Reply to the comments by Referee #2

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Thank you very much for reviewing our manuscript in detail and giving us very useful feedback. We needed some time to thoroughly consider your comments before responding. Below, we have listed your questions and comments (*italicized*), followed by our responses and proposed revisions to the manuscript (shown in violet). We believe that these revisions will enhance the quality and clarity of our work.

The three models (SO, VCV18, G24-3) are well-chosen to represent distinct mechanisms, but their simplicity raises questions about whether the results generalize to more complex systems. For instance, how would the timescale-matching hypothesis hold in models incorporating additional feedbacks (e.g., carbon cycle, dust-albedo interactions)? A discussion on this limitation would be valuable.

Indeed, our numerical investigations and survey in Table 1 focus on simple models. This is a limitation of the present work. Nevertheless, as mentioned below, some studies in the literature offer insights on how our timescale-matching hypothesis may hold in more complex models.

First, an early study by Oerlemans (1982) demonstrated that an ice-sheet–bedrock system could exhibit 100-kyr-scale self-sustained oscillations especially due to strong feedbacks involving basal melting and sliding of the ice sheets. This model is an instance that the 100-kyr-scale intrinsic oscillations are relevant for producing 100-kyr cycles under insolation forcing, even though our knowledge of lithosphere physics has since been revised.

Second, since the G24-3 model was, according to its author, inspired by experiments using the Earth system model of intermediate complexity, CLIMBER-2 model (Ganopolski, 2024). If we follow this argument, our results obtained from the G24-3 model can be relevant with complex climate systems including carbon cycles and dust–albedo interactions.

Third, and perhaps more importantly, Mitsui et al. (2023) showed that a version of the CLIMBER-2 model exhibits self-sustained oscillations with periodicities of several hundred thousand years, due to the glaciogenic dust feedback and carbon cycle feedbacks. Such long timescales are crucial for ∼100-kyr ice age cycles simulated in the CLIMBER-2 model under the forcing.

These previous findings support the timescale-matching hypothesis proposed in this study. **In the Discussion section of the revised manuscript, we will address the limitation and the above supports from complex models.**

The definition of "intrinsic timescale" varies across models (e.g., self-sustained oscillation period vs. relaxation timescales in bistable systems). The manuscript should clarify whether these differences affect the interpretation of timescale matching or if they represent fundamentally distinct dynamics.

Yes, the differences in model dynamics do affect the interpretation of timescale matching:
If the underlying model exhibits self-sustained oscillations, the period of glacial cycles under forcing tends to be tightly coupled with the period of the internal oscillation, as shown by the nearly vertical quasi-Arnold tongues in Fig. 4a. In such cases, the observed $\sim$100-kyr cycles may suggest the presence of self-sustained oscillations with a period very close to 100 kyr. In contrast, in systems with nonlinear resonance involving damped oscillations, the resonance frequency can deviate from the natural frequency of damped oscillations depending on the amplitude of the forcing. The importance of this deviation has been emphasized in the community comments by Dr. Verbitsky. In the case of the bistable model (G24-3), the intrinsic timescale is not purely internal, but instead includes a $\sim$10-kyr-scale waiting time until favorable astronomical conditions are met.

Considering these differences, we suggest viewing the intrinsic timescale as the time required to form a cycle. Notably, many models that reproduce realistic 100-kyr glacial cycles tend to exhibit such intrinsic timescales close to 100 kyr. These observations support the generality of the timescale-matching hypothesis proposed in this study. **In the the revised manuscript, we clarify the definition of intrinsic timescales across models introducing a schematic figure, and then discuss the different interpretations of timescale matching in each type of model.**

The brief discussion of the MPT (Section 4) is insightful but underdeveloped. The authors suggest that the 41-kyr periodicity before the MPT could also result from timescale matching, but this is not explored in depth. Including a sensitivity analysis or model experiments addressing the MPT would significantly strengthen the paper.

We thank the reviewer for this suggestion. It is indeed possible to extend our results into the pre-MPT period, although we had planned it for future work. **In order to make our results more comprehensive, we will expand the present discussion on the MPT in the revised manuscript. Specifically, we will present sensitivity experiments in the 41-kyr world before the MPT (i.e., a new figure corresponding to Fig. 4).**

The distinction between nonlinear resonance and synchronization is well-explained, but the manuscript could better highlight why this distinction matters for the 100-kyr problem. For example, does the dominance of one mechanism over the other have implications for predicting future climate variability?

Nonlinear resonance and synchronization are distinct concepts in nonlinear science, and using these terms properly is important to avoid confusion in discussions of ice age cycles. However, the present study does not aim to determine which mechanism is most plausible, thus the most adequate for long-term predictions. Our objective was to provide a unified perspective on these mechanisms through the lens of timescale matching.

As you suggest, if one plausible mechanism is ultimately identified, it may have implications for predicting future climate variability. For example, if the Earth system exhibits self-sustained oscillations, it may have a tendency to enter a new ice age spontaneously over the next tens of thousands of years. We keep in mind that in reality, anthropogenic forcing plays a significant role, and any such prediction must clearly be verified using more realistic models.

The power spectral density (PSD) analysis is robust, but the manuscript could include a more detailed comparison between model outputs and proxy records (e.g., time-domain metrics or phase relationships). This would help assess whether the models not only reproduce the 100-kyr peak but also the timing of deglaciations.

Yes, the time-domain metrics or phase relationships are definitely useful for assessing models with respect to the timings of deglaciations. The timings of deglaciations themselves are however complicated metrics because the last 800-kyr contains eleven deglaciations. The most simple time-domain metric would be the Pearson's correlation coefficient (PCC) between model outputs and the proxy record would. The PCC evaluates phase relationships, although it does not focus on deglaciations. **In the revised manuscript, we will include a new supplementary figure demonstrating that timescale matching is also necessary to achieve a high correlation with the data, using the PCC as the metric.** On the other hand, we cannot exclude the possibility that even good models may fail to reproduce the precise timing of deglaciations, particularly if that timing is sensitively dependent on parameters or influenced by stochastic forcings (Crucifix, 2013; Mitsui and Aihara, 2014; Mitsui et al., 2015; Mitsui and Crucifix, 2016). Therefore, caution is warranted when using correlation-based metrics for model comparison.

Figures S1–S7 are cited in the text but are not included in the preprint. The authors should ensure all supplementary figures are accessible or provide descriptions in the main text.

It is unfortunate that you could not access the supplementary figures during the review. Actually they have been provided in the preprint page at: https://esd.copernicus.org/preprints/esd-2024-39/esd-2024-39-supplement.pdf. **In the revised manuscript, we ensure that the manuscript includes the link to the supplementary material.**

Line 25: "Henceforth" should likely be "Previously."

Indeed, "Henceforth" is not suitable in this context. Instead, we find that "hence" provides a clearer logical connection between the sentences. Accordingly, we have rephrased the text as follows: **Hence**, the $\sim$100-kyr glacial cycles have **previously** been explained as ...

Lines 70-75: It only briefly explains each chapter's general content, not the research purpose and main methods, making it hard for readers to grasp the research core at the start. Suggest the author supplement research objective and main method info. When explaining objectives, state key scientific problems to solve and expected results. When describing methods, detail model selection criteria, simulation experiment process, and data analysis methods and ideas to help readers understand the paper's core content and research context.

We acknowledge that the research purpose and main methods may not have been clearly conveyed, possibly because they were embedded mid-paragraph (lines 54–60).

In response to your comment, we will revise the final paragraph of the Introduction to clearly state the objectives of the paper, outline the key scientific questions addressed, and describe the main methods, including model selection criteria, simulation procedures, and data analysis approaches.

Line 204: The term "quasi-Arnold tongue" (Section 3.2) is introduced without a clear definition. A brief explanation or reference would aid readability.

The original Arnold tongues are triangular-shaped regions in parameter space where synchronization occurs (Pikovsky et al., 2003). **In this study, we refer to quasi-Arnold tongues as triangular regions in which the principal frequency of a self-sustained oscillator under external forcing matches either one of the forcing frequencies or a linear combination of them.** This concept extends the classical definition of Arnold tongues to cases where exact frequency locking is replaced by more flexible frequency entrainment patterns.

We would like to thank you again for your thoughtful comments and very useful feedback.

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