General comment

Dear editor, dear reviewers,

Thank you for reviewing our manuscript and for your effort and time spent on this. Your constructive feedback helped to improve the revised version of this manuscript. We have made major revisions to our manuscript in order to address the concerns of the reviewers.

Based on this feedback, we implemented the following main modifications:

- We focused on reconstructing global ice volume alone and dropped benthic $\delta^{18}O$ as a target

- We only use the Berends and Rohling GMSL reconstruction as targets for the RAMP model

- We show that a slightly modified model (RAMP-2) can reconstruct the Berends and the new Clark GMSL curves. We add this modified RAMP model to the SI

- We changed t_1 and t_2 to physical units (kyr), s.t. they have negative values and the time is consistently in physical units throughout the model

- We rewrote the discussion section about the model extrapolation into the future

- We added a discussion of the new Clark GMSL into the introduction and discussion

In the following, we respond point-to-point to the comments of the editor and the reviewers and state the changes made in the revised version of this manuscript.

Response to the editor Lorraine Lisiecki

Editor comments

I thank the authors for their revisions and invite them to submit further revisions based on reviewer comments. The authors chose to address previous comments about uncertainty in the ice volume target by tuning their model to four different targets. However, both reviewers and the editor have concerns about how these additional records were incorporated into the revised manuscript. First, I want to emphasize that it is not necessary that this manuscript include application of the model to all of these targets. It would be sufficient for the authors to simply discuss that ice volume reconstructions are uncertain and some of the reasons why – for example, due to assumptions about the temperature component of benthic d18O. Three of the targets are very similar because they estimate that temperature and ice volume change approximately proportionately through time (thus, correlating strongly with benthic d18O). Because these 3 targets are very similar, it is unsurprising (although reassuring) that the model reaches similar findings for all 3. In contrast, the d18Osw reconstruction of Clark (and the recently published sea level curve based on it) uses a different approach to estimate temperature and generates a dramatically different estimate. The current version of the manuscript doesn't sufficiently explain why 4 different tuning targets are used and why this model would even be applicable to simulating detrended benthic d18O since it is designed to simulate ice volume. At a minimum, I would recommend removing the optimization to benthic d18O.

Accepted. We removed benthic $\delta^{18}O$ as a target from the RAMP model and only focus on reconstructing global ice volume (namely the Berends and the Rohling GMSL curves) with this model. The results presented in Figures 1, 2 and 3 remain the same for the RAMP model, when tuned for the Berends and Rohling GMSL curves, but the other two targets (Prob-stack and Clark $\delta^{18}O_{sw}$) were removed from the plots and in the text. The model sensitivity presented in section 3.3 is tested for the RAMP model, when tuned to the Berends GMSL, no longer for Prob-stack.

The next revision of this manuscript should carefully incorporate responses to all of the reviewers' critiques, those discussed above as well as other concerns including the model's variables for time and the reconstruction/forecast of the Holocene and next glacial maximum (if this remains in the revision).

The variables t_1 and t_2 have been changed into physical units (kyr), s.t. they are negative in the model. The model now uses consistently physical units. Equation (8) has been accordingly modified in the methods part:

$$v_0(t) = \begin{cases} v_{0,1}, & \text{if } t < t_1 \\ v_{0,1} + \frac{v_{0,2}-v_{0,1}}{t_2-t_1}(t-t_1), & \text{if } t_1 \leq t \leq t_2 \\ v_{0,2}, & \text{if } t_2 < t \end{cases} \tag{1}$$

The section about the next glacial cycle has been rewritten (see response to Ganopolski's comment below).

I'd also like to see a different title for the next revision: The model doesn't actually "reveal" a ramp-like change as the "cause" of the MPT because its only one possible conceptual model, it doesn't explore all alternate hypotheses, and there's uncertainty about the appropriate ice volume signal that should be reproduced. Thank you in advance for your attention to these details.

Accepted. We propose the following new title:
"Simulating global ice volume across the Mid-Pleistocene Transition with a ramp-like increase in the deglaciation threshold".

Response to Anonymous Referee #1

I focus in this 2nd round of reviews mainly on the aspect if the applied changes have improved the draft.

Having that in mind I have to say that in my view the draft has not improved, when it comes to also discussing what we know about the MPT from the new data on temperature, d18O_sw, and sea level from recent papers of Clark et al. (2024, 2025). Unfortunately, there was also bad luck with the timing of the submission, since on 16th October 2025 a new 4.5 Ma long record of global mean sea level change has been published by Clark et al in Science, DOI: 10.1126/science.adv8389, which were not yet considered here. This new paper of Clark et al. takes into account how temperature might influence d18O of land ice, and therefore how sea level was calculated in detail out of the previously published d18O_sw.

In the revised manuscript, we focus on global ice volume/GMSL reconstruction and no longer on benthic $\delta^{18}O$ or $\delta^{18}O_{sw}$. Hence, we removed Prob-stack and the $\delta^{18}O_{sw}$ record of Clark et al. (2025b) from the manuscript. We set up a slightly modified version of the RAMP model (RAMP-2 model, discussed below), which can simultaneously reconstruct the Berends and the new Clark GMSL curves. RAMP-2 is added into the SI (SI Sec. 7).

My main critics, the shortcoming of Pollak et al. so to say, is that the authors indeed took up the challenge to also test their approach against the new d18O_sw aka sea level of Clark et al (thanks for jumping on the challenge!), which is fundamentally different from all other sea level reconstructions, since the MPT now only contains a change in frequency (41 ka to 100 ka), but not anymore an increase in the sea level (or ice volume) amplitude. Thus, we has to speak about a new paradigm of how to understand the MPT. However, the authors in their revised draft do not even mention in the introduction that there are now two fundamentally different interpretations of the MPT out there (not citing Clark et al. 2025 at all in the introduction). The new interpretation of Clark - although not completely understood (e.g. no model explained the new reconstructions) - is for the first time able to explain paleo data, that suggest a large Laurentide ice sheet existed 2.5 Ma, something which all other approaches fail to explain. I mentioned this deficit already in round one of the reviews, but if I am not wrong, the authors did not include it in their revision.

Accepted. We added the following paragraph into the introduction (new L.57ff.):

"Moreover, the effects of the MPT on the evolution of global ice volume across the Quaternary remain incompletely understood, as there are currently two contrasting views. The reconstruction of global ice volume is based on benthic foraminifera $\delta^{18}O_b$, measured in marine records. It consists of two components, a temperature $\delta^{18}O_T$ and a seawater $\delta^{18}O_{sw}$ component, with the latter serving as a proxy for global ice volume. One common approach to disentangle these two components from the $\delta^{18}O_b$ signal is to apply some kind of regression method. This approach largely reproduces the variability present in the $\delta^{18}O_b$ signal, result-

ing in small ice sheets in the early Pleistocene, which increase in volume after the MPT. In contrast, a recent study by Clark et al. (2025b) deconvolved $\delta^{18}O_b$ into its seawater $\delta^{18}O_{sw}$ component, based on ocean temperature data, and subsequently Clark et al. (2025a) applied a mass-balance approach to this $\delta^{18}O_{sw}$ record, considering time-varying temperature and ice volume effects on the $\delta^{18}O$ of ice sheets, to reconstruct the Global Mean Sea Level (GMSL) over the past 4.5 Ma. This reconstruction no longer shows an increase in global ice volume over the Pleistocene, but just a transition in glacial frequency. Furthermore, this curve exhibits large ice sheets around 2.5 Ma, which is in better agreement with terrestrial (Balco and Rovey, 2010) and marine (Shakun et al., 2016; Rea et al., 2018) evidence. So at present, there are two fundamentally distinct perspectives on how global ice volume varied throughout the Pleistocene and on how the MPT affected the global ice volume variability."

And we added the following paragraph into the discussion:

"Our findings are constrained not only by the uncertainties in the Berends and Rohling GMSL reconstructions but also by the specific choice of which GMSL curves to use as the target for our model, given that there are currently two contrasting interpretations of how global ice volume evolved throughout the Pleistocene. One common approach to disentangle the effects of $\delta^{18}O_T$ and $\delta^{18}O_{sw}$ from benthic foraminifera $\delta^{18}O_b$ is to apply some kind of regression method on $\delta^{18}O_b$ to reconstruct the global ice volume (Berends et al., 2021; Rohling et al., 2022). This approach reproduces the variability of the underlying $\delta^{18}O_b$ record. In the case of the global $\delta^{18}O_b$ stacks as LR04 (Lisiecki and Raymo, 2005) or Prob-stack (Ahn et al., 2017), this method leads to an increase in global ice volume over the Pleistocene in addition to the shift in frequency. Hence, GMSL reconstructions (and hence global ice volume), as the ones of Berends et al. (2021) or Rohling et al. (2022), exhibit small global ice volume during the early Pleistocene. This is in contrast with evidence for large NH ice sheet extents already in the early Pleistocene. For instance, Balco and Rovey (2010) found terrestrial evidence of the Laurentide Ice Sheet (LIS) reaching 39° N at 2.4 Ma and 1.3 Ma, Shakun et al. (2016) found similarly large extents of the LIS to mid-latitudes between 2.55 - 1.7 Ma by using a marine $\delta^{18}O_{sw}$ record and Rea et al. (2018) used a multi proxy data set to infer the existence of extended ice sheets grounded within the North Sea, south of ∼60° N from 2.53 Ma. Hence, the Berends and Rohling GMSL reconstructions, as well as similar regression-based reconstructions, do not capture these large ice sheets around ∼2.5 Ma. An alternative approach is presented in a most recent work by Clark et al. (2025b), where the authors deconvolved the Prob-stack (Ahn et al., 2017) into its seawater $\delta^{18}O_{sw}$ component, based on ocean temperature data. The Prob-stack was corrected for a long-term increase in $\delta^{18}O_b$, possibly due to a combined effect of diagenesis and the carbonate ion effect. This new $\delta^{18}O_{sw}$ record was then subsequently used in Clark et al. (2025a) to reconstruct the GMSL over the past 4.5 Ma by applying a mass-balance approach, considering time-varying temperature and ice volume effects on the $\delta^{18}O$ of ice sheets. They find a sea level lowstand of -151±25 m at 2.52 Ma, which is comparable in size to the LGM lowstand, which aligns with terrestrial and marine evidence for large NH ice sheets during this period, as mentioned above. The extended ice sheets prevail in their reconstruction throughout the entire Pleistocene. Therefore, the new Clark GMSL curve differs fundamentally from former GMSL reconstructions by no longer exhibiting an increase in ice volume over the Pleistocene, but just a shift in glacial frequency. This leaves us with two contrasting views on the evolu-

tion of the global ice volume over the MPT. In this study, we selected the Berends and Rohling GMSL curves as target curves instead of the newly published Clark GMSL. This has the following reason: the RAMP model is based on former models (P98, L23), which were specifically designed to simulate an increase in ice volume through an increasing v_0 parameter, hence, the RAMP model is adapted to reconstruct GMSL curves which do show an increasing trend in global ice volume. So, by its model design, the RAMP model is not suited for a GMSL curve as the one presented by Clark et al. (2025a), since this data set no longer exhibits an increase in ice volume. However, we tested various approaches to adjust the RAMP model to simultaneously reconstruct the structurally different Berends and Clark GMSL curves. It seems that an adjustment of the glaciation threshold in combination with a ramp-like change in these threshold parameters is needed. We present this adjusted model, which we call RAMP-2, in the SI (SI Sec. 7). However, further tests are needed to verify the performance of the RAMP-2 model. Thus, leaving us with the RAMP model, applied to the Berends and Rohling GMSL reconstructions. Since the RAMP model is only applicable to a subset of the available GMSL curves, namely the ones which do show an increase in global ice volume, our finding that a long-lasting trend in the deglaciation parameter is key in reproducing the MPT is limited to these types of GMSL curves and this specific interpretation of the MPT."

Finally, my suggestion to also test models against the new Clark et al data was based on the variety of model which had been applied in version 1 (4 models). My idea was, that testing different models against structural completely different data would give the authors arguments which model works best for which data set. Unfortunately, in the revision the authors have chosen to restrict their zoo of models to one - without any support from data for this choice. This is somehow making a conclusion before the analysis was performed. Thus, they choose from the beginning how the model should perform and with no surprise the new data disagree most with the chosen RAMP model. In doing so, this whole excersice of including the Clark data in the test is more or less meaningless.

The RAMP model already incorporates the temporal scenarios of the GRAD, ABR and ORB models. Therefore, the failure of the RAMP model in reconstructing the $\delta^{18}O_{sw}$ implies that the other models would equally fail in doing so. However, all these models rely on the same mechanism, namely an increase (or decrease) in the deglaciation parameter $v_0(t)$, which was implemented in order to simulate an increase in global ice volume. Since the new Clark GMSL curve no longer shows an increase in ice volume, another model formulation would be required. Hence, we removed the $\delta^{18}O_{sw}$ as a target for the RAMP model. However, we tested various parameterizations and found that if the glaciation threshold of the RAMP model is slightly altered, this new model (which we call RAMP-2) is capable of reconstructing the Clark GMSL curve, especially it is capable of simulating the shift in frequency without an increase in amplitude. We added the method description of the RAMP-2 model and its results for the Berends and Clark GMSL into the SI (SI Sec. 7).

So how to continue? If the authors insist on testing only one model (RAMP), the test to the Clark data should be deleted again for the reasons given above. If so, they nevertheless need to explain in intro and discussion, that they choose to sample only a subset of available data. Whatever they decide, they need to state on the existence of the Clark et al papers

and that they are superior in explaining certain paleodata (large ice sheets at 2.5 Ma), which all approaches looked into here were not able to tackle. Check the new Clark et al paper on sea level in Science on more details of independent support for such large ice sheets. Alternatively, they revise the revision to again apply several models from which we then see which approach is explaining best which data set.

Corrected as discussed above.

Minors:
- page 4, line 117: „In the RAMP model, we only use a linear combination of precession and obliquity." This is not correct, Equation 1 contains also eccentricity (e).

Corrected, we meant precession **parameter**. L. 117 is now changed to:
"In the RAMP model, we only use a linear combination of the precession parameter and obliquity."

- page 5, line 124ff: I(t) has as units m/kyr. Therefore, I(t) cannot be an „orbital forcing" (which has as units W/m^2), it is more „a system response in terms of sea level change per time to the orbital forcing". Please revise throughout the draft.

We agree, a more precise description of $I(t)$ would be something like: "I(t), the global ice volume change per unit time in response to the orbital forcing ". However, this is quite a long expression to repeat throughout the manuscript, therefore, we suggest changing 124ff. In the following way:

"Hence, the global ice volume change per unit time in response to the orbital forcing $I(t)$ (m kyr^{-1}) is defined in the model as:

$$I(t) = \alpha_{\mathrm{Esi}}\mathrm{Esi}(t) + \alpha_{\mathrm{O}}\mathrm{Ob}(t), \tag{2}$$

with the constant weights α_{Esi} and α_{O} (m kyr^{-1}). For brevity, we will refer to $I(t)$ simply as the orbital forcing from here on."

Response to the review of Andrey Ganopolski (Referee #2)

Author comments
Referee comments

The author significantly revised their manuscript by addressing reviewers' concerns and comments. The paper has clearly benefited from being shortened and the model being simplified. However, some old problems still remain, and some new ones have emerged in the revised manuscript.

General comments

The model of "different paleoclimate records and variables". The Paillard (1998) conceptual model and its derivatives were always considered as the models of Quaternary global ice volume variations. This was also explicitly stated in the title of the first version of the manuscript (*"An improved conceptual model of Quaternary global ice volume and the Mid-Pleistocene Transition"*). However, in the revised manuscript, the authors refer to their model as the model of *"different paleoclimate records and variables"* (L.7). This turn is rather surprising. Firstly, there are numerous different paleoclimate records, and it is unclear to which of them the RAMP model can be applied. Secondly, paleoclimate records usually represent a mix of global, regional and local signals with a significant noise component. What is the purpose of modeling such records? The current version of the manuscript presents the results in a way that the RAMP model can simulate 75% (three of four) of "different paleoclimate records" correctly, which does not sound too bad. However, I cannot accept such an interpretation. What the authors are modeling are not "paleoclimate records" but the time series which represent highly-processed paleoclimate information with the explicit use of modeling results. Two of these series (Berends and Rohling) are GMSL reconstructions. The third one (prob-stack) does not have significant added value, as the benthic $\delta^{18}O$ stack was used for producing all GMSL reconstructions anyway. Finally, Clark's $\delta^{18}O_{sw}$ is also not a paleoclimate record, but more importantly, according to Clark's view, it is also not a proxy for GMSL. The authors wrote (L370/1) *"However, a final reconstruction of this $\delta^{18}O_{sw}$ record into the GMSL curve is not yet available"*. Unfortunately, this is not true anymore - this reconstruction was published in Science a month ago. Actually, it was available since P. Clark gave his "Requiem" talk at the EGU in 2021. To compare "apples with apples", the authors should have used Clark's GMSL rather than $\delta^{18}O_{sw}$.

The benthic $\delta^{18}O$ and seawater $\delta^{18}O_{sw}$ targets are again removed from the revised manuscript. Hence, the RAMP model is again only reconstructing global ice volume over the Quaternary. As tuning targets, we use Rohling and Berends GMSL curves. To address the new Clark GMSL reconstruction, we set up a modified version of the RAMP model (RAMP-2). We add the RAMP-2 model into the SI (SI Sec. 7). Furthermore, we no longer use the expression *"paleoclimatic record"*, but instead always refer to GMSL reconstructions or curves.

Clark's reconstruction of GMSL. The use of Clark's recent results was not my idea. Moreover, I do not think it is necessary. However, if the authors want to compare three

different GMSL reconstructions, they will need to consider how to interpret the fact that the RAMP model reproduces two of them but not the third, and whether they can still claim that the RAMP model *"yields consistent and good results"* (L.13) in this situation. Even more serious is the question of what implications this has for the proposed MPT mechanism

The revised manuscript focuses only on global ice volume and no longer on benthic δ^{18}O. Hence, we only use the Berends and Rohling GMSL records as targets for the RAMP model. Since the regular RAMP model is designed to simulate an increase in global ice volume (due to the increasing v_0 parameter), this model is not suited for the new Clark GMSL curve, since this reconstruction no longer shows an increase in amplitude. Instead, we tested various parameterizations and found that if the glaciation threshold of the RAMP model is slightly altered, this new model (which we call RAMP-2) is capable of reconstructing the Clark GMSL curve, especially it is capable of simulating the shift in frequency without an increase in amplitude. Hence, we set up a slightly altered model version, which we call RAMP-2 model, to simulate the new Clark GMSL curve (presented in SI Sec. 7).

Paleotime versus physical time. In my previous review, I noted that the authors use the variable t (time) in their equation in two different meanings. In one case, this is physical time, and in another, it is "paleotime", i.e. "minus-time". This is unacceptable. The authors responded *"we agree"* (p. 11 of their response) but, obviously, did not understand what I meant, as they only swapped t_1 and t_2 notations, yet still use two different times. There are two solutions for this problem: either to use a different letter for "paleotime", or to convert "paleotime" (ka) into physical time (kyr), i.e. make t_1 and t_2 negative and to rewrite their eq. (8) accordingly.

Corrected. In the revised manuscript, the variables t_1 and t_2 have been changed into physical units (kyr), s.t. they are negative in the model. The model now uses consistently physical units. Equation (8) has been accordingly modified in the methods part:

$$v_0(t) = \begin{cases} v_{0,1}, & \text{if } t < t_1 \\ v_{0,1} + \frac{v_{0,2} - v_{0,1}}{t_2 - t_1}(t - t_1), & \text{if } t_1 \le t \le t_2 \\ v_{0,2}, & \text{if } t_2 < t \end{cases} \qquad (3)$$

Holocene duration and future predictions. I had already a problem with the description of the "future" predictions in the first version of the manuscript, but the new version is even more problematic. The authors now write: *"... the RAMP model projects for all four tuning targets that* **the next glacial cycle has already started 6 - 10 ka** *..."* (L. 378/379). Do the authors really believe that we are already living in the Ice Age? Or they do not consider this discrepancy between the model and reality worth mentioning? In fact, the reason for the model's inability to simulate the long Holocene is straightforward. This is the condition for **(d)** $\rightarrow$ **(g)** transition (i.e. glacial inception), namely eq. (7). According to these two conditions, this transition occurs every time as the ice volume drops below v_1 (ca. 10 msl), result in in the regime change from deglaciation to glaciation. Since in equation (5), $\alpha_g < I(t)$ for most of the time, the ice growth begins immediately or soon after this transition. Therefore, the RAMP model does not recognize the existence of interglacial state as a stable climate state and thus cannot be applied to the problem of the natural duration

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of the Holocene (or any other interglacial), as it contradicts both physically-based Earth system models and observations. As far as the recent conceptual models of Quaternary glacial cycles are concerned (Model 3 and Telento and Ganopolski model) they do simulate a long (60 kyr) natural Holocene.

This section in the manuscript should not be considered as a realistic simulation of how the next future glacial cycle would look (obviously it can't since it's missing any CO_2), but rather as reporting a model characteristic and setting them into the scientific context as a tool of model evaluation. We propose to replace the wording *"future prediction"* with *"model extrapolation"* to not give the wrong impression to equally compare our results of extrapolating the model 100 kyr into the future with results from more complex Earth System Models like CLIMBER-2 as presented in Ganopolski et al. (2016), which presents a more appropriate tool for this task. As I pointed out in my last response to the reviewer, other conceptual models have reported their model extrapolations to the future as well, even though they report the next glacial onset in the past: Calder (1974) reported an onset 5 ka (see their Fig. 5) and Imbrie and Imbrie (1980) reported an onset 6 ka (see their Fig. 7). Even though such results are unrealistic, it is still worth reporting such a model characteristic and should not be omitted. Particularly, it is worth reporting any shortcomings of our model which could be addressed in future studies. Hence, it is important to discuss the inability of the RAMP model in predicting a prolonged Holocene. Moreover, the referee is completely right that the RAMP model does not recognize intergalcial states as stable states in our model and yields quick deglaciations followed by long-lasting glaciations. We propose to add a short discussion about this into this section, s.t. it becomes clear for the reader that the short Holocene in the RAMP model already follows by model design. Instead of speaking of *"the next glacial state"*, which implies that we would already live in glacial conditions, we should rather refer to *"crossing the next glaciation threshold in the model"*.
Hence, we propose to replace the paragraph as follows:

"While neglected in many conceptual models, extrapolating the next glacial cycle can be an important tool of model evaluation, since this represents the only time interval in which a model cannot be tuned or fitted onto some existing paleoclimatic target curve, and it can reveal model deficiencies. Since the extrapolated curves lack anthropogenic CO_2 emissions, they must be interpreted as baseline experiments of how the glacial cycles would evolve in the absence of anthropogenic impacts. The RAMP model consistently crosses the next glaciation threshold 6 ka, lasting until 64 kyr in the future. Some other conceptual models were also extrapolated into the future: Calder (1974) projected the next glacial onset 5 ka, lasting until around 119 kyr (see his Fig. 2), Imbrie and Imbrie (1980) estimated a start 6 ka (see their Fig. 7) and Figure 5 in Paillard (2015) reveals that only a large enough glaciation threshold can lead to a prolonged Holocene in the P98 model, while a lower threshold would have terminated the Holocene already a few kyr ago. While other models like the L23 models and the Leloup and Paillard (2022) model were not extrapolated for the next glacial cycle, it can be expected that they would yield similar results as the RAMP model (i.e. a short Holocene), since they are based on similar model dynamics. In their recent study, investigating the influence of precession and obliquity on glacial interglacial cycles, Barker et al. (2025) estimate that the current interglacial conditions would last for 11 kyr, when obliquity reaches its next minimum, and the succeeding glacial would be interrupted in around 66 kyr

These results do not agree with more complex models, coupled to the carbon cycle, which project, even without any anthropogenic influence, an unprecedentedly long Holocene, lasting for another 50 kyr (Ganopolski et al., 2016; Talento and Ganopolski, 2021). Hence, the short Holocene, the crossing of the next glacial threshold 6 ka and the failure in stimulating the Holocene minimum in global ice volume do not align with these more complex models. These deficiencies stem from the model design, which does not recognize interglacial states as stable states in the model and yields quick deglaciations followed by long-lasting glaciations. This becomes apparent from the model equations: In the late Pleistocene, when a large ice volume is present, the term $\frac{v(t)}{\tau_d}$ (Eq. 5) dominates the deglaciation regime and leads to very quick removal of the large ice sheets. Hence, the next glaciation threshold $v(t) \cdot \tilde{I}(t) < v_1$ (Eq. 7) is rapidly crossed in the model, leading to a short interglacial state, which is immediately followed by an increase in global ice volume (Eq. 4). Hence, by its very design, the RAMP model cannot produce extended interglacial states and will instead rapidly transition to the next glaciation. This model limitation could be addressed in a future study, either by modifying the threshold equations or by adjusting the differential equation in the deglaciation regime in such a way that it also allows for stable interglacial conditions, instead of just for rapid ice loss."

CO_2 "forcing". In the conclusion, the author wrote that it would be good to also use CO2 as an additional forcing, of course, as soon as the "old" ice is found. However, this is not a good idea since the only justification for the existence of conceptual models of glacial cycles is that they illustrate how the only external forcing (orbital) can be mathematically converted into global ice volume without the explicit use of paleoclimate data. During glacial cycles, CO2 was not an external forcing, and CO2 is very similar to global ice volume evolution, at least for the late Quaternary, and it is likely that this is also true for the entire Quaternary. What is the point of forcing a simple model with input that is essentially identical to the expected output?

Removed from the revised manuscript.

Specific comments

Title: *"long ramp-like change"* of what?

Corrected. The title has been changed to:
"Simulating global ice volume across the Mid-Pleistocene Transition with a ramp-like increase in the deglaciation threshold".

L. 13 and 113. What is meant under *"to reconstruct a paleoclimate curve over the Quaternary"*?

No longer included in the revised manuscript, since now the RAMP and RAMP-2 models are only used to reconstruct the global ice volume.

L. 14. The expression *"recent deconvolution"* does not provide sufficient information. Clark's

paper should be cited here.

We removed the Clark $\delta^{18}O_{sw}$ record from the manuscript; hence, this sentence no longer appears.

L. 115. If one selects as the insolation metric solstice insolation at 65 °N (which many modelers, including me, do), how it can *"poses a bias to the model"*?

We refer to the study by Leloup and Paillard (2022) who investigated the influence of different insolation metrics on the quality of the obtained model-data agreement:
"Here, we use a simple conceptual model to test and discuss the influence of the use of different summer insolation forcings, having different contributions from precession and obliquity, on the model results. [...] The quality of the model–data agreement however depends on the chosen type of summer insolation and time period considered."
To not decide a priori how strong the precession or obliquity signal is, we decided to follow the approach by Imbrie et al. (2011), who argue in their model design that:
"Our model uses orbital forcing which is a linear combination of ϵ (obliquity), $e \sin \omega$ (precession) and $e \cos \omega$ (phase-shifted precession). The choice of which linear combination to use is data-driven [...] In our view, this is preferable to working with a preconceived notion of which latitude and season is the most suitable for use in the forcing function."
We changed L. 114ff in the following way:

"Leloup and Paillard (2022) showed that the model outcome depends on the chosen insolation forcing, which differ in their precession and obliquity signals (e.g. summer solstice or caloric season at 65° N). Hence, selecting a specific insolation metric can pose a bias to the model, since this determines the strength of the precession and obliquity signals in the model."

L. 127. I guess under the "state" the author mean "regime", since *"deglaciation state"* sounds odd to me.

Corrected.

Ibid. What paleoclimate quantity is measured in meter sea level equivalent?

Corrected. In the revised manuscript, only global ice volume is reconstructed. Hence, L.127f. is changed to:
"The model has two different regimes, the glaciation ($\mathbf{g}$) and the deglaciation regime ($\mathbf{d}$). Depending on the current model regime, the global ice volume $v(t)$, given in meter sea level equivalent (m sl), is driven by two first-order differential equations:"

L. 139. What is [I]?

It refers to the units of I(t). However, to avoid any confusion, we removed this equation from the sentence and simplified L.139 to:
"To account for the correct units, the threshold equations use the dimensionless orbital forc-

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ing $\tilde{I}(t)$."

L. 435 *"It improves the previous L23 model ... by reducing the number of parameters"*. How a model can be improved by reducing the number of parameters is unclear, especially given that in the first version of this manuscript the authors presented an increased number of parameters as an improvement compared to L23 model. Indeed, in my first review, I noted that their model contains too many parameters for such a relatively easy task of converting orbital forcing into Quaternary ice volume variations. This is why I am glad that the authors found a way to do the job with fewer model parameters. But why it should be called "improvement"?

Accepted. We changed L.435, such that the "improvements" only refer to the listed changes in L.435f., and the reduced number of parameters is mentioned in the next line. L.435ff. reads now as:
"It improves the previous L23 models of Legrain et al. (2023) by reducing the model-data mismatch (ΔRMSE > 1 m), increasing the numerical efficiency (speedup of $\sim$30), refining the tuning strategy, extrapolating the model for the next glacial cycle, testing the model on another target curve, and implementing a more flexible ramp-like parameterization, which includes all former L23 temporal scenarios. Meanwhile, the required number of model parameters has been reduced by two to four."

L. 455. The authors wrote that the model *"only relies on precession and obliquity as input"* (obviously, they meant here "precessional parameter", otherwise eccentricity should be also mentioned), but this is incorrect – the authors also prescribe time-dependent $v_0(t)$ which is necessary to obtain the MPT.

Accepted. With the wording "input" we wanted to describe the data input which is given to the model, which is only the precession parameter and obliquity. As the referee points out, the model also requires a time-dependent change in the $v_0(t)$ parameter, in order to simulate the MPT. To avoid the misinterpretation that the RAMP model can reconstruct the MPT only with orbital forcing, we changed L.455ff. as follows:
"Currently, the RAMP model uses only the precession parameter and obliquity as inputs, along with a time-dependent v_0 parameter, to reconstruct global ice volume throughout the Quaternary."

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