Dear Editor,

We thank the referees for their careful reading and constructive comments on our manuscript. We agree to transfer the manuscript to *SciPost Physics Core*. In this revision, we have substantially improved the presentation in line with the referees' requests. Below we summarize the main changes and provide a point-by-point response (attached). For convenience, we also include a version in which all changes are highlighted in color [link].

List of main changes

1. **Introduction (Sec. 1).** We added a subsection, *Summary of main results and contributions*, that summarizes our main findings and contributions. We also added a paragraph clarifying the relation of our work to rigorous results on many-body adiabatic state preparation. The literature pointed out by Referee 1 is now cited as Refs. [24,46–51].

2. **Bound from Ref. [37].** As requested by Referee 2, the bound obtained in Ref. [37] now appears as Eq. (7).

3. **Figure 2 revised.** We added a new fourth row that overlays the curves shown in the second and third rows, with a logarithmic vertical axis; the original fourth row is now the fifth. The caption and main text were updated accordingly.

4. **Finite-size scaling made explicit (Sec. 6).** We added an unnumbered subsection titled *Condition for adiabaticity breakdown revisited* to make the finite-size scaling of the driving rate explicit.

5. **Summary and Outlook (Sec. 8).** We slightly revised the second paragraph to address a confusion raised by Referee 2.

6. **Appendix A restored.** We reinstated *Appendix A: Alternative derivation of Eq. (17)*, which appears in our arXiv preprint but was omitted during journal submission. Including this appendix improves self-containment and benefits readers, as the projection-operator technique—central to the present work and not used in Refs. [37,38]—is useful for extending the pure-state case treated here to mixed states.

7. Other changes are minor.

We believe the revised manuscript is ready for publication in *SciPost Physics Core*.

Sincerely,

Reply to Referee 1

Thank you for your careful evaluation and valuable suggestions. We have revised the manuscript accordingly and believe it now meets the criteria for *SciPost Physics Core*. Below we address your specific comments point by point.

1. *Referee 1:* Overall, the manuscript takes a somewhat pedagogical approach, explaining step by step the ingredients needed for their result. I think this approach is fine, but it was difficult to me to see what in the end the precise statement is that the authors want to make. In my personal opinion, technical papers like the present one could be improved by having a "Summary and main result" Section II, in which the main assumptions are outlined and the main result is presented.

   Authors' response: We appreciate the suggestion. Rather than creating a standalone Section II, we have added to the Introduction a new subsection, "Summary of main results and contributions," which states our assumptions and concisely summarizes the main results and their significance.

2. *Referee 1:* In a sense, the result is surprising: if the adiabatical fidelity and the overlaps of the instantaneous ground states are similar, then either (i) both are large, and nothing has happened (because the ground state is still essentially the same as at t=0), or (ii) both are small (the case the authors are interested in, in which case adiabatic preparation has failed, because the adiabatic fidelity is zero. Neither regime really can be called adiabatic state preparation. If adiabatic state preparation succeeds, then adiabatic fidelity should be large, and the overlap with the initial ground state small. The difference seems to be that the authors consider a linear ramp (Eq. (7)) with a constant driving rate. Typically, when adiabatic preparation is studied, the interpolation between initial and final Hamiltonian is smooth (e.g. Gevrey class or approximations thereof) and the ramp speed is scaled down with system size, to ensure high adiabatic fidelity.

   Authors' response: We have indeed mentioned in Sec. 6 (above Eq. (42)) that "in order to avoid adiabaticity breakdown, the driving rate Γ of driven many-body systems must scale down with increasing system size N." Our use of a linear ramp with a constant driving rate is for simplicity in our numerical illustrations; the formalism accommodates smooth schedules (including Gevrey-class ones) and N-dependent driving rates $\Gamma(\lambda)$. Our results are therefore consistent with existing rigorous results on many-body adiabatic state preparation.

3. *Referee 1:* There is a large body of mathematical results many-body adiabatic state preparation that the authors seem to disregard. It would be useful to understand their result in the light of what is already rigorously known about adiabatic state preparation:

   [1] Sabine Jansen, Mary-Beth Ruskai, and Ruedi Seiler. "Bounds for the Adiabatic Approximation with Applications to Quantum Computation". In: J. Math. Phys. 48.10 (Mar. 2007), p. 102111. arXiv:0603175[quant-ph].

[2] G. Nenciu. "Linear Adiabatic Theory. Exponential Estimates". In: Commun. Math. Phys. 152.3 (1993), pp. 479–496.

[3] George A. Hagedorn and Alain Joye. "Elementary Exponential Error Estimates for the Adiabatic Approximation". In: Journal of Mathematical Analysis and Applications 267.1 (Mar. 2002), pp. 235–246.

[4] Yimin Ge, András Molnár, and J. Ignacio Cirac. "Rapid Adiabatic Preparation of Injective Projected Entangled Pair States and Gibbs States". In: Phys. Rev. Lett. 116.8 (Feb. 2016). arXiv: 1508.00570.

[5] Sven Bachmann, Wojciech De Roeck, and Martin Fraas. "Adiabatic Theorem for Quantum Spin Systems". In: Physical Review Letters 119.6 (Aug. 11, 2017), p. 060201. arXiv: 1612.01505.

[6] Sven Bachmann, Wojciech De Roeck, and Martin Fraas. "The Adiabatic Theorem and Linear Response Theory for Extended Quantum Systems". In: Communications in Mathematical Physics 361.3 (Aug.2018), pp. 997–1027. arXiv: 1705.02838.

[7] Sven Bachmann, Wojciech De Roeck, and Martin Fraas. The Adiabatic Theorem in a Quantum Many-Body Setting. Mar. 18, 2019. arXiv: 1808.09985.

Authors' response: We thank the referee for pointing out the mathematical literature, some of which we were already aware of. In particular, [1] is cited as Ref. [24] in our manuscript, and [5] is cited within Ref. [37] (as its Ref. [17]). Our aim is not to establish adiabatic *success* under asymptotically slow, smooth ramps, as in mathematical treatments of many-body adiabatic theorems. Rather, our goal is complementary: we provide a mechanism-level explanation and *computable bounds* for the observed closeness between adiabatic fidelity and ground-state overlap in finite (and moderately large) many-body systems under simple, experimentally common protocols. In this context, mathematical results on adiabatic state preparation typically give *sufficient* conditions, whereas our bottom-up, diagnostic approach yields *necessary* criteria. We have added a paragraph titled "Relation to mathematical results on many-body adiabatic state preparation" to the Introduction and cited [1]–[7] accordingly.

4. *Referee 1:* I did not spot major omissions in the manuscript, but I am sceptical that the problem studied is really a "long-standing research stumbling block", because the studied regime is not so relevant for adiabatic state preparation and I am thus not convinced by the motivation underlying the research. As a result, I recommend publication in a more specialized venue, for example SciPost Core.

   Authors' response: We appreciate the assessment and agree to transfer the manuscript to *SciPost Physics Core*. We have revised the presentation to clarify the motivation, highlight the conceptual contribution and technical advances, and make the paper accessible to a broad audience (see the revised Introduction).

Reply to Referee 2

Thank you for your careful evaluation and valuable suggestions. We have revised the manuscript accordingly and believe it now meets the criteria for *SciPost Physics Core*. Below we address your specific comments point by point.

1. *Referee 2:* In my opinion, while this paper has some new results, it is not suitable for publication in SciPost Physics. The main reason for this evaluation is that the obtained results are not significant enough to warrant SciPost. In my view, this manuscript mainly tries to resolve a question raised in Ref. [37], which is considered to treat a rather specific problem.

   Authors' response: We agree that our study was motivated by a specific question raised in Ref. [37]. However, the scope is broader: we obtain model-independent results—(i) a perturbative bound on $|\mathcal{F}-\mathcal{C}|$ at small λ; and (ii) a large-λ mechanism based on almost-orthogonality in the complementary subspace. While (ii) has not yet been established rigorously in full generality, our numerical simulations (including a model different from the one in Ref. [37]) consistently support the almost-orthogonality assumption. We therefore believe these results are of broad methodological interest.

2. *Referee 2:* I also find it difficult to see what is the most important result in this manuscript in the current presentation.

   Authors' response: We thank the referee for the feedback. We have revised the Introduction to include a new subsection, "Summary of main results and contributions," which presents our main results and their significance.

3. *Referee 2:* In conclusion, I think that this manuscript is not suited for SciPost Physics. After all of the questions and comments given below are sufficiently addressed, I would recommend the manuscript for publication in SciPost Physics Core.

   Authors' response: We appreciate the assessment and agree to transfer the manuscript to *SciPost Physics Core*.

4. *Referee 2:* *I think that the inequality to bound $|\mathcal{F}-\mathcal{C}|$ with θ, obtained in [37], should be explicitly written down around Eq. (6).

   Authors' response: Implemented. The inequality from Ref. [37] that bounds $|\mathcal{F}-\mathcal{C}|$ in terms of θ is now written explicitly as Eq. (7), immediately after Eq. (6).

5. *Referee 2:* *After Eq. (18), the authors state that it remains unclear why the values of the adiabatic fidelity and the ground state overlap are nearly identical when the system size N is sufficiently large (e.g., $N \geq 100$). However, in the conclusion, they state that "we demonstrated that these refined estimates perform well even for system sizes as small as $N = O(10^2), \cdots$. These results distinctly outperform the previous estimates from Refs. [37,38], which are reliable only for system sizes no smaller than $N = O(10^3)$." I think these statements are inconsistent.

   Authors' response: We thank the referee for pointing out the potential ambiguity. The two passages address distinct issues. The issue stated in the paragraph after

Eq. (18) (now Eq. (19)) is "why the values of the adiabatic fidelity $\mathcal{F}$ and the ground-state overlap $\mathcal{C}$ are nearly identical when the system size N is sufficiently large (e.g., $N \geq 100$)." By contrast, the quoted sentences from the Conclusion refer to the performance of the inequalities $|\mathcal{F} - \mathcal{C}| \leq \cdots$ used to estimate $\mathcal{F}$ from the ground-state overlap $\mathcal{C}$ and the quantum speed-limit inequality. Here, the previous estimates from Refs. [37,38] only work well for system sizes no smaller than $N = O(10^3)$, while our refined estimates presented in Secs. 5 and 6 perform well even for system sizes as small as $N = O(10^2)$. To avoid confusion, we have clarified the wording in the Conclusion.

6. *Referee 2:* *The place of Fig. 2 could be the same as where it is referred to in the main text.

   Authors' response: Implemented. We now place Fig. 2 at its first mention in the main text. Note that figure float placement may vary slightly across compilers.

7. *Referee 2:* *In Fig. 2, I cannot see how the bound (e.g., (29)) is good for a large λ region because both sides are almost zero. I think one could consider a semi-log plot to see whether (29) really offers a good bound.

   Authors' response: We thank the referee for the helpful suggestion. In the revised Fig. 2, we added a fourth row: a semi-log (log-scale y-axis) plot that overlays the curves shown in the second and third rows. This panel shows that the bound (now Eq. (30)) performs well in the large-λ regime.

8. *Referee 2:* *In the first sentence in Sec. 5, what is the prime in $\mathcal{D}_{\mathrm{un}}$?

   Authors' response: This was a typographical error. We have corrected it by removing the prime and standardizing the notation to $\mathcal{D}_{\mathrm{un}}$ throughout the manuscript.

9. *Referee 2:* *There is an extra space at the end of the sentence after Eq. (32c).

   Authors' response: We have corrected this typographical error by removing the stray space after Eq. (32c) (now Eq. (33c)).

10. *Referee 2:* *Could you elaborate on the relations about $\sqrt{\mathcal{D}_{\mathrm{un}}}$ in the sentence after Eq. (33)? I was not able to follow the discussion at first sight.

    Authors' response: In Eq. (33) (Eq. (32) in the previous version), since both $\sin\theta$ and $\cos\theta$ remain $O(1)$ as $N \to \infty$, the large-N behavior of $\sqrt{\mathcal{D}_{\mathrm{un}}}$ (33a) and $\sqrt{\mathcal{D}}$ (33b) is controlled by $\sqrt{\mathcal{C}(\lambda)}\,\alpha(\lambda)$. Furthermore, according to Eq. (34), $\alpha(\lambda)$ grows no faster than $\sqrt{N}$ as $N \to \infty$. We thus have the asymptotic behavior

    $$\sqrt{\mathcal{D}_{\mathrm{un}}} \sim \sqrt{\mathcal{D}} \sim \sqrt{\mathcal{C}(\lambda)}\,\sqrt{N},$$

    which vanishes as $N \to \infty$ (for fixed λ) because $\mathcal{C}(\lambda)$ decays exponentially with N.

11. *Referee 2:* *I am confused about how the results obtained by the authors are better than the previous ones. In Sec. 5, the authors present the bounds based on g (Eq. (35)) and f (Eq. (37)), which they say come from Eqs. (16) and (18), respectively.

While Eq. (16) is the previously known bound, Eq. (35) seems to be better than Eq. (37), questioning the advantage of the new inequality (18). Am I misunderstanding something?

Authors' response: We thank the referee for raising this point. Let us first clarify the relation between Eq. (17) and Eq. (19) (Eq. (16) and (18) in the previous version). While Eq. (17) is the previously known *general* bound, it is of no use if the value of $\mathcal{D}_{\rm un}$ is unknown. The main useful inequality employed in previous work is Eq. (19), obtained by applying the trivial bound $\mathcal{D} \leq 1$ to Eq. (17) (recall that $\mathcal{D}_{\rm un}$ and $\mathcal{D}$ are related via Eq. (16)). Thus, inequality (19) is not new; it is a special case of inequality (17).

To clarify:

(i) *Eq. (36) (the $g(\lambda)$ envelope)* follows from the general inequality (17) together with available control on $\mathcal{D}(\lambda)$ (see Eq. (33)).

(ii) *Eq. (38) (the $f(\lambda)$ envelope)* is a coarser corollary obtained when one *discards* information about $\mathcal{D}(\lambda)$—e.g., using $\sqrt{\mathcal{D}} \leq 1$—applied to the baseline bound (17), yielding Eq. (19). This elimination makes $f(\lambda)$ looser than $g(\lambda)$.

Hence, the fact that $g(\lambda)$ is tighter than $f(\lambda)$ is precisely the advantage of the general inequality (17) whenever any control over $\mathcal{D}(\lambda)$ is available.