The preprint titled "100-kyr ice age cycles as a timescale matching problem" presents a compelling hypothesis that the dominant ~100-kyr periodicity of late Pleistocene glacial cycles arises from the proximity of the climate system's intrinsic timescale to the ~100-kyr eccentricity cycles. The study systematically analyzes three distinct ice age models—representing synchronization, resonance in a mono-stable system, and resonance in a multi-stable system—to demonstrate that the ~100-kyr periodicity emerges when the intrinsic timescale of the system aligns with the astronomical forcing. The manuscript is well-structured, clearly written, and addresses a long-standing question in paleoclimatology with a novel perspective. The manuscript makes a significant contribution to understanding the ~100-kyr problem by unifying diverse mechanisms under the timescale-matching hypothesis. With minor revisions—particularly expanding the discussion of the MPT and clarifying the generality of the results—the paper would be suitable for publication. I recommend acceptance after addressing the specific comments above.

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.

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.

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.

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?

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.

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.

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

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.

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