Response to the referees

Dear Editor,

Thank you very much for sending our paper out for review and we apologize for the delay in resubmission.

We are happy to note that referee 2 finds no weaknesses in our paper and says that the numerical results are “sufficiently clear and convincing”, the results are presented “in a coherent fashion” and that “the authors also point out the open questions based on their findings”, and recommends publication in SciPost Phys. They suggest the addition of certain references, which we have done in the revised manuscript.

We are also happy to note that referee 1 finds one of the main results of our work that PT symmetry breaking results in chaos, interesting. They say that “despite the interesting result, the paper needs a bit of a makeover and the following questions/points need to be addressed before any recommendation for publication can be made”. In this resubmission, we have addressed all the questions and comments of the referee in detail. We believe that the comments of the referee have helped us improve the presentation of our work in this resubmission and than them for the same.

Please find below a point by response to the comments of both referees along with a list of changes to the draft. We hope that this will suffice and look forward to the publication of our work in SciPost.

Thank you very much.

With best regards,

Thank you for the referee report. Below we address the comments point by point.

Response to Referee #1

Referee 01

We thank the referee for going through our manuscript in detail and coming up with comments and suggestions that we think have helped us to significantly improve the presentation of our work. We provide below a point by point response to all their comments.

Referee Comment 1

Please briefly elaborate on Wigner’s surmise that is mentioned in the first paragraph.

Response: We have added a small discussion on Wigner's surmise in the introduction.

Referee Comment 2

Page 2: the authors motivate the paper as an extension of previous works. It would be useful to describe what the previous results are and highlight the contribution of the current work.

Response: Our work has been inspired by previous work on the Hermitian-kicked rotor in [2] which presents a numerical calculation of the Out of Time Ordered Correlator (OTOC) to the study the transition from integrability to chaos in the model. Our model is a non-Hermitian extension of the aforementioned model and possesses PT symmetry thereby admitting the possibility of a PT symmetry breaking transition in addition to the one from integrability to chaos. We have studied the interplay of these transitions in the current work and identified the various phases and phase transitions. Our work is thus a significant extension of the previous work on the Hermitian kicked rotor. We have now highlighted this in the introduction.

Referee Comment 3

Page3: Define z_K, is K the strength ? Important as this forms the backbone of the work. How does one interpret this definition in a manner akin to normal level statistics for Hermitian systems?

Response: We thank the referee for pointing this out and apologize for the confusion in notation. We had used the notation z_K to simply refer to the Kth eigenvalue. The K in the subscript has no connection to the kicking strength for which we also used the variable K which, presumably, is the source of confusion. We have now modified the notation to avoid confusion and use the symbol γ to label the eigenvalues.

We use the definition of the CLSR introduced in [3] as the magnitude of the quantity $\langle r \rangle$. This is very similar to the real level spacing ratio (RLSR) which involves the ratio of differences of ordered successive real energy values. Since complex eigenvalues have real and imaginary parts, and cannot be ordered, the definition of $\langle r \rangle$ invloves calculating the Euclidean distance in the complex plane between a particular energy eigenvalue and its two closest neighbors. This has now been discussed in scetion 3.1.

Referee Comment 4

More info on the values for CLSR for Poisson and GOE... one does not get any sense of why these values are higher for PT symmetric matrices compared to the case of real symmetrical matrices.

Response: We can understand why the CLSR and the RLSR need not yield the same ratio when the spectrum is completely real in the following manner: Without loss of generality let us assume that corresponding to the energy E_i we have $E_i - E_{i-1} < E_{i+1} - E_i$ and we define $\Delta i, n = |E_i - E_{i+n}|$. The RLSR is then defined for E_i as $\Delta_{i,-1}/\Delta_{i,1}$. However, it is possible that $E_i - E_{i-2} < E_{i+1} - E_i$ in which case the CLSR as defined for E_i is $\Delta_{i,-1}/\Delta_{i,-2}$ since it involves the ratio of energy differences between an eigenvalue and its nearest and next nearest neighbors. We can thus clearly see that when averaged over all the energy eigenvalues, the CLSR is guaranteed to be greater than or at least equal to the RLSR. This has now been discussed in section 4.1.

Referee Comment 5

In Fig1, left caption: mention lambda=0, in the right figure, though RLSR is mentioned in the caption. . . there is no RLSR plotted

Response: We thank the referee for pointing this out and have now made the suggested corrections.

Referee Comment 6

Pg.3 There are two computational techniques to improve the statistics in our calculation. The first is to replace m in Eq. (1) by $m + \Delta m_p$, where Δm_p is a small random number selected independently for each p.

Please indicate what this p corresponds to. It is discussed in the appendix and it would make sense to have a part of those statements repeated in the main body of the paper. I do not see the impact of App A.1 where the coupling strength is renormalized . . . as the authors really do not study the regime $\lambda/K \gg 1$ in this work. So is this necessary ?

Response: As suggested by the referee we have clarified what p is in the main text in section 3.1 and have clarified the notation used.

Further we point out that in the non-hermitian case, without normalization, the kicking strength would be a combination of K and λ. For instance, without normalization, there would be a non-zero kicking of strength λ arising from the non-hermiticity parameter even if $K = 0$. The aim of this paper is to study the effects of changing the kicking strength and the magnitude of non-hermiticity independently. Thus, we define the complex potential with a normalization in such a way that K solely determines the strength of the kicking and λ the amount of non-hermiticity. With this definition, there is no kick when $K = 0$ regardless of the value of λ. This is especially important since we study the system for large values of λ (up to 10^2) for which the kicking strength does not increase proportionally since it is only determined by K.

Referee Comment 7

Page 5. However, in this limit, Eq. 7 does not yield the standard real level spacing ratio (RLSR) for a real spectrum. Given the plots in Fig.1, where the CLSR approaches the RSLR values for GOE etc for the Hermitian case, could the authors try and explain why the CLSR as given by Eq. 7 give different values when there is nonhermiticity ? If z_K only involves the Floquet spectrum which is real in the PT symmetric case, I do not understand what leads to these differences. In absence of any discussion on how z_K is defined, it is very opaque. A brief explanation of why one averages over the kicking strength will be useful. I think the authors should elaborate on these points. A few plots of the quasienergy spectrum in the appendices would be illuminating.

Response: We have already addressed this point in our response to 'Commment # 3' of the referee. As suggested by them, we have now added a few plots of the quasi-energy spectrum to the appendix for points in different parts of the phase diagram.

Referee Comment 8

Page 6. Regarding the different lines in Fig2, T1, T2 etc... is the boundary where PT symmetry breaking is observed a numerically obtained one by considering the Floquet eigenvalues or is there some analytical reasoning which can explain this ? Have the authors looked at possible similarity transformations of the Hamiltonian which gives rise to a pseudo-hermitian Hamiltonian, as this would help indicate the boundary between PT symmetric and PT broken regimes. This is something that one often does in non-hermitian problems (cf. Bender books and papers that the authors have referenced) Such a calculation will eminently help improve the clarity of the paper. Or is there some simplifcation that can be done in the limit $\hbar \to 0$? I would like the authors to address this.

Response:

We thank the referee for this suggestion. The psuedo-Hermitian construction for a general PT symmetric Hamiltonian can be done [1] in a fairly straightforward manner when the system of interest can be represented as a 2×2 Hamiltonian. This involves obtaining an opeartor S operator such that $S^{-1}H^{\dagger}S = H$. We are studying Hamiltonians of much larger dimensions ($\mathcal{O}(10^3)$) and it is not very clear how to generalize such a construction to such Hamiltonians. The main utility of obtaining the S operator is locating the PT symmetry breaking transition, as the referee too seems to suggest. Naively, for our system, one could try to treat the kinetic energy part of the Hamiltonian as the "unperturbed" part, construct an S operator for it and then construct the S operator of the full Hamiltonian treating the kicking term as a perturbation. However, trying to construct the S operator by the aforementioned strategy is not useful in locating the transition accurately. This is because we know that we cannot locate the PT symmetry breaking transition by treating the kicking perturbatively from our numerical calculations which show that the observed transitions takes place at $\mathcal{O}(1)$ values of K and λ. If there is indeed a perturbative construction of S to identify the transition, it will involve starting with a zeroth order Hamiltonian that already has a chaotic spectrum as opposed to the integrable kinetic energy Hamiltonian. There is no straightforward way to construct the S operator of such a zeroth order Hamiltonian, let alone trying to calculate corrections to it perturbatively.

Referee Comment 9

Page 6: Caption: Right: The absolute value of the maximum imaginary part of an energy eigenvalue, α, across the transition Main panel: T1 Inset T2, calculated for $N = 4095$. It can be seen that while α shows an abrupt change along T2, it seems to increase smoothly along T1. The CLSR on the other hand, shows an abrupt transition along both T1 and T2.

This is very confusing as, there is a rather sharp change (within numerical finite size effects) in alpha along the T1 line as PT symmetry is broken. One expects α to show a non-analytic behaviour as the system crosses an exceptional point. In the inset, along T2, there is only one point which is off the smooth straight line behaviour of alpha. The authors seem to be drawing the opposite conclusion. I would appreciate a clarification of this.

Furthermore, in the inset for alpha what is varied across the x-axis ? The caption mentions that it is the variation along the T2 line, but there both lambda and K vary. . . so this is confusing. Additionally, the scales in the inset do not conform to the color map axes.

Response: We thank the referee for pointing this out and apologize for the lack of clarity. The plots in the insets correspond to the transition T1 and the ones in the main figure to T2. We have added labels to the insets which both show a variation as a function of λ. Although we have not shown values of λ till 10^{-5} in the colorplot, we added that the date for these values in the inset line plots to show the behavior of the CLSR and α for a larger range of values. Indeed, along T2, both K and λ are proportionately varied, we only show the plots with respect to λ because that is sufficient for our purpose.

Referee Comment 10

Table1 caption . . . please mention that this is valid for the PT symmetric case only. In fact, I would recommend that the authors discuss the complex random matrix ensembles discussed in the appendix here and link it to the numerical results that obtain for the CLSR. Such a discussion would be very useful to contextualize the numerical results.

Response: We thank the referee for their input in clarifying subtle but important details regarding the contents of Table.1. It is meant to act as a reference to standard values of the CLSR and RLSR in the PT-symmetric case (all eigenvalues real) [3]. Additionally we have quoted the value of CLSR for a PT-symmetric, chaotic phase which we could not find in previous works.

Referee Comment 11

In Fig. 1, we show the mean RLSR as well as the CLSR with varying values of the kicking strength K, for $\hbar = 0.2$ and system-size N = 8005. We notice that the transition points are reasonably independent of the value of N. Thus, in what follows, the system size is chosen to be large enough so that all quantities are well converged. We find that both the CLSR and RLSR display a transition at the same value of K.

In Fig.1, why does the deviation from Poisson statistics happens at quite different K values in the Hermitian case? Is there any understanding of this that the authors can provide ?

Response: We thank the referee for this comment. Indeed on probing higher systems sizes ($N = 10095$) we see an exact match of the CLSR and RLSR lines. We have updated the plot with this new data.

Referee Comment 12

In Figs.3 and 4. I think the caption misstates which figures correspond to T1 and T2. This again confuses the reader.

Response: We thank the referee for pointing this out and apologize for the confusion. We have changed the labels to the correct ones now.

Referee Comment 13

In all the parameter regimes shown in the normalized OTOC, the long time behaviour approaches a constant. . . contrary to the authors; statement, this seems to be true irrespective of whether it is chaotic/PT broken or not. Could this authors discuss this more ?

Response: In the $\mathcal{PT}$-symmetric broken phase, the OTOC growth has two components. One from the Hamiltonian's chaotic dynamics which dominates at early times and the other from the imaginary eigenvalues which dominates at late time. The normalized OTOC suppresses the growth due to the imaginary eigenvalues i.e. $\mathcal{PT}$-symmetry breaking, and therefore any growth in this quantity is only due to chaotic dynamics. When we mentioned growth in the normalized OTOC, we meant to say this early growth due to chaotic dynamics.

Referee Comment 14

In Fig.5, it would make for easier reading if all the axes had the same ranges to see the points made by the authors.

Response: Since the different plots in Fig. 5 (in revised manuscript Fig. 6) have different values of $\hbar_{\text{eff}}$ the regions in parameter space that are accessible get severely limited due to numerical accuracy. On account of this, we have chosen different ranges for different values of $\hbar_{\text{eff}}$. It can still be seen clearly that the Poisson to GOE transition moves closer to the classical value as $\hbar_{\text{eff}} \to 0$.

Referee Comment 15

The main result of the paper is that PT breaking is always accompanied by chaos. Can the authors address why there is no PT broken phase without chaos ? Could this plausibly be a feature of the particular model studied ? In the random matrix ensembles that the authors allude to for complex matrices and eigenvalue structures, is there a possibility to have integrability to chaos transitions or does RMT prohibit these ? As this is the principal result of the work, the authors should present some arguments, atleast heuristic to bolster their results.

Response: We thank the referee for this comment. We would like to point out that PT breaking being accompanied by chaos is *one* of the main results of our paper. There are other equally important observations/demonstrations such as the ability to go from an integrable to chaotic phase by tuning the non-Hermiticity parameter alone and the use of non-Hermitian extensions of the level spacing distribution and OTOC to identify the transitions in non-Hermitian and Hermitian models.

Our numerical results do indeed indicate the absence of a $\mathcal{PT}$ symmetry broken integrable *phase*. Note however that the model is integrable with broken $\mathcal{PT}$ symmetry when $\lambda = \infty$ and $m = \infty$. In this limit, the Floquet eigenvalues are simple $iK \sin\theta$ for all $\theta \in [0, 2\pi)$. Thus, the eigenvalues are pure imaginary, occur in complex conjugate pairs showing that the system breaks $\mathcal{PT}$ symmetry. Moreover, since they can also be obtained exactly, the system is clearly integrable. However, it appears from our numerical study that this special $\mathcal{PT}$ symmetry broken integrable *point* does not extend into a phase for finite m and λ. The question of whether the absence of such a phase is special to our model or more generic will require further investigation but a priori this does not seem to be a generic feature of RMT. We hav added a dicussion in the conclusion along these lines.

Referee Comment 16

Page 9: Given that non-unitary evolutions are known to not preserve state norms, I recommend only plotting the normalized OTOC in the main body of the paper and move Fig 3 with the Norm and OTOC to the supplemental material.

Response: We thank the referee for their suggestion and have moved the plots for the norm and the unnormalized OTOC to the supplemental material keeping only the one for the normalized OTOC in the main text.

Referee Comment 17

Page 13. . . . PT symmetric phase in the phase diagram as determined from the complex level spacing ratio $\langle r \rangle$, which implies the sufficiency of PT symmetry breaking for the setting in of chaos. We also made a couple of other interesting observations, which, while not very clearly understood at this stage, motivated further work.

I think the authors mean that an integrable PT broken phase is not present in this model.

Response: Yes, this is indeed our conclusion. Note however that the model is integrable with broken $\mathcal{PT}$ symmetry when $\lambda = \infty$ and $m = \infty$. In this limit, the Floquet eigenvalues are simple $iK \sin\theta$ for all $\theta \in [0, 2\pi)$. Thus, the eigenvalues are pure imaginary, occur in complex conjugate pairs showing that the system breaks $\mathcal{PT}$ symmetry. Moreover, since they can also be obtained exactly, the system is clearly integrable. However, it appears from our numerical study that this special $\mathcal{PT}$ symmetry broken integrable point does not extend into a phase for finite m and λ. The question of whether the absence of such a phase is special to our model or more generic will require further investigation. We have added a discussion along these lines to the conclusion section.

Referee Comment 18

Page 14: typo formafter

Response: We thank the referee for pointing out the typo.

Referee Comment 19

Page 15. . . missing reference Some of these values have been calculated previously as well [?].

Response: We thank the referee for pointing this out and have now supplied the missing citation.

Referee Comment 20

In Table 2, GinOE is missing . . .

Response: We thank the referee for pointing this out. We have now included the values for GinOE in Table 2.

Referee Comment 21

Table 3. . . Is this table indicating that PT broken and PT symmetric chaotic regimes are described by the same RMT ? If yes, this is very curious as in quantum problems with no chaos, PT broken regimes have very different behaviours of observables as compared to PT symmetric regimes. So if the onset of chaos blurs any difference, this would be a very important point to make. Are there other observables/quantities that one can numerical obtain for the PTKR that explore this further ? The authors should comment on this.

Response: We would like to clarify that Table 3 only shows that if a random matrix is chosen that possesses PT symmetry (which our Hamiltonian does independent of the value of λ and K) then the resulting CLSR values are in agreement with those obtained from a larger set of random matrices in the GinOE category. PT- symmetric here only implies that the matrices have PT symmetry, not that their spectrum is such that they are in the PT symmetric phase.

Referee Comment 22

Appendix C PGE 16 The yellow shaded region is the PT symmetry broken region. . . . which figure are the authors referring to ? Cannot find yellow regions in the figures presented.

Response: We thank the referee for this, we have added the figures with yellow regions.

Response to Referee # 2

We thank the referee for going through our manuscript in detail and pointing us to important relevant references that we missed. We also thank the referee for the very positive assessment of our work.

Referee Comment 1

In the sixth paragraph of the Introduction, the authors discuss applicability of non-Hermitian Hamiltonians in various other context, such as topological phases ... This works are particularly relevant in the context of the present study as they also consider effects of interactions in NH systems with real eigenvalue spectrum (admittedly in different setups).

Response: We thank the referee for pointing us to relevant previous works in non-Hermitian systems. We have added the suggested references in our introduction.

References

[1] Ashok Das. Pseudo-hermitian quantum mechanics. In *Journal of Physics: Conference Series*, volume 287, page 012002. IOP Publishing, 2011.

[2] Efim B. Rozenbaum, Sriram Ganeshan, and Victor Galitski. Lyapunov exponent and out-of-time-ordered correlator's growth rate in a chaotic system. *Phys. Rev. Lett.*, 118:086801, Feb 2017.

[3] Lucas Sá, Pedro Ribeiro, and Toma ž Prosen. Complex spacing ratios: A signature of dissipative quantum chaos. *Phys. Rev. X*, 10:021019, Apr 2020.