tense. E.g., on page 7, the authors use future tense and present tense to describe results in the paper. Elsewhere, the authors use past tense to also describe results in the paper. I recommend using past tense for prior work and present tense for results in the paper.

We have carefully reviewed the manuscript to ensure consistency in tense usage, to the best of our ability.

11. The Green's functions in equation 31 are not labeled by longitudinal or transverse. The word "respectively" should be added after "longitudinal and transverse fields" in the sentence after this equation.

12. As an example of grammar that needs to be fixed, on page 14, the penultimate sentence in the paragraph after Equation 65 should read "can be found in Appendix B.1."

13. Likewise, the sentence after Equation 87 should be proofed, and later on that page, the authors misspell "resumming."

We thank the referee for the careful review. We have revised the manuscript according to comments 11-13. Additionally, we have reviewed the manuscript to ensure accuracy in terminology and grammar.

14. I agree with the other referee's comments on the clarity of the derivation of the extraordinary-log transition. Ref 19's argument might be easier to use for this derivation.

We thank the referee for this comment. While we keep the original argument from Metlitski's paper, we have added a sentence to point out the existence of another clear argument in Padayasi et al: "Readers may also refer to Padayasi et al for a clear and precise argument for the derivation."

15. I am quite confused by the assignment of a critical N_c in equation 106. It seems as if the authors are treating the boundary as still being a 2d plane for $d = 3 - \epsilon$. If the authors treated the boundary as a $2 - \epsilon$ d plane, a linear term would appear in the RG equation for g, and so the extraordinary-log universality class would not exist. Is treating the boundary as a 2d plane physically meaningful here? A codimension $1 - \epsilon$ defect seems odd to treat.

We thank the referee for this insightful comment. We added a sentence to point out this explicitly, "However, it corresponds to a $3 - \epsilon$ dimensional bulk theory couples to a two-dimensional boundary, which does not represent the standard codimensional-one boundary".

16. I agree with the other referee on the spelling of "Callan-Symanzik". However, I think

using "Callan-Symanzik" in this context is as appropriate as using "renormalization group equation" – I have seen both conventions used in the literature.

To enhance clarity and consistency, we have opted to use "renormalization group equation" in the manuscript.

17. As I mentioned earlier, the last paragraph on page 24 should be emphasized a lot more.

As noted previously, we have improved the emphasis on this point by explicitly highlighting it in the introduction.