

## **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 minor revisions to our manuscript in order to address the concerns of the reviewers.

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

# Response to Anonymous Referee #1

[Author comments](#)

[Referee comments](#)

Slowly evolving improvement. . .

I hope we are finally getting to a publishable version in the near future.

From the performed changes in the draft I am still not entirely happy with the way the new Clark et al. (2025) global mean sea level (GMSL) record is treated. It is now mentioned as contrasting / alternative view in the introduction, which is great. However, the reader is then completely left alone with that information. Why has it not been taken up for further analysis? The introduction misses a statement why the authors are focusing on the other two alternatives GMSL versions. The introduction should also include the information that a modified version of the RAMP model has been applied to the Clark GMSL with all details contained in the SI (which is ok) and that these results are discussed in section 4.

The following paragraph has been added to the introduction:

*"Since the precursor L23 models were designed to simulate an increase in global ice volume amplitude over the MPT, we use the GMSL reconstructions of Berends et al. (2021) and Rohling et al. (2022) as target curves, instead of the recent reconstruction by Clark et al. (2025). An adjusted model version, the RAMP-2 model, which can simultaneously reconstruct the Berends and Clark curves, is presented in the SI (SI Sec.7) and discussed in Section 4.4."*

In the SI it is said that various parametrizations were tested in order to reconstruct the Clark GMSL. Maybe this can be more specific. Did you only test alternative versions of the RAMP model or also others? From the initial draft version I remember there was the model ORB which was only using the orbital parameters and no further thresholds. I would imagine that this model might also help here. Furthermore, the modified model RAMP-2 that was fitted to the Clark GMSL is still a model that simulates a rise in glacial amplitudes over time. Does this make sense if this rise in amplitude is not seen in the Clark GMSL data (or do you see such a rise in the data somehow, then some more details on this would be needed)? Or was RAMP-2 chosen to have a model that can also successfully be applied to one of the other GMSL curves?

The initial plan was to adjust the RAMP model (hence, we only tested changes in the RAMP parameterization like different threshold equations, etc.), such that it can be applied to both GMSL curves. Therefore, the RAMP-2 model still contains the parameterization for the rising  $v_0$  parameter, although it can be seen (SI Fig. S4) that for the Clark GMSL target,  $v_0(t)$  has a relatively flat profile (as to be expected, since there is no rise in GMSL in the Clark data), while for the Berends and Rohling targets, RAMP-2 resulted in a distinct ramp shape for  $v_0(t)$ . The ORB model is entirely driven by changes in orbital parameters and has no non-linearities. Although the Clark GMSL no longer contains an increase in amplitude, it still contains a change in frequency over the MPT. Since there is no apparent change in the insolation forcing during this time, it can be expected that a purely orbitally

driven model, such as the ORB model, would fail to reconstruct the Clark GMSL. Ideally, a completely new parameterization would be used (which would no longer need a rising  $v_0$  parameter) to reconstruct the Clark GMSL. This is beyond the scope of this work and that's why we only presented the RAMP-2 model as an adjusted RAMP model in the SI, which can simultaneously reconstruct the Berends and Clark GMSL. We added the following paragraph to the SI to clarify this point:

*"The RAMP-2 model presents an adjusted version of the RAMP model. It can simultaneously reconstruct the Berends and Clark GMSL. The ramp-like increase in  $v_0$  is needed in order to simulate the increase in ice volume amplitude during the MPT, seen in the Berends GMSL. Since the Clark GMSL curve no longer exhibits an increase in amplitude, but just in frequency, an ideal model whose main goal is the reconstruction of the Clark GMSL would no longer require an increasing parameterization for the  $v_0$  parameter. Hence, a specifically designed model for the Clark GMSL would require a different parameterization than the RAMP or RAMP-2 models."*

The values of the two additional threshold model parameters  $v_1(t)$  and  $I_0(t)$  are never mentioned or showed, Figs S4c contains only  $v_0(t)$ . And if we add now  $v_{1,1}$ ,  $v_{1,2}$ ,  $I_{0,1}$  and  $I_{0,2}$ , is this not + 4 parameters ending in a total of 9+4=13 (not 12) parameters?

We added SI Table S5, which contains all tuned parameter values for the RAMP-2 model.  $I_0(t)$  is a threshold concerning the orbital forcing  $I(t)$ , not  $v(t)$ . Adding the  $I(t)$  and  $I_0(t)$  would make the plot quite overloaded. We decided not to include  $v_1(t)$  for the same reason, since it would make the plot too messy (the focus should be on the  $v(t)$  curve anyway). However, since  $v_0(t)$  was shown in previous plots in the main manuscript, we decided to still include it here, and since it also shows the rather flat profile of  $v_0(t)$  for the Clark target.  $v_1$  was already a constant parameter in the RAMP model. For the RAMP-2 model, it is changed into 2 parameters:  $v_{1,1}$  and  $v_{1,2}$ . Hence, this only adds one extra parameter here, resulting in a final number of 12 parameters (see new SI Tab. S5).

line 169 in SI: x1 in {} appears twice, but x2 in {} is missing.

Corrected.

Furthermore, the part in the discussion dealing with the Clark GMSL to a large extent copies text already given in the introduction. Please delete the repeating part in the discussion and focus only on the results.

Corrected.

The alternative view of GMSL from Clark (and what you found here with RAMP-2) should also briefly be mentioned in abstract and conclusions and what this new time series would imply for future studies.

The focus of this paper lies on the RAMP model and its implications. The RAMP-2 model does not present an optimal model design for the Clark GMSL curve, since it still contains

the  $v_0$  parameter to simultaneously reconstruct Berends and Clark. Therefore, we do not consider that it represents a result that is worth mentioning in the abstract of our study. Indeed, this does not represent one of the key results from the presented work. However, the implications of the new Clark GMSL curve and the adjustments needed for the RAMP-2 model are explicitly mentioned in the introduction and discussion (Sec. 4.4) and we have also added a more detailed explanation into the SI (see above).

Orbital parameters: As already mentioned before there are 3 orbital parameters (eccentricity, precession, obliquity) and all three appear here, but the authors repeatedly talk about 2 orbital parameters (e.g. line 136 or the title of section 4.3), probably coming from the fact, that eccentricity and precession are combined in the precession parameter.

Corrected. There seems to be some ambiguity on whether to refer to the precession parameter as an orbital parameter or not, since it already consists of two orbital parameters (eccentricity and obliquity). Legrain et al. (2023) referred to them as orbital parameters, Parrenin and Paillard (2012) referred to them as functions of orbital parameters and Laskar et al. (2004) referred to them as insolation quantities. We will stick with the term "*function of orbital parameters*", since this seems to be the most distinct naming. Changed throughout the manuscript and SI accordingly.

Technical:

- line 158: the dimensionless orbital forcing  $\tilde{I}(t)$  needs to be defined. Somehow this definition was deleted here but not replaced with an alternative.

Corrected. It was deleted due to a comment from Referee #2.  $\tilde{I}(t)$  is just  $I(t)$  without units. Now reads as: "... use the dimensionless orbital forcing  $\tilde{I}(t)$  (=  $I(t)$  without units)."

- line 168: „Time is given in physical units (kyr).“ I believe this formulation was chosen due to comments of rev#2. However, I think you might use „physical units“ in the response letter, but this is not necessary here in the text.

Corrected.

- Figure 2: x axes labels and titles are missing

Corrected.

- line 352, Title of section 4.1: Please use word instead of symbols for  $v_0$

Corrected.

- Entries in the reference list contains very often the publisher, which is not needed.

Corrected.

- SI line 150: typo in „cruves“

Corrected.

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

[Author comments](#)

[Referee comments](#)

I appreciate the author's efforts in revising the manuscript to address the recommendations of different reviewers, which were not always consistent. I believe there is a limit to how much a paper of this kind can and should be improved, and I would recommend its publication after minor revision. Therefore, I only have a few relatively minor comments:

[We thank you and appreciate your valuable feedback, which helped improve this manuscript.](#)

L.12. 'The identified orbital forcing differs from the widely used insolation at summer solstice at 65° N, as it exhibits a larger precession signal'. This sentence does not make it clear which of the two has the stronger precessional component. Please clarify that this refers to solstice insolation.

[Corrected. Now reads as: \*"The identified orbital forcing differs from the widely used insolation at summer solstice at 65° N, as the latter exhibits a larger precession signal."\*](#)

L.122: I still do not understand what the authors mean by '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'. Selecting a single metric for the real latitudinal and seasonal redistribution of insolation caused by variations in the Earth's orbital parameters is obviously subjective. One option is to choose a physically meaningful metric for orbital forcing, such as solstice insolation, and then tune the model with this forcing. Alternatively, one can tune the forcing and the model together. In the latter case, there are more 'tunable' parameters, so it is not surprising that the fit between the modelling results and the target should be better. This does not mean, however, that the 'orbital forcing' chosen in this way is less 'biased' than solstice insolation, for example. However, apart from the toy world of conceptual models, there are physically based Earth system models that allow us to analyse the relative importance of the precessional and obliquity components of orbital forcing during glacial cycles. The results of such an analysis clearly show that, although the response of ice sheets to orbital forcing is complex and non-linear, it is beyond doubt that the precessional component is much more important than the obliquity component during the late Quaternary (see, for example, Ganopolski et al., 2010).

[Corrected. We changed the wording of \*bias\* to \*subjective choice\*. Tuning the orbital weights also allows us to investigate the final orbital forcing in the model, presented in Sec. 3.2 and compare it to other metrics like the caloric summer half-year or summer solstice insolation, which would not be possible with a fixed insolation metric in the model. The line now reads as: \*"Hence, the subjective modelling choice of selecting a specific insolation metric, determines the strength of the precession and obliquity signals in the model."\*](#)

4.2 'Model extrapolation'. 'Extrapolation' is an odd term for these kinds of models, as they are not actually 'extrapolating' but simulating the future. However, the main problem is not with 'extrapolation'. The problem is that the RAMP model simulates glacial inception 6,000 years ago. Thus, this is not 'extrapolation' but a simulation of past ice sheet evolution, which we know well, unlike the future, which we do not. This is why the fact that Imbrie and Clader (with all due respect to these researchers) who also simulated a glacial inception in the recent past is irrelevant. What is relevant is that the glacial inception did not happen in reality and thus this is an obvious failure of the RAMP model.

We changed the wording *extrapolating* to *simulating*. The RAMP model simulates the crossing of the next glacial onset 6 ka. This does not imply that the model is simulating full glacial conditions 6 ka, but just the crossing of this abstract model threshold. However, the RAMP model simulates full glacial conditions and the next glacial termination around 64 kyr in the future, which contradicts more complex modelling studies which project a prolonged Holocene which lasts for another 50 kyr (Ganopolski et al., 2016; Talento and Ganopolski, 2021). This model deficiency is already explicitly stated in l. 354: "*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.*"

L.3832 and L389. The term 'caloric summer insolation' does not exist because what does 'summer' mean? If summer is JJA or even MJJA, as I demonstrated in my 2024 paper, the insolation during this period is still dominated by precession. The correct term, introduced by Milankovitch and also used by Tzedakis, is 'caloric summer half-year insolation'. Indeed, six-month 'summer' insolation is dominated by obliquity.

Corrected.

Supplementary information

The RAMP-2 model results are shown, but the model itself is not presented properly. SI is a part of the paper, and the same rules apply: if a new model is used, it should be described properly, including the numerical values of all 12 of its parameters.

Corrected. SI Table S5 added with all parameter values.

Line 169. There is a typo here: it should be  $x$ ,  $x_1$  and  $x_2$ .

Corrected.

## References

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