Our reply to reviewer's report

(Dated: September 1, 2021)

We are grateful for the involved reading and comments made by the reviewers and are delighted that both are in favor of publication once we have addressed the issues raised. They have both captured the essence of our work beautifully in their comments and have raised excellent points. We have gone through all the individual points, addressed them and made appropriate changes in the manuscript. With our point by point response below and the improved manuscript, thanks to the reviewer comments, we hope that our work is now in publishable form, as the reviewers had prescribed.

- What authors call a new scaling regime looks to me perfectly KZ scaling with twice the dynamical exponent: $z \to 2z$. The dynamical exponent can be determined through e.g. two point functions and I suspect in the regime of new scaling $2z$ is the right exponent. If the authors agree I would suggest that they re-word their findings.

  Reply: Yes, indeed there is a more direct way to see the origin of the scaling. Since the critical behaviors in our system is only controlled by how the excitation energy scales as the system approaches the critical point, the dynamical exponent z is determined by how the typical time scale depends on the excitation energy. This simple replacement of $z \to 2z$ results from the peculiar excitation energy dependence in decoherence time ($\tau_{\mathrm{dec}} \sim \Delta^{-2}$). Note that the time scale ξ_t in standard Kibble-Zurek scaling is inversely proportional to the excitation energy $\xi_t \sim \Delta^{-1}$. This crucial difference leads to a doubling of the conventional KZ dynamical exponent in strong decoherence regime. We have added a comment to elaborate on this point in the paragraph below equation 20.

- Changing dynamical exponent with e.g. temperature is not new. E.g. it is discussed in standard textbooks like Sachdev's Quantum Phase Transitions. Here the rate translates into excitation energies and at least qualitatively plays a similar role as temperature. So the fact that one can observe different exponents as a function of rate and is not totally unexpected, though details are of course nontrivial.

  Reply: Our case is far from thermal equilibrium, which is different from the standard textbook discussions of equilibrium phase transitions, but indeed, the logic is similar. We add some comments in the paragraph below equation 20. We also tuned down the claim that the varying dynamical exponent is unexpected.

- I think the authors missed many references studying somewhat related questions....

  Reply: We thank the referee for pointing out these references. We've added them to our reference list.

- I wonder if the coupling to the bath the authors introduce can be interpreted as simply introducing random waiting times in their evolution. Then time average over these waiting times will lead to a non-unitary dynamics, which seems to be very similar to the one studied. If so, one can provide some physical basis to the considered setup.

  Reply: The bath (environment) is introduced to model the quantum non-demolition measurement of the system's energy in our setup. It is unclear to us whether measurement effect can be replaced by random waiting times or not. The way we model the decoherence is to continuously measure the system by the environment. The strength of the decoherence can be controlled by the measurement rate γ. Measurement not only leads to quantum Zeno effect which slows down the dynamics, but also creates entanglement between the system and the environment and causes the system to decohere. We believe that the decoherence effect is beyond the non-unitary dynamics of introducing waiting times.

- Right now K_j in Eq. (3) is a number, not the operator. One can interpret it as a vector. TO be an operator I guess one has to add the second index . Related to this, I simply do not understand Eq. (4). I am sure it is right as Eq. (5) looks fine, but I think that authors have to introduce right notations, write operators as operators and show how they get Eq. (4) from short time expansion.

  Reply: K_j is an operator. We add an identity operator in the first term of equation 3 to avoid this confusion.

- When the authors analyze strong coupling limit of Eq. (5) at various places, I think it is good to comment how it is consistent with perturbative derivation of Eq. (5). Is there an order of limits which is implied?

  Reply: Our discussion in the strong decoherence limit is from equation 6. To obtain equation 6 from equation 5, we did not use the perturbative expansion. Thus, there is no controlling parameter or order of limits involved. We only take the continuum limit by fixing the ratio of $\epsilon^2/\delta t$.

- What about the system size dependence in Eq. (14) and Fig. 1. Normally the gap scales as some inverse power of the system size so Eq. (14) would suggest that the second term is always irrelevant in TD limit. I guess it is good to use scaling theory instead and introduce all scales in the system and then argue which dominate in which regime.

  Reply: Equation 14 is already in thermodynamic limit. To avoid the confusion, we've changed the name of Δ to excitation energy instead of excitation gap. The typical excitation energy has a well-defined thermal dynamic limit that does not scale with system size.

- I mentioned that change in the exponent z is not unexpected as we introduce dissipation to the system. But as far as I know classical exponents are not generally given by 2 times quantum exponents. This often happens when classical dynamics is diffusive and z=2, while quantum is ballistic and z=1, but not at all universal. If the authors imply that their findings applicable even when classical exponent is not 2z, they might want to comment on the origin of the difference. Hard to imagine there are two sets of critical exponents determining same two-point functions. Maybe this model of dissipation does not always lead to equilibrium as it cools to the ground state. But still it is good to mention these things.

  Reply: When we say "classical" in strong decoherence limit, we mean the density matrix is diagonal in the energy basis. All off-diagonal terms vanish and any information about the relative phase is washed out. Thus, we called this strong decoherence limit as "classical" limit. However, this "classical" limit is not like any equilibrium distribution since we choose a special interaction between the system and environment. We agree that this name might be misleading. Thus, we add a quotation mark when we mention classical and a few sentences to elaborate on it.

- I think it is good to mention explicit derivation of Eq. (26) from Eq. (6).

  Reply: We note that each momentum sector is decoupled in our free fermion example, allowing us to use Eq. 6 in a relatively straightforward way. We added a sentence in the paragraph above equation 26.

- Why is Eq. (32) thermal entropy? In which sense is it thermal?

  Reply: Here, we actually use the equation for the Von Neumann entropy. The definition for Von Neumann entropy is $-Tr(\rho \ln \rho)$. We have now corrected it.

- What is the late time limit after Eq. (32)? If we wait infinitely long time everything should relax. Can the authors be more precise.

  Reply: The long time limit is defined to be long enough for the decoherence to have effect but short enough for other possible relaxation mechanism (not included in our paper) to affect. We added a sentence in the paragraph below equation 32.

- I have a general question on linear response calculation. Normally one assumes initial equilibrium state. Here it is not the case. So Kubo response does not directly apply (e.g. initial density matrix in interaction picture is not constant). If τ is big then the density matrix is almost diagonal/stationary. But it is good to discuss I think. I believe that implicitly the authors are doing some additional time averaging over say period T, which cannot be too short or some additional averaging over t0 should be introduced.

  Related to 11 in discussion of Chern number, with which I agree, maybe the alternative explanation could be that effectively the authors do time averaging over instantaneous density matrix to justify linear response. The Chern number of this time averaged density matrix is not conserved unless dynamics is truly adiabatic.

  Reply: Here, we do not assume that the initial density matrix corresponds to an equilibrium state. The implicit assumption we used is that $\rho(t_0)$ does not significantly change during T in equation 38. Note that T is the probing time scale and much smaller than the quench time scale τ. We added this assumption in the paragraph above equation 38.

- Typo in caption to Fig. 6. The arrows indicates...

  Reply: Thank you for pointing out this typo; we have now corrected it.