Response to Anonymous Referee #1

Dear Referee,
Thank you for reviewing our manuscript and for your effort and time spent on this. Your constructive feedback will help to improve the final revised version of this manuscript.

Based on this feedback, we will substantially revise the manuscript with the following main changes:

- we will reduce the complexity of the used model to rely on fewer parameters
- The benthic δ^{18}O curve (probstack with removed trend) from Clark et al. (2025) will be used as a new main target. Berends and Rohling SL curves will only be shown in SI
- ORB, ABR and GRAD models will be removed to focus on the novel RAMP model. This will make the manuscript more concise and highlight the novelities compared to the Legrain et al. (2023) model

In the following, we respond to your comments and propose several changes to the manuscript, motivated by your suggestions.

Author comments
Referee comments

This paper improves an exisiting conceptual model on global ice volume changes as function of orbital forcing and some internal feedbacks which is then applied to the last 2.6 Ma in order to understand the Mid-Pleistocene Transition (MPT).
In my view this is a solid piece of work, but misses one important piece of discussion. All sea level (=global ice volume) reconstructions used here as target are some sort of deconvolution of the LR04 benthic d18O stack into sea level and temperature. However, a most recent approach by Clark et al., published some weeks ago in Climate of the Past (doi: 10.5194/cp-21-973-2025) doing the same thing supported by SST and deep ocean data comes to a significantly different deconvolution with larger glacial/interglacial amplitudes in the sea level component throughout the Quaternary than the sea level records used here.
In an ideal world, this new record (data to figure 10b in Clark et al., 2025 available in the SI there) would as an alternative be used here as tuning target, but I understand that this would be a major effort and leave it up to the authors if they want to jump on this challenge. However, I noted that the authors referred to another discussion paper (Scherrenberg et al., 2024), so it is not clear to me why they not also included in their discussion of the MPT the Clark et al paper, whose discussion version was also available since September 2024. What should be and needs to be done is that this difference between the sea level component in the new Clark et al 2025 paper and those used here needs to be discussed. This is in my view important for two reasons.

The other referees agree with the point of not using the Berends sea level reconstruction as the main target for the model. Therefore, we will use the benthic δ^{18}O (Probstack with removed trend) from Clark et al. (2025) as our new main tuning target and for model evaluation in the revised version of this manuscript.

1. The Clark paper is simply one of the newest papers published on MPT sea level changes (although only indirectly contained in the contribution to the benthic d18O stack) and should be considered/discussed in any upcoming paper on the same topic.

We agree. Other conceptual models also used benthic δ^{18}O as model targets, e.g. a normalised or scaled LR04 stack is used in the Leloup and Paillard (2022) and the Ganopolski (2024) MiM models.

2. The regolith hypothesis, formulated some decades ago, also by Clark and others, was trying to explain that there were ice sheets in North America, that were reaching as far south as 39-40°N around 2.5 Ma ago (e.g. Balco & Royery, 2010, doi: 10.1130/G30946.1). However, more complex models are not yet able to simulate these ice sheet extends. The Utrecht model (in the version of de Boer et al 2014, doi:10.1038/ncomms3999) was failing to do so, as has been shown in Köhler & van de Wal 2020 (doi: 10.1038/s41467-020-18897-5), which plotted in their Fig. 2b the latitudinal ice sheet extend of the Utrecht model as function of time.

(Side note: Köhler & van de Wal 2020 is to some extend a reinterpretation of Tzedakis et al 2017 on the appearance of interglacials during the Quaternary and might for that content also be discussed here as one recent study on the understanding of the MPT.)

We agree. Since our model relies on a specific switch to transition between a deglaciation and glaciation state, we propose to add the following paragraph to our discussion:

"The model presented above depends on a specific threshold choice to switch between a glaciation and a deglaciation state. Köhler and van de Wal (2020) challenge this binary view of interglacials and glacials. By classifying interglacials based on the absence of substantial NH land ice outside of Greenland, they found that the classification of interglacials, especially in the early Quaternary, is ambiguous. This classification depends on the defined threshold and the choice of the underlying record. This perspective questions the ability of any threshold-based, two-state model to unambiguously classify interglacial states. Therefore, the identified glacial and deglacial states in our model should only be carefully considered in combination with the applied thresholds, defined in Eq. xy, and for the used target record (benthic δ^{18}O, sea-water δ^{18}O or global ice volume)."

The sea level of Berends et al (2021) used here is a follow-up study of the de Boer et al 2014, both using a model to deconvolve benthic d18O into sea level and temperature, thus both results differ in detail, but rely on the same approach and give in principle comparable results. Also the 3 Ma long simulations of the CLIMBER model (Willeit et al., 2019, doi: 10.1126/sciadv.aav7337), which is a more complex climate model than in the Utrecht approach, and which does not use benthic d18O as input, do not get large ice sheets down to 40°N at 2.5 Ma BP.

That said, these interpretations of the benthic d18O stack are still failing to explain the terrestrial evidence of Balco and others. These shortcomings are also worth mentioning in the discussion.

We agree. The Berends and Rohling sea-level curves will no longer be the main target of our model, but instead be shown in the supplementary material. To discuss the limitations of these model-based sea level reconstructions, we propose to add the following paragraph to our discussion:

"Available global mean sea level data exhibit large uncertainties. Between 50 and 30 ka, geological and geochemical reconstructions of GMSL vary up to 60 m and above (Farmer et al., 2023). Model-based deconvolutions of global δ^{18}O into GMSL, like for the Berends et al. (2021) and Rohling et al. (2022) sea level reconstructions, exhibit similar large uncertainties. While the Berends curve shows an LGM lowstand of around 100 m, the Rohling curve gives around 108 m. Both values are well below observational-based reconstructions of around 130 - 135 m for the LGM (Austermann et al., 2013; Lambeck et al., 2014; Yokoyama et al., 2000). These large uncertainties limit the usability of sea level reconstructions as targets for conceptual models, as presented in this study."

With this two contrasting deconvolutions of the benthic d18O stack - one based on ice sheet model, one based on an ocean temperature data compilation - such a simple model as used here might be able to give ideas how to understand them. Eg if completely different conceptual models are necessary to satisfy both data sets, but again, maybe this is a task for a future study, especially since a final interpretation of the sea level related changes in benthic d18O from Clark et al is still not published. Nevertheless, I would say it is a missed opportunity if the authors decide not jump on this issue here and now.

We agree. In the revised version of this manuscript, we will use the benthic δ^{18}O record from Clark et al. (2025) as our new main tuning target (Fig. 1b). Our reduced RAMP model yields similar results for this target, i.e. a long-lasting trend for the ramp, which started over 2 Ma. While the model performs well on this target, it has more difficulties with the seawater δ^{18}O record from Clark et al. (2025) (Fig. 1c), as can be seen by comparing the R values. In general, the model cannot accurately reproduce the larger pre-MPT amplitudes of the glacial-interglacial cycles and the decreasing trend of $\delta^{18}O_{sw}$ during the MPT. The RAMP formulation seems to be less suited for this target, as can be seen by the identified ramp period, which lies now in the interval $\sim$ 400 - 200 ka. For the $\delta^{18}O_{sw}$ target, the model also cannot reproduce the shift towards the 100-kyr periodicity correctly, since it only shows a 100-kyr signal for the last 200 ka.

Minor issues:

- Barker et al. 2025, explaining the role of orbital parameters for the 100k-world after the MPT also gives a glimpse on their roles in the 41k-world (their SI Figure 8). Thus, I believe this paper is one of the most recent studies discussing underlying processes of the MPT and should already be discussed widely in the introduction.

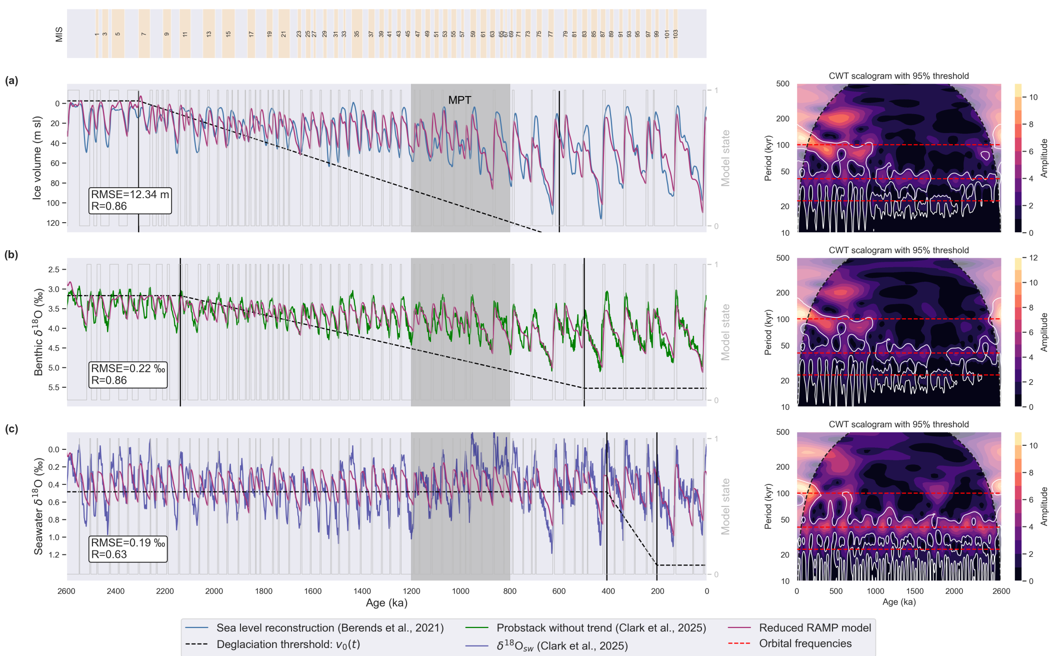

Figure 1: Reduced RAMP model optimised for three different tuning targets: Berends SL curve (a), Clark et al. (2025) benthic $\delta^{18}O$ (b) and Clark et al. (2025) seawater $\delta^{18}O$ (c).

We agree. L. 651 f. will be shifted to the introduction and extended in the following way to also mention the 41-kyr world:

Recent studies suggest that the duration and timing of deglaciation and glaciation events over the last 900 ka were largely deterministic and driven by the relative phasing of precession, obliquity, and eccentricity (Barker et al., 2022, 2025). Barker et al. identified candidate precession peaks, which are precession peaks that begin while obliquity is increasing, as essential for glacial terminations. They found that all glacial terminations during the last 900 ka correspond to the first candidate precession peak after a minimum in eccentricity. This is how the 100 kyr periodicity comes into play for the post-MPT world. On the other hand, it seems like obliquity alone controls the following glacial inception, which is triggered by the start of its next decreasing phase. During and prior to the MPT, the authors found that almost all candidate precession peaks are linked to glacial terminations. Since they depend on rising obliquity values, they are mainly paced by obliquity, resulting in the observed 41-kyr world with no more influence of eccentricity on these cycles.

- Please update the reference to Scherrenberg et al., 2024 to the now available final version: https://doi.org/10.5194/cp-21-1061-2025).

Corrected.

- Figure 4: purple and red dots look the same. Make one symbol differently, eg squares.

Corrected.

- lines 520ff: Somehow now „glacial inception“ is shortened to „gl. inception“. Please use full words.

Corrected.

- line 614: The most recent review on MPT SST changes is in Clark et al. 2024 (doi:10.1126/science.adi1908). please revise your discussion here based on that paper, instead of using the older study by McCymont et al 2013.

We agree. We propose to replace the sentence starting in L. 613 with the following:

"A synthesis of globally distributed SST records by Clark et al. (2024) found that two long-term cooling stages occurred during the last 4.5 Ma. The first started around 4 Ma, which was followed by a second period of intensified cooling between 1.5 Ma and around 0.8 Ma. Thereafter, temperatures stabilised for the late Pleistocene."

- line 618: δ^{13}C instead of δ_{13}C

Corrected.

- Section 4.5. The starting sentence should already say that such a future exercise would be one which would ignore anthropogenic impacts.

We agree. We propose to shift the sentence in L. 680 to become the second sentence in this paragraph. The new paragraph would start as following:

"Another feature of the conceptual models presented in this study is their ability to extrapolate future glacial-interglacial cycles, in the absence of any anthropogenic impacts. Since these models do not account for CO_2, and thus do not include anthropogenic climate change, these results can only be considered as baseline experiments of how the glacial cycles would evolve without the anthropogenic effect of rising atmospheric CO_2."

- line 688:"... would reach a maximum rate". A rate of what? Glacial inception can hardly be a rate.

Instead of defining sharp transitions between interglacials and glacials based on thresholds in sea level or δ^{18}O, Barker et al. (2025) refer in their work to the maximum rate of δ^{18}O increase, denoted as the maximum in glacial inception, which returns the Climate system to the next glacial cycle. To avoid a misleading interpretation of this sentence, we propose to replace it with the following sentence:

"Excluding anthropogenic CO_2 emissions and extrapolating their analysis of candidate precession peaks, Barker et al. (2025) found that the next glaciation would reach a maximum rate, i.e. the change in $\delta^{18}O$, during the next 11kyr and should be terminated around 66 kyr in the future."

- The content of the Appendices (Tables A1-A2, Figures B1-B6) are in my view actually Supplementary Figures which should appear in an SI (in the final format an extra PDF) and not as Appendices (added extra figures in the main PDF).

We agree. This will help to make the manuscript more concise.

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