**Response to the Referee 1**

## Strengths

1- Analyzing operator entanglement growth in this specific dissipative model with SU(2) symmetry is new.

2- The presentation of the results is clear and the manuscript is well written.

We thank the Referee for the positive assessment of our study about the operator entanglement growth in the SU(2)-symmetric dissipative quantum many-body system and the presentation of our manuscript.

## Weaknesses

1- The manuscript provides interesting new numerical evidence for a specific model, but no new analytical or universal understanding is achieved.

2- To judge the validity of the results a discussion on details of potential numerical errors for the data is missing. Convergence plots are missing.

We thank the Referee for raising these points.

First, for the missing analytical or universal understanding about our numerical results, we would like to note that in the U(1)-symmetric open quantum many-body system with dephasing considered in [Wellnitz *et al.*, PRL 129, 170401 (2022)], the analytical understanding about the late-time operator entanglement growth is possible because the corresponding late-time operator entanglement can be fully captured by the classical Shannon entropy associated with the probabilities for the half system being in different U(1) sectors, which is further related to the well-known classical symmetric simple exclusion process by the well-established perturbation theory in the strong dephasing limit, thus significantly simplifying the task of analysis. However, in our SU(2)-symmetric case, both the classical Shannon entropy of probabilities and the symmetry-resolved operator entanglement contribute nontrivially to the late-time operator entanglement, which makes the analytical understanding extremely hard. Moreover, since our model contains dissipation proportional to the dipole interaction between neighbor sites, even in the strong dissipation limit it is still hard to identify the density matrix $\rho$ satisfying $\mathcal{L}_0[\rho] \equiv \sum_i (L_i \rho L_i^\dagger - \{L_i^\dagger L_i, \rho\}/2) = 0$ and the corresponding projector $\mathcal{P}$ onto these states, which makes the usual perturbation theory also unrealistic for our model. Therefore, for the above reasons we do not have an analytical understanding for our present results. However, as the Referee pointed out that our work provides "*interesting new numerical evidence*," it would be interesting to further develop new theoretical tools or construct much simpler models with the same symmetry to achieve an analytical understanding of the results presented in this manuscript, which is left for future studies.

Second, to further validate our results, in this resubmission we have added a section "Details on numerical convergence" in the Appendix, in which both the discussions on potential numerical errors and convergence plots are provided.

## Report

In this manuscript the author analyzes the long-time growth of operator entanglement in a dissipative spin-chain with SU(2) symmetry. This follows up earlier works with U(1) symmetric models, where a universal log-growth behavior was attributed to the classical entropy growth stemming from symmetry block diffusion. Here, new numerical evidence suggests that also in this specific SU(2) symmetric setup, the U(1) subsymmetry leads to log-growth behavior.

Almost all general acceptance criteria are fulfilled:

1- The paper is well written and results are clearly presented (only minor suggestions).

2- Details on all physical and numerical parameters are clear.

3- Relevant literature is properly cited.

4- The conclusion is written well and results are objectively summarized

5- The introduction to the problem is well done.

6- Some more details on the numerical convergence behavior are missing and convergence plots should be provided.

We thank the Referee for the careful reading of our manuscript and the positive assessment that "*Almost all general* *acceptance criteria are fulfilled.*" In this resubmission, we have addressed all of the minor suggestions in the presentation and added the convergence plots in the Appendix.

While the results are certainly an interesting addition to the existing knowledge, unfortunately I don't agree with the author on the claimed expectations for SciPost Physics being fulfilled:

1- This work is focused on a sub-field of entanglement dynamics in open quantum spin-chains. I fail to see any synergetic link between research areas.

2- As honestly described in the manuscript, the numerical calculations lead to some follow-up insight, amending previous understanding. I don't see it opening up a new research direction. While further investigations, including e.g. SU($N > 2$) or SO($N$) symmetries would indeed be interesting, they would not be multi-pronged, and not be a consequence of this numerical paper.

3- While being interesting, I don't find the findings ground-breaking. As the author honestly writes, this work provides new numerical evidence on operator entanglement growth being a generic feature of U(1). However, this conjecture is not new, and also the paper does not add any new analytical ground-breaking understanding on setups with additional symmetries.

Overall, I therefore think that after some revisions (see below), this paper is publishable, but not in SciPost Physics. All criteria for SciPost Physics Core are fulfilled, so the paper could be published there.

We kindly disagree with the Referee on the above judgment.

First, our work indeed established a synergetic link between the quantum information theory, open quantum many-body dynamics, and non-Abelian symmetries. Operator entanglement is an important concept in quantum information theory to analyze the entangling capabilities of quantum evolution. While it has been used to study the closed nonequilibrium quantum many-body physics like quantum chaos and information scrambling very recently (see Refs. [53-55] of our manuscript), the application to the open quantum many-body systems is quite rare, especially its interplay with the symmetries—another important concept of physics. Although the Abelian U(1)-symmetric case with dephasing has been considered in [Wellnitz *et al.*, PRL 129, 170401 (2022)], it was our work that investigated for the first time the influence of the much more complicated non-Abelian symmetry on the operator entanglement dynamics of open quantum many-body systems and obtained interesting new results. Therefore, our work satisfies the SciPost Physics acceptance criterion that "Provide a novel and synergetic link between different research areas."

Second, we agree with the Referee that the criteria that "Open a new pathway in an existing or a new research direction, with clear potential for multi-pronged follow-up work" may not be fulfilled by our work. However, our manuscript does "Detail a groundbreaking theoretical/experimental/computational discovery." The reason is as follows. In [Wellnitz *et al.*, PRL 129, 170401 (2022)], the discussions were limited only to the U(1) symmetry with dephasing and no explicit conjecture about other situations was provided, from which it was not known whether the late-time logarithmic growth behavior of operator entanglement can be applied or not to other symmetries and/or dissipations. It was our work that provided for the first time a positive evidence that the logarithmic growth of operator entanglement at long times is a generic behavior of dissipative quantum many-body dynamics with U(1) as the symmetry or subsymmetry (like the SU(2)-symmetric case) and for more broad dissipations beyond dephasing, which is absolutely a groundbreaking discovery as now the logarithmic growth behavior of operator entanglement can be applied to much more broad dissipative quantum many-body systems than the one considered in [Wellnitz *et al.*, PRL 129, 170401 (2022)].

With the above points, we would like to reemphasize that our work indeed fulfills the SciPost Physics acceptance criterion that "Provide a novel and synergetic link between different research areas" and "Detail a groundbreaking theoretical/experimental/computational discovery" and is suitable for publication in SciPost Physics. We hope the Referee will also agree with this.

**Requested changes**

1- Convergence plots need to be shown and a discussion on potential numerical errors/artifacts needs to be added. For example, in Fig. 1(d) results for $\gamma = 0.05J$ and $\gamma = 0.1J$ exhibit strange wiggles. Is this due to: Fitting problems over finite time? Time-step in the simulations? Finite bond-dimension? The 4-th order Trotter decomposition usually allows to use large time-steps, but the reader should be convinced about the convergence of the results in the time-step and in the bond-dimension by showing some convergence plots in an appendix.

We thank the Referee for the suggestions. In this resubmission, we have added a section "Details on numerical convergence" in the Appendix, in which the convergence plots in time step $\delta t$ and maximum bond dimension $\chi$ are provided. The discussions on potential numerical errors/artifacts are also added. In particular, we explained in detail the fitting procedure we carried out to obtain the prefactor $\eta$ and offset $S_0$ shown in Fig. 1(c) and 1(d). The strange wiggles at late times in results for $\gamma = 0.05J$ and $\gamma = 0.1J$ (the wiggles in the later are much weaker) arise from the numerical errors due to finite time step and maximum bond dimension. Especially, the maximum bond dimension has more significant influence since the operator entanglement $S_{\mathrm{op}}$ for $\gamma = 0.05J$ and $\gamma = 0.1J$ is higher than that with larger $\gamma$, and a larger maximum bond dimension is required to achieve a more accurate result for these small $\gamma$'s. However, as we show in the manuscript, most properties of the operator entanglement, e.g., the late-time logarithmic growth and probability distribution of different symmetry sectors, have already been captured very well for our currently used maximum bond dimension $\chi = 50000$. The late-time strange wiggles in $\eta$ and $S_0$ of these small $\gamma$'s do not affect our results.

2- (minor) I find some wording in the introduction of the paper a bit "over-the-top". I suggest to remove words like "fantastic", "extremely".

We thank the Referee for raising this point. In this resubmission, we have removed these words from the Introduction.

3- Also some statements are a bit imprecise. For example, I would not call operator entanglement a "measure" for classical simulability (strictly speaking, low von Neumann entanglement entropy does e.g. not guarantee an efficient classical state representation [24]). Furthermore, fast operator entanglement growth make simulations with matrix product density operators hard, but not necessarily with another numerical technique. Lastly, I would suggest to use the acronym MPDO (matrix product density operator) instead of MPO, as the latter is more commonly used in a general context and for Hamiltonian representations.

We thank the Referee for the suggestion. The operator entanglement was first introduced in the context of quantum information theory to characterize the entangling capabilities of quantum evolutions [Zanardi *et al.*, PRA 62, 030301 (2000); Zanardi, PRA 63, 040304 (2001)]. Later, in the study of one-dimensional quantum many-body systems via tensor network methods, the operator entanglement was further widely related to the MPO simulability of quantum dynamics and provides a direct measure of complexity of the MPO representation of density matrix; see [Alba *et al.*, PRL 122, 250603 (2019)] and Refs. [9, 11, 14–17, 21] therein. We agree with the Referee that the fast operator entanglement growth mainly indicates that the numerical simulation with MPO of the quantum dynamics is hard and does not provide information about the numerical simulability of other numerical techniques. However, as we mainly focused on the study of open quantum systems via tensor network methods, the "classical simulability" in our work only refers to the MPO simulability of quantum dynamics. To make this more clear and avoid any possible confusion, in this resubmission, we have rephrased the statement as

"The presence of symmetries can lead to nontrivial dynamics of operator entanglement in open quantum many-body systems, which characterizes the cost of an matrix product density operator (MPDO) representation of the density matrix in the tensor-network methods and provides a measure for the corresponding classical simulability."

"The bipartition of MPDO through Schmidt decomposition further defines the entanglement entropy in operator space, which is known as the operator entanglement and characterizes the cost of an MPDO representation, i.e., how many Schmidt values are needed at least for faithfully representing an operator [32–37], hence providing a measure for the classical simulability of open quantum many-body systems via the MPDO tensor-network method."

Here we also followed the suggestion of Referee and changed the "MPO (matrix product operator)" to "MPDO (matrix product density operator)" in the whole manuscript.

**Recommendation**

Accept in alternative Journal (see Report)

We thank again the Referee for the review of our manuscript and the helpful comments/suggestions. Our manuscript has been carefully updated according to the above response. With all of the comments and suggestions being addressed, we wish the Referee would now agree the publication of our manuscript in SciPost Physics.