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 $\delta^{18}O_{sw}$.

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

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.

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) → (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 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.

CO_2 "forcing". In the conclusion, the author wrote that it would be good to also use CO_2 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, CO_2 was not an external forcing, and CO_2 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?

Specific comments

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

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

L. 14. The expression "*recent deconvolution*" does not provide sufficient information. Clark's paper should be cited here.

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*"?

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

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

L. 139. What is [I]?

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"?

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.