Referee Report

Paper title: "Bypassing eigenstate thermalization with experimentally accessible quantum dynamics"

Apology

I sincerely apologize for the delay in submitting this referee report.

Summary

This manuscript studies variants of the eigenstate thermalization hypothesis that are suitable for experimentally accessible settings and realistic applications. The main results consist of introducing a notion of *energy-band thermalization* and deriving its implications for autocorrelation functions through rigorous inequalities. These results are strongly inspired by earlier discussions of ergodicity in semiclassical theories.

Strengths

The main strength of this paper is that the authors generalize the eigenstate thermalization hypothesis to realistic systems, which typically exhibit degeneracies and require a discussion of thermalization at finite times.

1. In particular, the notion of energy-band thermalization discussed in Section 5 provides a nice resolution to subtleties that many papers typically dismiss by assumption for simplicity.

2. The discussion in Section 6 offers a robust clarification of finite-time thermalization. To my knowledge, this point has not been discussed clearly in the existing literature.

3. I have checked the derivations of the inequalities, and they appear to be mathematically correct.

Weaknesses

Despite the strengths listed above, there are several weaknesses, mainly related to connections with earlier discussions. I list them below:

1. Although the authors aim to address realistic situations, they do not discuss cases involving multiple observables that may not commute with each other.

2. In several places, the authors refer to a "physical basis." However, the meaning of this term is unclear to me. In realistic situations, and even from a theoretical perspective, one generally considers superpositions of such bases.

3. Sometime the sentences are very long. To clarify the nice results it is better to shorten some of them.

Detailed Comments

Below I list detailed comments and questions.

Major comments

1. The authors should clarify the difference between $\mu(\rho)$ and the notion of effective dimension discussed, for example, in P. Reimann, Phys. Rev. Lett. **101**, 190403 (2008), or in Anthony J. Short and Terence C. Farrelly, New J. Phys. **14** (2012) 013063. Similar questions also arise in **Line 1097 on page 32**. The discussion with effective dimensions are already published. For example "Typicality of Thermal Equilibrium and Thermalization in Isolated Macroscopic Quantum Systems" by Hal Tasaki for thermalization and "A Strategy for Proving the Strong Eigenstate Thermalization Hypothesis : Chaotic Systems and Holography" by Taishi Kawamoto for equilibration.

2. The authors emphasize that one strength of eigenspace thermalization is that it avoids non-degeneracy conditions. However, there already exist references pursuing this direction. For example, Anthony J. Short and Terence C. Farrelly (New J. Phys. **14** (2012) 013063) derive upper bounds without imposing non-resonance conditions. This work should be compared explicitly with the discussion in Section 8.

3. Question: In **Line 1030 on page 31**, why is $\epsilon = O(1)$ assumed? In typical situations, one expects $\epsilon = 1/\mathrm{Poly}(d)$, as suggested by canonical typicality. Similar questions also arise in **Line 1064 on page 32**.

4. I find the discussion in Section 7 confusing. It seems that γ should depend on the choice of observables. Why is this justified? In realistic situations, one should discuss thermalization with respect to multiple observables. Perhaps I am misunderstanding the motivation of this section.

5. In the discussion in Section 6, I am interested in the recent work: *"Time Evolution of Correlation Functions in Quantum Many-Body Systems"* by Álvaro M. Alhambra, Jonathon Riddell, and Luis Pedro García-Pintos. In that paper, the authors estimate the decay time of autocorrelation functions and the steady-state value using weak ETH. What is the relevance of that discussion to the present work? Are the two approaches consistent?

Minor comments

1. Question: In **Line 169**, the authors comment that spatially translation-invariant systems are a special case. Is there any numerical example demonstrating ETH in non-spatially-translation-invariant systems?

2. Typo: In **Line 437**, the authors write $\hat{\rho}(t) = p_k \left|\psi_k(t)\right\rangle \left\langle\psi_k(t)\right|$, but it should read $\hat{\rho}(t) = \sum_k p_k \left|\psi_k(t)\right\rangle \left\langle\psi_k(t)\right|$.

3. In **Line 1229 on page 37**, why do the authors introduce the weight function $w(t)$ in Eq. (93)? This function does not appear to be positive definite.

4. Typo: In **Line 1793**, the authors write the Hamiltonian as $\mathcal{H}$, but it should be denoted by H.

5. Question: How does one derive the right-hand side of Eq. (D.5)?

6. For the derivation of Eq. (D.39), it would suffice to mention the Markov inequality explicitly.

Recommendation

I recommend:

- ☐ Accept, provided that the authors address and clarify all major comments listed above.

The manuscript has merit, but addressing the points above is necessary before publication in SciPost.