Dear Editor and Authors,

I have carefully reviewed the submission titled "*Timescales of quantum and classical chaotic spin models evolving toward equilibrium,*" in which the authors investigate the high temperature dynamics of the 1D long-range Ising model. The study explores the relaxation of local and total magnetization, as well as the inverse participation ratio. The authors introduce a diffusive model and a driven parametric oscillator to explain the relaxation of magnetization. Next, they use a classical estimation of the energy uncertainty to explain the growth of the inverse participation ratio.

While the topic of the submission is of interest, I regret to note that the manuscript is not suitable for publication due to several significant issues with its presentation and argumentation. The writing quality is below the standard expected for publication, and many of the claims made in the abstract are not adequately supported within the body of the paper.

Having reviewed the authors' previous works, I am aware of their capability to produce high-quality research. Therefore, I believe that this submission does not reflect their usual standards of excellence. I do not believe that the manuscript meets the high standards of SciPost, and I recommend that it be revised extensively or withdrawn from further consideration.
I have detailed my specific concerns below.

Lack of scientific rigor

The ideas of linear v.s. non linear chaos is poorly developed.

A major claim of the paper is that chaos has two mechanisms for relaxation of the magnetization: linear and non-linear chaos. There are many issues with claim:

1) **The claim is not made precise and it is not clear what linear chaos is.** Eq 7 shows the equations of motion can be written as a parametric oscillator, while section V claims some of the dynamics can be captured by using equation 7 where the parameters of the parametric oscillator are given by the solution of the perturbed, non-chaotic hamiltonian. Imprecise claims are made about changing the random frequency distribution for the linear and parametric drive, but no clear way to distinguish linear from non-linear chaos is ever given.
2) As far as I could find, there is **no where in the paper where "non-linear" chaos is claimed.** The authors appear to use section V to describe all relaxation processes. It would appear the abstract and section V are in contradiction.
3) **The model parametric oscillator they use does not appear to describe the dynamics well** as L increases. The only results in this regard is shown in Fig 9. The parametric oscillator appears to only match for short times for L=21. While for L=51 it the parametric oscillator relaxes faster. Perhaps at L=100 it is even worse.
4) **No investigation of the parametric oscillator model is performed.** Does it match for all J? Does the behavior of the parametric oscillator change with J and L? How about the distribution of frequencies in the linear and parametric drive? Does the parametric drive or the linear drive dominate? When is it necessary to include the non-linear feedback?

L spin angular momenta?

The abstract claims the physical explanation for the relaxation time scales lies in the conservation of the L spin angular momenta. I didn't find any information about this in the text. The effect of breaking the conservation law is not investigated.

Energy diffusion is not observed

The authors claim that after the ballistic energy spreading, the model under goes diffusive energy spreading with $\Delta E^2 \propto t$. A significant portion of the paper is directed to identifying the time scale at which the crossover from Baltic to diffusive spreading occurs and the time at which the equilibrium occurs after diffusion. However, Fig. 2 shows that the $\Delta E^2 \propto t$ scaling only matches numerical results at a single time. The yellow line in Fig 2b, that represents the diffusive spreading ,looks like a tangent line. This is in contrast to the ballistic curve shown in purple which matches the dynamics for a sufficient time. As size increases the situation does not improve, Fig 2a shows that the diffusive lines remain poor descriptions of the energy relaxation.

Lyapunov exponent claims appear false

In section III D, it is claimed "the Lyapunov tie τ_λ as a function of J_0 is comparable to that of τ_d". I don't see this. Fig 6 shows τ_λ saturating to a fixed value, while τ_d appears to approach zero.

Misc

1) The classical initial state is not justified. Why take S_x and S_y random? I think references to work on DTWA are missing here.

Poor presentation of results

One of the most challenging aspects of reviewing this paper was grappling with the significant issues related to its writing quality. The manuscript exhibits instances of inappropriate language and an abundance of typographical errors, which significantly hindered the reading experience. A few are list as follows.

Definition of QCC: The acronym QCC (quantum classical correspondence) is heavily used in the introduction but is only defined at the end of the paper. This definition should be provided earlier to aid reader understanding, especially since it differs from the more common interpretation involving $\hbar \to 0$.

Reliance on Unexplained Results: The introduction heavily relies on results from Ref. 20, which are not explained in the paper. It's essential to either provide sufficient explanation or minimize dependence on external references to ensure the paper stands on its own.

Undefined Concepts: The Lyapunov exponent, λ_+ is mentioned but not defined. Providing a clear definition would enhance the reader's understanding of the concept. "Projection of H onto H_0" This usually means \tr[H H_0], but I don't think that is how the authors are using it.

Random Discussion: The model section includes random discussion of the Kolmogorov-Sinai entropy, which seems out of place and should be either expanded upon or removed.

Missing Definitions and Clarity: The initial state in Eq. 11 is not defined, which could lead to confusion for readers. Additionally, the term "quantum and classical model" in Eqs. 1 and 2 is unclear and needs to be rephrased for clarity. Finally, the term "single particle energies" is not defined, and it is not clear what this refers to.

Poor Terminology: Replace "non-homogenous" with "inhomogeneous" for accuracy.

Poor Sentence Structure: The sentence "Specifically, ergodicity means …" at the end of section III is a run-on sentence and should be revised for clarity and readability.

Confusing Figure Contents: What distinguishes Fig. 5a and 5b is not clearly explained. Providing a clearer description or labeling would enhance the figure's effectiveness. Clarify the meaning of "dim" in Fig. 10 to improve reader understanding.

Confusing Sentences: The sentence "Let us first consider a very small interaction …" below Fig. 8 is confusing and contains a redundancy. It should be revised for clarity. Also the sentence above Eq 21.

Poor Figure Labels and References: Explain the significance of the marks (circle, cross, diamond, etc.) in Fig. 12 to help readers interpret the figure accurately. Ensure that Fig. 12 correctly references Equations 19 and 21 for accuracy. Specify the meaning of σ in Fig 10a

Unprofessional Language:

1. Referring to the participation ratio as the "quantum observable with no classical limit" is unclear and could be stated more precisely.
2. The phrase "We decided to include it in this paper despite its lack of classical limit" could be rephrased to remove any perceived passive-aggressive tone. Consider stating the rationale for including the concept more objectively.
3. Phrases like "To keep the quantum-classical description as close as possible", "To find the time \tau_b at which .., we need to find the velocity …", and "Combining these results one gets …" are more appropriate for lecture notes and educational material, then professional articles.