Resubmission of “Replica approach to the generalized Rosenzweig-Porter model": answers to Referee reports

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Dear Editor,

we thank you for handling our paper which we would like to resubmit to SciPost Physics. Our paper was reviewed by three referees, whom we want to thank for their careful reading of our manuscript and for their helpful suggestions, which have undoubtedly improved our work. We were glad to read that the three referees acknowledged the scientific quality of our results. Unfortunately, we cannot agree with the judgment of Referee 1 who suggested to publish our paper in SciPost Core, since he considered that “*none of the “expectations” for SciPost Physics is clearly met*”. In fact, this opinion is in strong contrast with the report of Referee 2, who wrote explicitly that our paper “*meets all the criteria for acceptance, and meets (if not exceeds) the general expectation of clarity, correctness, and broad interest*”. Similarly, Referee 3 also started his report by writing that “*[Our] calculation opens the possibility to understand the results for the level compressibility conjectured in [23] and checked numerically in [31]*”.

Below we give a detailed response to all the issues raised by the three Referees, indicating the location of the corresponding changes in the revised manuscript. We have also highlighted in blue color, for convenience, all the changes in the manuscript. These edits appear in the Abstract, Introduction and Conclusions, as well as in Sections 1.1-2, 2.0-1, 2.4, 3.0, 3.2, 3.5-6, B. A whole new section (Appendix F) has been included in response to comment 1 of the third Referee. With these extensive changes, we hope that our manuscript will be accepted for publication in SciPost Physics.

Yours sincerely, the authors.

1 First Referee report

1.1 Reply to report

We thank the Referee for their report (and for taking the time to read and comment our paper). As we have tried to emphasize in our manuscript, the generalized RP model in the standard form which we have chosen to address (i.e., with a generic distribution of the diagonal disorder $p_a(a)$) already displays, despite its simplicity, some quite nontrivial fractal properties in its partially extended phase. This is why it has already found wide application even beyond the physics of quantum many body systems where it was initially conceived, ranging from theoretical ecology, to soft modes in disordered systems and even data science.

We do acknowledge that a lot of attention has recently been paid to the fractal/multifractal properties of the eigenstates (rather than of the eigenvalues); however, the level compressibility $\chi(E)$ which we have analyzed in the second half of our work is already sufficient to highlight the fractal nature of the intermediate phase, since it can be used to characterize the

local level statistics (see, e.g., Ref. [1]). It is also arguably simpler to address analytically – indeed, most of the previous studies analyzing the eigenstates are numerical.
The scaling form of the level compressibility which we derived in Sec. 3.5 is, in our opinion, quite an unexpected result: checking its universality in more complicated settings (e.g., Wigner/Wishart off-diagonal disorder, sparse matrices, genuine multifractal ensembles...) is clearly important in relation to quantum many-body systems and quantum chaos. Besides its physical applications, in the context of Random Matrix Theory it is very natural to ask whether the universality we found in the full counting statistics of the GRP model persists in other structurally similar random matrix ensembles, and this already motivates the need for substantial follow-up work.
Finally, many other hints and open questions have been indicated in the report of the third Referee (see also our replies). We have now revised the Conclusions of our manuscript in order to give a more comprehensive overview of these research directions.

1.2 Reply to requested changes

Referee: *Page 4: The authors claim that "the GUE case can be analyzed with only minor changes of the calculations that we develop". I doubt whether this is true. The Edwards-Jones formula, an essential ingredient in the calculations presented in this work, can for example not be trivially extended to complex-valued matrices (see e.g. Ref. [69]). This statement needs justification.*

Reply: We thank the Referee for pointing out this issue. The reason why we expected the deformed GUE case to be possibly addressed within the same formalism as in our manuscript is that a previous study (see Ref. [2]) has successfully applied this method to the pure GUE ensemble, thus accessing the statistics of the number of eigenvalues in an interval, $\mathcal{I}[-E, E]$. While the E-J formula may not be readily extended to a complex-valued matrix, on the other hand we note that the average spectral density $\rho(E)$ is formally proportional to the derivative with respect to E of the first cumulant $\kappa_1(E) = \langle \mathcal{I}[-E, E] \rangle$ – see Eqs. (10) and (99) of the revised manuscript. Of course, in order to access the finite-size corrections to the spectral density one would need to compute the cumulant generating function of $\mathcal{I}[-E, E]$ beyond the leading order in N (which is where we stopped in Section 3 of our work), but this still provides in principle a way to bypass the E-J formula.

In the revised manuscript, we have rephrased our initial claim in a less assertive form. We have also briefly addressed this discussion in the introduction of Section 3.

Referee: *Page 18: It might be helpful to add a brief comment on what is meant by "quenched" and "annealed". Although it is well-known in the field, to me it feels like a missing point since the rest of the work is essentially self-explanatory.*

Reply: We thank the Referee for spotting this missing point, which we have now briefly addressed in the introduction of Section 2.

Referee: *Page 27: If I understand correctly, "at low energy" here means "in a small range around zero energy". As "low energy" can also be interpreted as "close to the ground-state energy", it might be worth re-considering the formulation.*

Reply: Yes, what we meant was indeed *in a small range around zero energy*. We have changed the title of this Section in order to avoid possible confusion.

Referee: *Page 30: In Eqs. (B.3) and (B.4), I think $i \leq j$ should read $i < j$.*

Reply: We thank the Referee for pointing out this misprint, which has been corrected in the revised manuscript.

With these changes and clarifications, we hope that Referee 1 will be convinced that our paper deserves publication in SciPost Physics.

2 Second Referee report

We warmly thank the Referee for their kind and eager support. We also welcome their suggestion to include a comment on the "unreasonable effectiveness" of Eq. (76), which is not only a curiosity but indeed a truly remarkable point (it can now be found in the introduction of Section 3).
We have done so by also including references to Ref. [3], and to the discussion contained in Appendix A of Ref. [4].

3 Third Referee report

We thank the Referee for his numerous interesting and useful comments, to which we reply below.

3.1 Reply to report

1. (a) We thank the Referee for raising this illuminating point. The reason why we had not attempted a direct comparison with the results of Ref. [5] is that we are actually talking about two distinct (albeit structurally similar) random matrix ensembles: in our manuscript the matrix B belongs to the Gaussian orthogonal ensemble (GOE), while in [5] (at least for what concerns the calculation of the spectral form factor) it is drawn from the Gaussian unitary ensemble (GUE). In particular, the analysis in Refs. [5,6] relies on the Harish-Chandra-Itzikson-Zuber integral, a powerful analytical tool for which no equivalent exists in the GOE case.
   Nonetheless, starting from the spectral form factor studied in [5] one can indeed recover the corresponding level compressibility $\chi(E)$. This analysis is detailed in the new Appendix F of the revised manuscript (and further commented on in Section 3.5 – we have also added changes to the abstract and to the Introduction, accordingly). By choosing as a starting point the universal scaling form of the spectral form factor found in [5], we show that the scaling form assumed by $\chi(E)$ in the fractal regime *coincides* with the one of the GOE case derived in our manuscript (note that the two level compressibilities do *not* coincide outside of this regime). This result is very interesting, because it supports the idea that this universal function may stem from the structural properties of the model, and be robust against changing the details (and the symmetries) of the particular random matrix ensemble.

   (b) The coherent potential approximation heuristically consists in writing down cavity-like equations for the Green function of the model, and then replacing the self-energy by its "average" (which is a scalar). By looking at Ref. [7], it is not clear to us why the said approximation should solve exactly the RP model with Cauchy diagonal disorder – note that Eq. (50) in the first version of our paper is simply the resolvent of the pure Cauchy distribution, and not the density of states of the corresponding RP model. Still, even in different contexts, cavity-like equations are indeed sometimes solved exactly by the Cauchy distribution, at least in the thermodynamic limit [8].
   In principle, we don't see any reason why this conclusion should extend to other generic choices of $p_a(a)$; besides, we don't expect the approximation to work in any case for a large but finite matrix of size N. However, investigating the accuracy of the coherent potential approximation is surely an interesting perspective for future work.

2. We reckon that the main difference between our calculation and those performed in

Refs. [6, 9] is that the latter two considered a deformed GUE ensemble as a starting point, rather than the GOE. For instance, Ref. [6] relies explicitly on the Harish-Chandra-Itzykson-Zuber integral, for which there is no equivalent in the GOE case. Reference [9] presents instead a perturbative diagrammatic calculation, and this method could only in principle be extended to include the GOE and GSE ensembles (as the authors remark in their conclusions). Thus, as they stand, these results are not directly applicable to the GOE case addressed in our manuscript – although of course strong similarities do exist. Conversely, Eqs. (36)-(37) in our (revised) manuscript instruct on how to compute $\rho(\lambda)$ in terms of the distribution $p_a(a)$ of the diagonal disorder, for large but finite N, in the real case. We have now added several comments in the introductory sections of the revised manuscript, which will hopefully clarify these points.
Besides, although we agree with the Referee that in the numerical calculations the parameter γ can be reabsorbed in a redefinition of the parameter ν as $\nu' = \nu N^{(\gamma-1)/2}$ (and $\gamma' = 1$), we also note that ν and γ are not really interchangeable, because N is also the actual size of the matrix $\mathcal{H}$ (see the next point).

3. In our work we have adopted the standard definition of the GRP model, with $\nu \sim \mathcal{O}(1)$, so that for $1 < \gamma < 2$ and finite N it is relevant to account for finite-size corrections. But indeed our perturbative calculation is expressed in small powers of the parameter $\eta = \nu^2 N^{1-\gamma}/4$, rather than in terms of N or ν separately. As a result, the final "shapes" of $\rho(\lambda)$ which we have predicted for $\gamma > 1$ and finite N formally coincide, *only up to the lowest perturbative order in* η, with those found in a system with $\gamma' = 1$, $N' \to \infty$ and a suitable choice of $\nu'(N)$.
In fact, in the particular case $\gamma = 1$ the parameter η becomes N-independent, and it can be used to interpolate from $\rho(\lambda) = p_a(\lambda)$ to $\rho(\lambda) = \rho_{\text{GOE}}(\lambda)$ – this is known in the literature [10], and we had actually briefly recalled this point in Sec. 1.1.
While we agree with the Referee on this interesting observation, we would still prefer not to report it explicitly in the manuscript, lest raising potential confusion with the requirement $\nu \sim \mathcal{O}(1)$ adopted throughout our work.

4. The method proposed in Ref. [11] follows the technique first introduced in Refs. [12,13]. For each of the $l = 1, ..., M$ realizations of the $N \times N$ Hamiltonian $\mathcal{H}$, one constructs the matrix X whose elements X_p^l store the p-th eigenvalue in the l-th realization. One then performs the singular value decomposition of the matrix X, and constructs the so-called *scree plot* of $\lambda_k \equiv \sigma_k^2$ vs k (where σ_k is a singular value and $k = 1, \ldots, \text{rank}(X)$), or else of its Fourier power spectrum with respect to k.
Since replica methods have indeed been adopted in the past to study problems involving the singular value decomposition of random matrices (see [14] for a recent example), then in principle the problem studied numerically in [11] may be treatable by means of a replica-symmetric calculation reminiscent of the one we performed in our work.

5. This is an interesting, yet subtle point. Indeed, as correctly noted by the Referee (and as it is clear from the plots in Figs. 6-7), our scaling function $\chi_T(E)$ (which tends to zero for small arguments) does *not* reduce to the GOE result $\chi_{\text{GOE}}(E)$ (which instead tends to 1). We do have an intuition of why this happens – we had tried to express it in Sec. 3.6, but we have now commented further on this point in the revised manuscript (in the same Section). The problem stems from having estimated the cumulant generating function in Eq. (66) by using the saddle-point method, see Eq. (78). For energies down to the mean-level spacing $\delta_N \sim N^{-1}$, we are basically trying to quantify the variations of the action $\mathcal{S}$ in $e^{N\mathcal{S}}$ in correspondence of variations of $\mathcal{O}(1/N)$ in its parameters, and this is necessarily delicate. One may thus expect that the variations of $\mathcal{S}$ on this scale are too small to be accounted for by the leading order term $e^{N\mathcal{S}}$ alone, thus requiring to explicitly

resum all the next $\mathcal{O}(1/N)$ contributions. This is however technically challenging (see our attempt in Appendix D2), and we reckoned it would go beyond the scopes of our work. It is also a "known" problem in the RMT community: for instance, this is why the Tracy-Widom distribution for the top eigenvalue of a GOE matrix has never been obtained using replicas.
On the other hand, we stress that a matching between the GOE result and our scaling function does occur: indeed, for *large* arguments one has that $\chi_{\rm GOE}(E) \to 0$, while for *small* arguments $\chi_T(y) \to 0$. In fact, the typical scale for $\chi_{\rm GOE}(E)$ is given by the mean level spacing $\delta_N \simeq [N p_a(0)]^{-1}$, while the typical scale for $\chi_T(E)$ is the Thouless energy $E_T \sim N^{1-\gamma}$. This means that for $1 < \gamma < 2$ and for large N one typically has $\delta_N \ll E_T$, and the matching occurs in between these two regions.

6. We had indeed been imprecise on this point. The regular character of the distribution $p_a(a)$ at $a = 0$ seems to have been more or less tacitly assumed in most of the past literature on the GRP model: we have made the same assumption, as we have now better specified in the revised manuscript (see Section 3.5). A divergence of $p_a(a \to 0)$, albeit integrable, would likely affect the mean level spacing in that region, and its outcome on the level statistics is not clear to us (we have included this among the open problems listed in the Conclusions). We are in fact additionally assuming $p_a(0) \neq 0$, as we have now pointed out.

7. We thank the Referee for raising this interesting point. As we stressed in our manuscript, the assumption that the diagonal elements a_i are independent (and identically distributed) was unfortunately crucial in our derivation – see, e.g., Appendices B and D. As it stands, we thus see no way to address the effects of changing the *dimensionality* of $p_a(a)$ by direct application of our analytical results. However, we do agree that the generalization here proposed would possibly be within reach of the replica method, and more in general that it would be important to try to release the requirement of *i.i.d.* diagonal elements a_i so as to allow for their correlations. We have now commented on this point in the Conclusions, by also referring to Ref. [15].

8. As the Referee correctly mentioned (and as we also specified in the Introduction, Sec. 1.1), the non-ergodic extended phase of the RP model is fractal but not multifractal: we thus expect that there is no need to resort to a RSB solution in order to describe it. We have now briefly commented on this point in the revised manuscript (see Section 2.1).

3.2 Reply to requested changes

1. We agree that all the references suggested by the Referee are relevant, and we have now included them in the revised manuscript. We have also added a comment on the use of the Itsykson-Zuber formula for the Hermitian GRP model (slightly later, in Sec. 1.2, and then more extensive ones in Sec. 3.5 and Appendix F). We also thank the Referee for pointing out some misprints, which have been corrected.

2. We have now stressed in the Introduction that the deformation comes from the addition of the diagonal random matrix A to the GOE matrix B.

3. We have now added numerical data supporting the analytical prediction in Eq. (54) for the average spectral density in the Wigner case – they can be found in the new Fig. 3b.

4. The form of our Ansatz is suggested by the structure of Eq. (76), which contains the factor $\psi_a\left(-\vec{\tau}\,\hat{L}\,\vec{\tau}/2\right)$. However, in hindsight this is not exactly *natural* as we claimed, so we have now replaced that sentence with a more explicative one (in Section 3.2).

5. We believe that there is in fact no discrepancy between Secs. 3.4.1 and 3.5. First, as we think the Referee correctly intended, nowhere in the manuscript we have claimed that $E_{\max}(\eta)$ and the Thouless energy $E_T \sim \eta$ were the same – on the contrary, these two quantities do *not* coincide. Our analysis provides no clear physical intuition on the origin of $E_{\max}(\eta)$, but plots akin to Fig. 5 show that it generally depends on the specific form of $p_a(a)$. As a general rule, this maximum occurs for energies of $\mathcal{O}(1)$, hence much larger than the Thouless energy. In Section 3.4.1 we merely noted that $E_{\max}(\eta)$ (i.e., the point at which the level compressibility $\chi(E)$ is maximum) shifts towards larger energies upon increasing η, and it does so with a sublinear dependence on η (the precise growth exponent is p_a-dependent). The decay at large energies of the level compressibility $\chi(E)$ for $E \gtrsim E_{\max}(\eta)$ is a typical feature of the Poissonian regime (see Appendix A), and indeed at $E \sim E_{\max}(\eta)$ we are already into this regime: hence we see no contradiction with our statements in Sec. 3.5.
   We have now commented on this point in Sec. 3.5 of the revised manuscript to avoid further confusion.

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