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.

- The basic idea of obtaining the modified Kibble Zurek scaling is the following: a) There exists a Kibble Zurek healing length time ξ_t (in the conventional problem without the measurement) whose scaling in terms of τ and the critical exponents ν and z are well known. b) The (weak) measurement induced decoherence leads to a new scale τ_{dec}. c) The scaling of the freezing time (and hence the quantities like defect density) are determined by the smaller scale of the two. d) If ξ_t is smaller, we have the conventional Kibble Zurek scaling (quantum regime) while in the other (strong decoherence limit where τQ is smaller),there is a modified Kibble Zurek scaling in terms of $\gamma\tau$ with a different exponent. There also exists a γ_c determining the crossover from one scaling to the other. This is like the standard equilibrium critical phenomena where the smaller scale dictates the scaling (e.g., as in finite size scaling). Is this observation correct?

  Reply: We thank the referee for accurately summarizing our main ideas. The referee's understanding is correct.

- Given points a)-d) above, it seems to me the main result of paper is summarised in point d). The modified scaling and the crossover are expected when there are two scales in the problem. In that case, the role of decoherence, which is very smartly chosen in the paper, is to introduce a new scale of the problem. This if true, should be clearly elaborated in the paper drawing analogy with the equilibrium critical point.

  Reply: We thank the reviewer for this careful analysis, which very well captures our main point. We have now elaborate on this after Eq. 20.

- The scaling of the freezing time in two limits and that of c, is valid only for the type of decoherence studied in the paper. Question therefore remains how universal these results are and with respect to what.

  Reply: Our derived scaling is applicable for any system as long as the decoherence is in the energy basis. The generic behavior of quantum system under decoherence remains an open question.

- Authors should elaborate on how appropriate is the application of the Lindblad formalism in the present time-dependent problem. A clarification is necessary here.

  Reply: To derive the decoherent dynamics of the density matrix, we start with the density matrix evolution and explicitly write down the Kraus operators (in equation 3). By taking the continuum limit, we can obtain the final result in equation 6. Interestingly, our derived result is equal to the Lindblad equation for Lindblad operator being Hamiltonian itself. We added few sentences in the paragraph below equation 6.

- Considering the model considered here in (21), how does the result change in the case of a non-linear quenching protocol? (see Physical review letters 101 (1), 016806)

  Reply: This is an excellent point that has been brought up, especially given the literature on non-linear quenches in the absence of decoherence. Suppose that a nonlinear quench as τ^{α} is considered, we can replace the current ν with $\alpha\nu$ in our derived exponent to obtain the new scaling.

- In 3.6, (irrelevant) disorder is so chosen that it apparently does not introduce a new scale in the asymptotic limit and hence there is no change in the scaling behaviour. Is this right? Then it may be connected to point d) above.

  Reply: Indeed, the presence of disorder is only to break the translational invariance but not to vary the scaling behavior. As the reviewer points out, disorder is irrelevant.

- I have a further question: what would the fate of the scaling in a non-integrable model (or models not reducible to a Bloch form) under a similar decoherence. Authors may like to comment on that.

  Reply: For non-integrable models (i.e. generic interacting systems that do not reduce to a Bloch form) under a similar energy-basis decoherence, we believe our derived scaling is still applicable since our derivation does rely on any specific model. Our discussions in Section II precisely answers this question.

- As correctly explained in the conclusion, the result is valid also for spin models. This should be mentioned in the outset while elaborating on why a topological transition is chosen for illustration of the scaling relation.

  Reply: Indeed, there is a wide class of spin models that also map onto free fermion models and would have analogous scaling behavior in appropriate measures as long as the decoherence is in the energy basis, as in our case. We appreciate the reviewer's comment and have mentioned it in the conclusion. Our topological transition case serves as a demonstrating example due to its tractable property.

- Concerning the dissipative Kibble Zurek scaling (in a gain loss type bath) the following papers....

  In connection to the Kibble Zurek scaling of the Chern insulators following two very recent papers are relevant....

  Reply: Thank you for pointing out these references. We've added them to our reference list.