Referee Responses

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1 Response to Referee Reports

We thank the referees for their attentive reading of our previous submission. Their comments have led us to perform new computations which should relieve their most serious concerns (see figure 13 of our new submission and surrounding discussion). Our previous conclusions were based on a rather inaccurate and bold extrapolation of the spectral function to the $\omega \to 0$ limit. We have now performed an scaling analysis to locate the maximum of the fidelity-susceptibility, χ. Our results indicate that the point of "maximum chaos" defined according to χ occurs exactly where one would expect, around $x \sim 2.5$ in our notation, where x is the integrability breaking parameter.

First, let us note that our primary motivation in this work is to test whether criteria for chaos proposed by us for quantum systems can be applied to classical models. Our results indicate that there are no contradictions in applying our criteria to classical models, and with the helpful suggestions of the referees, we have performed additional calculations to confirm this picture.

Let us also point out that traditional definitions of chaos in both quantum and classical systems have severe problems, which often lead to controversies in the literature. Consider the canonical example of chaotic dynamics, namely the Earth's atmosphere. This is a many-particle system consisting of air molecules that engage in collisions. Often, these dynamics are modeled classically, which is clearly accurate at long distances; however, any microscopic theory of atomic collisions necessarily invokes quantum mechanics. In this sense, one cannot define chaos without understanding how it manifests in both quantum and classical mechanics. Even if we assume that the dynamics are completely classical, it is neither practically nor computationally feasible to apply existing definitions of chaos to such a system, as it is impossible to prepare two copies of the atmosphere with nearly identical initial conditions. Moreover, such initial conditions are not even well-defined due to the quantum uncertainty principle (not to mention much larger statistical uncertainties). Hence trajectories will separate in space long before Lyapunov exponents can even be detected. Yet, it is clear that chaos exists in the atmosphere irrespective of approximations used to study the dynamics and is related to the development of long-time instabilities, such as tornadoes, which are clearly detectable in physical observables without any need

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to create two copies of the system or perform an echo.

Nevertheless, the scientific community has developed heuristics for identifying chaos in many-body systems. Returning to the example of Earth's atmosphere, we say that this system is chaotic because its long-time behavior exhibits instabilities. These long-time instabilities are naturally encoded in the time-average of trajectories in classical mechanics and correspond to quantum mechanical eigenstates. From this perspective, it is natural to associate the instability of eigenstates and/or time-averaged trajectories with chaotic dynamics. As explained in our manuscript, there is a natural quantity associated with these instabilities, the fidelity-susceptibility. Importantly, a growing body of work on both quantum and classical systems indicates that this definition of chaos is viable and the authors are not aware of any counterexamples [1,2]. For example, in Refs. [2,3] the point of maximum chaos was found near the localization transition where long-time magnetization patterns show complex fractal structures. In this sense, the definition of maximal chaos is perfectly intuitive. We note that none of OTOCs, operator spreading, or level statistics exhibit any special features at the localization transition.

Now, let us consider the key concerns raised by the referees at a high level. The intuition they have appealed to, which is indeed relevant to our study, pertains to few-particle classical systems. As they have noted, there is substantial literature on this subject and a consensus has been reached that most of the phase space is regular near integrability. There is no contradiction between this observation and our arguments; we have corrected the text that led to these concerns. Following the referee's suggestion, we defined and analyzed the fidelity susceptibility for different trajectories and found that for regular trajectories χ is finite, while for chaotic trajectories it diverges as inverse waiting/cutoff time $\mu \to 0$. We also carefully analyzed the scaling of the maximum of χ and found that our previous statement was incorrect: the maximum occurs at intermediate integrability breaking strengths, as expected. On the other hand, in extended models without a mixed phase space, maximum chaos occurs at weak integrability breaking strength, which is again completely consistent with our everyday experience.

One measure of chaos, which is favored in parts of the literature and presumably by the referees, is the fraction of phase space which exhibits chaotic dynamics. This is perhaps a reasonable measure in few-particle systems, but it does not generalize to many-body systems and does not apply to quantum systems. We expect that the phase space of even a weakly non-integrable many-body system is dominated by chaotic domains. It is therefore desirable to find alternative measures of chaos which apply equally to few-particle and many-body systems; we propose that the fidelity-susceptibility is such a measure. Having said that, we of course understand that the fidelity-susceptibility is just a number characterizing a particular average over phase space. One can study more refined measures such as the fluctuations of χ (over different trajectories, different eigenstates, or over the center of mass time of two-point functions like in glasses), large deviations of χ, the difference between typical and average fidelity susceptibility, or the full metric tensor. All of these studies are potentially very interesting and important. They have the potential to reveal additional details about the inhomogeneity of chaos, especially in non-thermalizing or slowly thermalizing regimes.

In consideration of this discussion, we have relaxed our terminology around "maximal chaos" throughout our new submission, either explicitly stating what we mean by "maximum chaos" or simply replacing "maximum chaos" with maximum χ. However, none of these considerations lead to any inconsistencies or modify our key idea that the phenomena at the heart of chaos are long-time instabilities.

Report 1

> The authors have answered all the questions and comments of the referees, and the manuscript has considerably improved. Therefore, I recommend publication.

We thank the referee for their recommendation and for their helpful comments during the editorial process.

Report 2

> To summarize my initial concerns, which remain unaddressed: the authors make a bold claim that chaos in both quantum and classical systems should be redefined based on eigenstate sensitivity, quantified by fidelity susceptibility. Unlike conventional measures such as the Lyapunov exponent, which captures sensitivity to initial conditions, the authors focus on the response of eigenstates or time-averaged trajectories (e.g., KAM tori in phase space) to changes in a Hamiltonian parameter. They find that this sensitivity, when averaged over phase space, is maximal at weak integrability breaking—stronger than in the fully chaotic (ergodic) regime.

As explained in the preceding comments, our latest calculations reveal that the fidelity-susceptibility, averaged uniformly over phase space, is maximal for $x \sim 2.5$, which is not particularly close to integrability. Our calculations reveal that, at the level of individual trajectories, the most severe late-time instabilities are present at weak integrability breaking. However, to convert this into a quantitative assessment of chaos, one needs to introduce an averaging procedure over phase space. The uniform measure suggested by the referees leads to a fidelity-susceptibility which agrees with the qualitative picture accepted by the community.

We also note that there is a growing body of work which embraces definition of chaos in both quantum and classical systems. In particular, in Refs. [2–4], it was observed that maximal χ occurs near the localization transition, which is a very intuitive result. In Ref. [1], it was shown that the fidelity approach is very practical in finding chaos in the Fermi-Pasta-Ulam chain, distinguishing it from an integrable Toda lattice. Finally, in an unpublished work, two of us have analyzed the emergence of chaos and ergodicity in a classical Ishimori chain and found that Lyapunov times only characterize instabilities along tori (this is known as a confined chaos) and are unrelated to instabilities of the tori themselves, which lead to observable chaos and thermalization.

> This is not surprising if fidelity susceptibility is primarily detecting bifurcations, such as the breaking of KAM tori as an integrability-breaking parameter is varied. By the Poincaré-Birkhoff theorem, rational tori will be broken, while KAM theorem ensures that sufficiently irrational tori persist. Thus, while the weak integrability-breaking regime is indeed fragile to parameter variations, it is misleading to call this “maximal chaos,” as most of the phase space remains regular.
>
> Rather, the fact that the authors' observable suggests "maximal chaos" in this regime should be seen as evidence that it is not a reliable indicator of chaos. If the authors instead framed their findings as showing that weak integrability

> breaking/mixed dynamics leads to maximal parameter sensitivity—without redefining chaos—I would have no objection to publication. However, given the well-established understanding of Hamiltonian chaos in low-dimensional systems, I cannot support a redefinition of chaos based on the authors' findings.

Let us distinguish between the problems of *identifying* chaos and *quantifying* chaos. In our view, chaos is equivalent to the presence of long-time instabilities in a system's dynamics. This can be diagnosed through the low-frequency behavior of the fidelity-susceptibility and in our terminology, the system exhibits chaos even if these instabilities are confined to a small region of phase space. In the context of our few-particle classical model, maximal χ appears at intermediate integrability breaking, as expected. We have also demonstrated that χ distinguishes regular and chaotic trajectories.

To summarize once more, the main point of this work is to demonstrate that the proposed definition of chaos applies to few-particle systems. However, this definition, unlike others known to us, also works in both quantum and classical many-particle systems. Moreover, it agrees with our everyday intuition that most chaotic systems are those which weakly thermalize. No one would call a metallic spring chaotic despite the existence of interacting and non-integrable electrons that rapidly relax. Yet, everyone would call a highly unstable atmosphere or burning flames chaotic. As we explained, this intuitive definition of chaos is not based on trajectories, but rather on unstable distributions. We thus do not see why we cannot define chaos through χ. Beyond these physical considerations, we have given a very precise mathematical meaning to χ and explained why it characterizes unstable (and hence chaotic) behavior.

> As I previously requested, the authors should better connect their results to established literature in both classical and quantum chaos and provide a careful phase-space analysis of their observable. Given that they focus on low-dimensional systems, this should be feasible.

We are extremely grateful to the referee for raising many valuable points. We believe that we have significantly improved our work through the editorial process by correcting erroneous conclusions through careful numerical analysis and providing additional context, discussion, and references. We disagree, however, that our proposed definition of chaos has any conceptual deficiencies. Moreover, our approach has been tested by other groups in numerous models and has returned consistent and intuitive results. Of course, one should be careful when drawing conclusions from a single number, χ. We could instead analyze additional observables such as the full distribution function of χ, or study the full metric tensor with respect to integrability breaking and integrability preserving observables as in Ref. [5]. However, such an analysis is interesting by itself and is beyond the scope of this work.

Report 3

> p3: The ref [20], is, as its title ("regular and irregular wave-function") imply, not concerned with spectral statistics and the ref [21] is a study of "level clustering in the regular spectrum", so not really focused on chaotic systems. In any case neither [20] not [21] is particularly concerned with billiard. The discussion at the bottom of p3 (sentence starting by "Perhaps the most accepted definition of quantum chaos") is too imprecise and should be modified.

We thank the referee for this point and have removed the reference to billiards. As for the concern pertaining to an “accepted definition”, most papers or research talks, at least prior to our work, have defined quantum chaos through either the spectral form factor or level statistics.

> p3 : $K(\tau)$ is a spectral statistics. The characteristic form of $K(\tau)$ mentioned is thus just a consequence of the BGS conjecture, and not of ETH (which is an hypothesis about eigenfunctions).

ETH is a generalization of BGS conjecture. Indeed, the original formulation of M. Srednicki did not have much to say about random matrix theory, but most numerical tests of ETH are conducted through level statistics. Hence, for manyy in the community, ETH is understood as a generalization of the BGS conjecture to many-particle systems. Therefore, we find no contradiction with our statement that ETH can be tested using the SFF since systems that obey ETH also obey RMT after the Thouless time (corresponding to BGS), which is also when the characteristic linear growth of the SFF occurs. We have modified the relevant sentence to avoid controversy by adding “obeying the BGS conjecture or more generally ETH" to our text.

> p4 (top) : “both ETH and the BGS conjecture are really statement about ergodicity". This is not correct, even in system (or region of phase space) which are fully ergodic, deviation from the RMT can be found if mixing is not fast enough (see eg Bohigas et al prl 1990 & Phys Rep 1993).

The referee is correct. Neither we, nor anyone known to us, has said that ETH is a necessary condition for ergodicity. In many-particle systems we are not aware of any exceptions in local models. To address this point, we have added a comment citing the review by Bohigas et. al. and a recent paper by Swietek et. al. on fading ergodicity, where they also found deviations from ETH due to long relaxation times in a so-called quantum sun model.

> p5: The paragraph “However it is intuitively clear that this effect is not related to Lyapunov instabilities [.....] will be minimal no matter how long we wait" is very confusing to me. I'm not a specialist in fluid mechanics, but my own intuition, which I think will be shared by many people, is that viscous fluids have small Lyapunov exponents, and non-viscous fluids have large Lyapunov exponents. The “intuition" mentioned by the authors is clearly not as universally shared as they assumed, and cannot be stated as fact without at least a reference to a paper where it is justified in detail.
>
> Same thing for the sentence “The fact that maximal chaos defined in this way occurs at small integrability breaking perturbations agrees with our everyday intuition as we discussed above."

We have modified our statement to highlight that as one increases the collision rate between atoms they relax faster and, at the same time, become more stable. We also added an example of a metallic spring where the electrons thermalize immediately and, for that reason, the spring is less chaotic than the air. There are of course definitions of chaos based on coarse-grained equations of motion, e.g. Navier-Stokes equations, but such approaches cannot be generic as they depend on details of coarse graining, involve extra assumptions, and do not connect well with microscopic descriptions especially when the number of degrees of freedom is mesoscopic, say $N = 100$. Further, we are not aware of any microscopic calculations of Lyapunov exponents for water molecules. Most likely they do not even exist since collisions between them are quantum in nature. It is clear that more frequent collisions in a classical gas lead to larger Lyapunov exponents and faster relaxation. How can it be otherwise?

> p7: "It is visually obvious that the motion in the top (bottom) panels is regular (chaotic). This fact can be quantified by analyzing the scaling of the distance between the two trajectories in time, which is a standard method for measuring chaoticity."
> * What is "visually obvious" is that the right and left panels looks more different in the bottom line than in the top one. But is the statement that any time dependent function with a little bit of structure is necessarily associated with chaos?
> * Again, what is propagated here is only one trajectory. So we don't know if what is represented is a property of the system, or just of this particular trajectory.

What is obvious without doing any math is that it should be very easy to map the top two trajectories onto each other by slightly modifying the canonical variables, but it is very difficult to do so for the bottom two plots. They are meant to serve as a simple, intuitive illustration for the meaning of small χ (top) and large χ (bottom). As for the last point, we spend the rest of the paper addressing it. See also our comments to the second referee report and opening comments in this response.

To briefly recapitulate, the quantity of interest to us throughout the manuscript is defined as the phase-space average of χ. This defines a property of the system, or phase space, as the referee wishes, and produces results that agree with the existing literature on few-particle chaotic systems (see figures 13 and 14 of the new submission).

> P14: "Therefore the issue of connecting trajectories defined through short time expansions to chaos remains an open problem, both in quantum and classical systems." I am not sure how much this question is open. Classical chaos is always defined as a long time property of a system (there is always a $\lim_\infty$ involved in the definition). So it is not so much of a surprise that not a lot can be told about chaos and integrability through a short term expansion of the motion. And as for "quantum chaos", it is well recognized that RMT behavior is expected only on short energy scale, namely below the Thouless energy, and that everything beyond that energy scale (which correspond to short time dynamics) does not distinguish between chaotic or regular dynamics. Connecting short time dynamics to chaos seems to me more a dead end than an open problem.

Most many-particle systems exhibit so-called confined chaos, where the Lyapunov time is parametrically shorter than the relaxation time, hence it is correct to refer to Lyapunov instabilities as short-time features. We will soon submit a separate paper on this; see also page 10 in Ref. `https://arxiv.org/pdf/1909.02145`. In a way, all kinetic approaches are based on time scale separation between fast dephasing (governed by Lyapunov instabilities in the confined chaos picture) and relatively slow relaxation of nearly-conserved momenta. There is a lot of work including recent work on operator spreading (which is defined in both quantum and classical systems) trying to connect short- and long-time dynamics. The referee is correct that there is not much progress, but there is a lot of activity. Also, to our knowledge, there is no direct connection between Lyapunov instabilities and long time-relaxation. In particular, one consequence of our work is that, in integrable systems with no Lyapunov exponents, the spectral function must vanish at low frequencies. This has now been checked successfully in tens of quantum and classical models but we are not aware of any prior mathematical explanation. Finally, this connection beyond short and long times is an open problem in disordered and glassy systems which has attracted substantial attention, but, again, only limited success.

> P14 : section 3 : it would be good to see Poincaré sections of the dynamics, to get a sens of how much chaos is present for the various set of parameters discussed. In particular, showing a single trajectory as done in Fig. 3 provides very little information about the dynamics.

Fig. 3 gives a simple visual illustration of the dynamics of a single trajectory. To get a sense of how "chaotic" the system is for the various sets of parameters, we refer the referee to Sections 4, 5 and 6, which show how "chaotic" our system is using spectral functions and fidelity susceptibilities. Again, we stress that this work focuses only on concepts which can be immediately extended to many-particle systems.

> p19: "In each case, the low-frequency weight of Φ_{ZZ} drops off as $\omega \to 0$, consistent with our expectations for integrable systems." Can the authors specify where this expectation was formulated?

In integrable systems, selection rules suppress low-frequency contributions for observables that respect the selection rules. This means that the terms $\langle n|ZZ|m\rangle$ are suppressed for small ω_{nm} and thus the spectral weight at low-frequencies vanish as $\omega \to 0$. As noted previously, this statement has now been checked in many models, see e.g. Ref. [1] for the Toda model. For classical models, this expectation follows from the intuition that tori deform smoothly under integrable deformations. In this work, we show that the mathematical formalism underlying this intuition requires that the spectral functions of integrable deformations must vanish at low frequencies.

> p21: How is defined the "strength of integrability" ?

The parameter x denotes the "strength" of the integrability breaking perturbation because at $x = 0$ (and at $x \to \infty$) the system is integrable while at $x > 0$ the system is non-integrable.

> p21: Fig 7: I was not able to locate the definition of the "rescaled" fidelity.

It is defined in p22, "...consider Fig. 7, which reports the rescaled fidelity $\mu\chi$...", which is when Fig. 7 is first mentioned in the main body of the text.

> p22: why is $S = 50$ considered as "relatively small" ?

In comparison with the XXZ model (fig. 5) and the chaotic case (fig. 8), the low-frequency behavior of the XYZ model converges substantially faster as a function of S. In comparison with either of those cases, we regard $S = 50$ as quite small for the quality of data we are able to extract.

> p24: scaling $\omega_S = 1/S^{3/4}$: is it just that the reasoning according to which the mean level spacing $\sim 1/S^2$ is too simplistic (in the sens that this mean-level spacing might vary with S), or is it that the scale ω_S eventually is not related to the mean level spacing?

We are not entirely sure, but it is possible that another relevant quantum scale that is parametrically larger than the mean level spacing enters the problem. The nature and origin of such a scale is unknown to us.

> p25 Eq. (53) : The factor $C = 3 \times 10^{-5}$ is indeed "anomalously small". Can the author explain why they cannot calculate it analytically? Is it because they can only compute the scaling $S(x/\omega)^2$, but not the full results, or because they have a prediction for the prefactor, but this prediction is off by a few orders of magnitude? In the first case they should explain what makes it impossible to get this prefactor (usually perturbative calculations are simple enough to get the full functional dependence). In the second case, such a large discrepancy may merit further discussion.

Indeed, we could do perturbative calculations using quantum mechanics. However, we have found that the limits $x \to 0$ and $S \to \infty$ do not commute. In other words, the low-frequency tail in the classical limit is non-perturbative. We have tried various scaling ansatzes but they are not convincing. In general, we agree that it is a very interesting problem to find some scaling limit for $\Phi(\omega, x, S)$ with large S, small x and small ω. Most likely it is some two-parameter scaling.

References

[1] N. Karve, N. Rose and D. Campbell, *Adiabatic gauge potential as a tool for detecting chaos in classical systems* (2025), 2502.12046.

[2] R. Świętek, P. Łydżba and L. Vidmar, *Fading ergodicity meets maximal chaos*, arXiv (2025), doi:10.48550/arXiv.2502.09711.

[3] M. A. Skvortsov, M. Amini and V. E. Kravtsov, *Sensitivity of (multi)fractal eigenstates to a perturbation of the hamiltonian*, Physical Review B **106**(5) (2022), doi:10.1103/physrevb.106.054208.

[4] D. Sels and A. Polkovnikov, *Dynamical obstruction to localization in a disordered spin chain*, Phys. Rev. E **104**, 054105 (2021), doi:10.1103/PhysRevE.104.054105.

[5] H. Kim and A. Polkovnikov, *Integrability as an attractor of adiabatic flows*, Phys. Rev. B **109**, 195162 (2024), doi:10.1103/PhysRevB.109.195162.