

Report of Cubic asymmetric multitrace matrix model

This paper titled *Cubic asymmetric multitrace matrix model* aims to derive phase transition in matrix models and associated response functions. In the first part, the authors quickly review the matrices model basis and recall solutions of symmetric 1-cut / 2-cut and asymmetric 2-cut for the quadratic potential model. Furthermore, they define the free energy function and derive its expression for 1-cut and 2-cut cases. In the second part, an additional multitrace term tc_1c_3 , $t \in \mathbb{R}$ is added to the action, and its associated 1-cut, 2-cut solutions and free energy are studied. In particular, the authors show that no stability of the asymmetric 2-cut solutions is possible. The last part of this paper examines response functions. Heat capacity, magnetization, magnetic susceptibility, and string susceptibility are derived using the free energy.

While the topic is relevant to the field of random matrices, there are significant concerns regarding the analysis and overall contribution to the literature. **I think this paper is unsuitable for publication in its present form and for this journal.**

Major Concerns:

Journal of Physics A is a scientific journal covering theoretical and mathematical physics; however, this article has several errors or omissions in computations. First, equation (25) seems false; the authors have forgotten an integral of the eigenvalue density. Equation (26) is also problematic. In the free energy defined on (22), the second integral has its integration domain defined as $\mathcal{C} \times \mathcal{C} - \{\lambda - \tau\}$. The 2-cut solution defined in (27) has its integration domain defined by $\mathcal{C}_1$ and $\mathcal{C}_2$. The authors said that they are *substituting the proper integrals from (27) to (22)*, but doing this gives a problem with integration domains, which do not fit. After (38), the support of 2-cut solution is given by $-(D_1 + \sqrt{\delta_1}), -(D_1 - \sqrt{\delta_1}) \cup (D_2 - \sqrt{\delta_2}, D_2 + \sqrt{\delta_2})$, however $D_1, D_2, \delta_1, \delta_2$ are never given.

The part concerning the magnetic susceptibility χ must be revised. First, the definition of this quantity is unclear. χ is calculated with (58) and (60). Expression (58) is well defined, but in (60) the authors derive the magnetization m defined in (55) with respect to h , which was put to 0 in (55). This error translate at discrete level, in equation (61) m is evaluate at 0 or $\epsilon + 0$ whereas in the footnote associate to (59) m isn't evaluate at any point. Apart from this point, the authors compare the numerical results of expression (59) and (61) (which represent the same object, here χ) and claim that *the difference of the numerical values of these two methods was approximately around 1, but "1" what? Unit of magnetic susceptibility?* Furthermore, the authors choose expression (59) to continue the analysis without justification for this choice and not expression (60). Lastly, results obtained for $g = 1.5$ and $g = 2.4$ in the present paper are compared with results obtained for $g = 1.1$ and $g = 4.4$ of ref [7]. Those comparisons are doubtful, and furthermore, sentences like *Qualitatively, we found the same behaviour of the magnetic susceptibility but quantitatively we collected at least one order higher results* had nothing to do in a scientific paper. If the authors want to compare results between two papers, they should take the same values for g and make a real comparison between graphs.

The part with string susceptibility for multitrace c_2^2 is messy; the authors take several results from different articles without reminding the reader of them, which makes this part very difficult to read. Furthermore, the statement *Our numerical analysis confirmed the correctness of the critical exponents of the string susceptibility at the phase transitions derived from the original free energy* indicates a numerical analysis for the case c_2^2 . However, there is no such analysis in the present paper. Finally, the paper deals with matrix models with an additional c_1c_3 term; why is string susceptibility given for the c_2^2 case? No comparison with the results obtained for c_1c_3 string susceptibility is made.

About the scientific contribution, this study seems to offer no significant advancement in theoretical physics. The focus is on matrix models with an additional multitrace term. The authors justify this additional term in the introduction by pointing out the fact that it has already been used in other articles. However, here, the choice seems to be discussable as the authors recognize in the conclusion that *The examined mere action (40) does not describe any physical system so far*, which sheds light on the lack of physical meaning of the paper.

Other/Minor Concerns:

- The English is sometimes approximation; some examples are listed here (but the list is not exhaustive). The sentence *This means that all the contributions to the integral (4) will be suppressed but that one stable configura-*

tion lambda which globally minimizes F is lacking a verb at the end. The sentence where $P(z)$ is a polynomial of a specific uninteresting form is written in a too familiar language. This is also the case for *these two parameters should be varied in synch*, synch is an abbreviation.

- Under equation (24), there is a misprint; there is written for every $\tau \in \mathcal{C}$, it should be λ .
- In the description of numerical calculation, the authors assume that $\delta_1 \geq \delta_2$. Why this choice? Would $\delta_1 \leq \delta_2$ lead to similar results?
- The authors investigate different phase diagrams for several values of the parameters t , but they do not compare them together nor explain why they retain the value $t = -1$ for the rest of the analysis.
- Under Figure 6, the authors mention *A careful numerical investigation of the behavior of the heat capacity in the vicinity of the transition point also showed that the critical exponent is 0*. Could you first define the critical exponent you are talking about and secondly detail why it is 0?
- Figures 7 and 9 are lacking of (relevant) comments.
- The footnote *Before releasing our numerical calculations to (40), we have checked the correctness of our numerical method on the action $(M) = \frac{1}{N}[\frac{1}{2}rtr(M^2) + gtr(M^4)] \pm c_2^2$* needs further explanations. What kind of checks have been made? How is it related to the current model under analysis?

In conclusion, the article contains a considerable number of errors and imprecisions. Furthermore, there is a lack of physical meaning as the authors recognize themselves in their conclusion, which questions the relevance of this paper in the field of theoretical physics. I do not recommend the publication of this paper in the Journal of Physics A.