

Report on J.Phys.A-121909

Title: Cubic Asymmetric multitrack matrix model

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The matrix representations of many linear operators of complex systems can be well modelled by random matrix ensembles with system specific probability densities and their detailed comprehensive study is very important not only for mathematical physics but for a wide range of areas, extending from classical to quantum as well as to neural networks and AI. An important tool for the statistical analysis of many physical properties for these systems is based on the eigenvalues and eigenfunctions of the related ensembles. The theoretical derivation of the statistical measures however requires multiple integrations of the ensemble densities over undesired variables and is in general technically very complicated, more so if the ensemble density is basis non-invariant, and requires various approximations. The authors consider one such class of ensembles, with their densities consisting of 1-cut and 2-cut basis invariant parts but basis non-invariance introduced through an additional term and use saddle point approximation to solve the integrals and derive the spectral statistics. While the authors mention their lack of knowledge of the physical systems where their ensemble model may be applicable, I believe such ensembles have very important role to play in wide range of complex systems e.g. in artificial neural networks. I find the work well-written, with new routes explored to solve complicated integrals and enough mathematical details to justify the results. In my opinion, the work certainly deserves publication in JPhysA for its new scientific content.

The reports from the referees 1 and 2 consider the work significant, original, written very clearly and recommend its acceptance for publication after the corrections. As mentioned above, I agree with them.

The referee 3 suggests the work to be rejected based on several errors or omissions in computations and as an example, mentions eq.(25) which he/she believes to be false and claims that the authors have forgotten an integral of the eigenvalue density. This referee also believes that some integration domains are wrong and the work has no physical significance. I checked equation (25) and could not find any mistake. Contrary to referee's objection, I believe, the authors have used the normalisation condition for the eigenvalue density and replaced it by 1. It is usually a standard procedure and I am unable to understand why the referee 3 could not see it? The referee then claims that D_1, D_2 appearing after eq.(38) are not defined. But again it is obvious that D_1, D_2 are the solutions of the set of equations(38). Indeed I do not find any of his/ her objections deep or justified enough to reject the paper. But assuming that the referee 3 represents vast majority of the readers, I do believe that the authors could include more explanations at the points questioned by this referee. Regarding the referee 3's comment on physical significance of the work, I again do not agree with him and believe it to be significant enough for the publication in JPhysA.

I recommend the paper should be accepted for publication once the authors have correctly addressed all the concerns raised by the referees e.g. misprints indicated by the referee 2 and the comments made by referee 3.