Dear Editor,

I read carefully the manuscript ***“Eigenstate Thermalization to non-monotonic distributions in strongly interacting chaotic lattice lattice gases”***, submitted for publication to SciPost Physics and, to be honest, I do not find it very convincing, neither very well explained and motivated.

The authors consider interacting bosons (1d) and fermions (2d) systems, studying the occupancy of single particle orbitals. Their main finding is that, even when increasing the number of degrees of freedom, remarkable deviations from the Bose-Einstein or Fermi-Dirac statistics for the single particle levels are found. My main question/concern is the following: why we should expect even for a strongly interacting system of quantum particles that the free particles statistics should be followed for the occupancy of single particle states? Perhaps the authors follows a (common?) logic according to which a strongly interacting quantum system can be seen as a system of “free” single particle states in contact with a thermal bath, where the role of the thermal bath is provided by the interaction part of the Hamiltonian? This is sometimes a “bona fide” assumption which works classical systems, supported by numerical evidence, but it is rather a speciial case rather than a general fact to be expected. I do not see why things should work different for quantum ones. In case, please explain me.

First of all the authors should clarify much better the context and provide in the introduction the general and strong examples needed to convince the reader that also for strongly interacting systems the quantum free particles statistics are to be expected for the single-particle levels of the interacting ones. I would have been more convinced by an argument proposing that weakly interacting ***quasi-particles*** excitation follows free particles statistics, but this does seems the case, all their core arguments are not build on ***quasi-particles*** modes of their systems. What are the cases and the reasons why single particle levels of interacting systems should obey independent particles statistics?

Then, I am not covinced and I am not familiar with the description of the Eigenstate Thermalization Hypothesis (ETH) at the beginning of paper, in particular Eq. (1), which is only saying that the expectation value of the observable \hat{O} varies across neighbouring eigenstates of the full interacting Hamiltonian smoothly enough that its expectation on a single eigenstate can be replaced with its average over neighbouring ones. This is only part of the ETH, which also requires a “strong” assumption on the exponential decay with system size of the off-diagonal matrix elements of the kind

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<\alpha | \hat{O} | \beta > ~ exp{- S(E_av)}

where E_av = (E_\alpha + E_\beta) / 2 and S(E_av) is the corresponding entropy, where clearly it is assumed that the entropy is extensive. This second hypothesis is crucial to guarantee thermalization and “independence” on initial conditions.

Indeed the more correct enunciation of ETH is that by choosing any initial state | \psi_0 > the time evolution with Hamiltonian \hat{H} of this state induces for almost all observables \hat{O} and almost all times a probability distribution on the measures of \hat{O}which cannot be distinguished from the microcanonical one on the energy shell [E – DE , E + DE], where E = < \psi_0 | \hat{H} | psi_0 >. Something rather different from what written by the authors in the introduction and where the role of dynamics and of initial conditions is clear (which is not in the present discussion). This point should be clarified.

Starting from this initial flaw in the presentation of ETH, I find the whole following discussion and analysis of the chaoticity properties of the system not very convincing. Can we really trust simply the average value of eigenvalues spacings as a reliable order parameter from the transition from "near integrability" to "chaos"? I general the statement on chaoticity or integrability is about the full distribution of eigenvalues spacings. In full generality, it is really hard to tell the difference between two probability distributions only from the knowledge of their first moment. For instance, with reference to Fig. 4 in the appendices, if one also looks at the distributions, does he also find a consistent transition from a Poissonian-like shape to a Wigner-Dyson shape?

The authors should also be much more clear about the definition of principal components, only presented at the beginning as "the number of integrable system eigenstates comprising the non-integrable ones". This definition seems rather obscure, just a jergon used by people working daily on this topic. What does it means? Is it a sort of statement on the "vicinity" between integrable and non-integrable systems eigenstates? Or is a statement on how many integrable eigenvalues fills the gaps between non-integrable eigenvalues? And why is this related to ETH and chaoticity?

Let me now jump to the core section of the paper, which is Sec. 4, "Non-monotonic distributions". Again, I find the presentation really not clear. First of all, as a general impression, it seems to me that only here is presented a a "rough" argument to demonstrate what it is on the contrary assumed by the very beginning as a general fact, namely that the statistics of single particle levels occupancy should be similar in interacting a non interacting systems. Then, notations and explanations in my opinion are not clear. At a certain point it appears a number operator \hat{N}_k where it is not specified what the subscript index "k" refers to. They say *"Consider a particular case of the orbital occupation operator…"*. Which orbital? The single-particle non interacting system orbital? So, in this case, what the index "k" referes to? Then, again without a definition is introduced *"\overline{N}_k(E)"* and is declared that the *"shape of the microcanonical distribution of orbital occupation"*--- which has now become the "\overline{N}_k(E)"--- *"alters"*. Alters with respect to what? With respect to which previous form? Then in the following line, in a chain of equalities which should be the core argument to say that the single-particles levels statistics is similar in interacting and non-interacting systems, it comes out of the blue a new symbol, "\overline{N}_k^{int}(E)", which makes me even more confused about the meaning of its previous version without the superscript "^{int}". All these inaccuracies in the presentation makes really difficult to appreciate the relevance and rigour of the whole discussion, the core argument and the main findings.

Let me now mention another emphatic assertion of the authors, which to me seems a very general and trivial fact and, honestly, they have not succeeded in convincing me that it is not:
"Our main point is that the interactions mix different microcanonical energy shells of the non-interacting system, so that the microcanonical occupation means over the interacting system's energy shell do not match any of the corresponding microcanonical means over non interacting shells."

In its present form, I suggest rejectance of this manuscript. Due to either a lack of strength of the results or a lack of accuracy in the presentation, I do not see any true novel physical message or finding beside the almost trivial observation that the properties of an interacting and non-integrable quantum system are remarkably different from that of free quantum particles or from that of an integrable system.