Reply to the referee reports of the paper "Annealed averages in spin and matrix models"

We thank the referees for the appreciation of our work and the suggestions to improve the presentation of our results. Below you will find the reply to the referee's questions/remarks.

I. REPORT I

1. There are some typos/imperfections in the notation that should be fixed. For instance:
   (a) eqn (1): $\sum_i \tilde{s}_k^2$ should be $\sum_i \tilde{s}_i^2$
   (b) eqn (1): $\sum_k \lambda_k \tilde{s}_k \tilde{s}_k$ should be $\sum_k \lambda_k \tilde{s}_k^2$
   (c) eqn (2): $E(\lambda)$ should be $E(\boldsymbol{\lambda})$ or $E(\lambda_1, \dots, \lambda_N)$
   (d) section 2.8, line 6: 'ans' should be 'and'
   (e) eqn (35): $\boldsymbol{\sigma}_1, \boldsymbol{\sigma}_2 = \pm 1$ should be $\boldsymbol{\sigma}_1, \boldsymbol{\sigma}_2 \in \{-1, 1\}^N$
   (f) eqn (36): remove the large space betwee $\int$ and dq.

   The typos have been corrected.

2. The authors use the notation $\mathrm{D}\mathbf{x} = \prod_{i=1}^N dx_i$ throughout their paper. This I find confusing. One would normally simply have written this product as $\mathrm{d}\mathbf{x}$, and moreover in the disordered systems community the upper case D has traditionally already been used to denote the zero-average and unit-variance Gaussian measure. I see no reason for or benefit of this deviation from standard practice.

   The notation have been corrected throughout the manuscript.

3. The spherical spin models are consistently introduced with hard constraints on the spin vector length, imposed via delta-functions. However, in the subsequent calculations they actually use soft constraints: they use a real-valued Lagrange parameter, whereas the delta function would have given an imaginary one. In steepest descent expressions for mean-field models one would for $N \to \infty$ ultimately find the same end result, via contour deformation. Here, however, with the unexpected condensation phenomenon in the annealed case, it is not a priori obvious to me that the soft and the hard constrained model can be interchanged.

   This is a legitimate concern. Our solutions are in fact for hard constraint.

4. It is not fully clear why the physics of the problem follows for annealed disorder from minimization of the relevant Hamiltonians over $\boldsymbol{\lambda}$ and z. These terms that appear in the exponent of the measure that contain eigenvalues only are not coupled to the heat bath1 (they have no β), so it is not down to looking for ground states. If steepest descent is the argument, then one should devote some attention to the fact that we here have also $O(N)$ integration variables. Is it clear that the curvature around the minimum (and possibly higher order terms) do not contribute to the thermodynamics for large N?

   This is indeed a valid consideration. Even after taking into consideration the Vandermonde term (which is relevant precisely because the number of variables is order N), we still would have to justify taking the saddle point over N variables. This is at the bottom of Matrix Model Theory, and we are not in a position here to revisit this issue.

5. In section 3.1 the calculations involve normalised vectors $\boldsymbol{\sigma} \in R^N$. Why do the authors use matrix notation such as the Hermitian conjugation symbol, and write e.g. $\boldsymbol{\sigma}_a^\dagger \boldsymbol{\sigma}_b$ instead of simply $\boldsymbol{\sigma}_a \cdot \boldsymbol{\sigma}_b$, and $\boldsymbol{\sigma}_a^\dagger A \boldsymbol{\sigma}_b$ instead of $\boldsymbol{\sigma}_a \cdot A\boldsymbol{\sigma}_b$? The present notation wrongly suggests to the reader that $\boldsymbol{\sigma}_a$ is a matrix.

   The notation have been corrected throughout the manuscript.

6. In the context of comparing annealed to quenched disorder and interpretations of slowly evolving interactions between spins, the authors could perhaps refer also to a set of early papers in which that idea was developed in a more general context, starting with Penneyet al (J Phys A26, 1993), and followed by multiple extensions and applications in physics and biology (see e.g. Rabello et al, J Phys A41, 2008 for further references). The

quenched and annealed scenarios are just two special cases (n= 0 and n= 1) of a more general family, with even more extreme self-induced Mattis type status for $n > 1$. One could even ask at which value of n between 0 and 1 the condensation discussed in the present paper happens.

We have added these references.