Answer to RC2 :

We thank the reviewer for his helpful comments and suggestions that will help us revise and improve the manuscript. We hope the answers and modifications proposed satisfactorily address his remarks.

In the following, the reviewer's comments are in black, our answer in blue and suggested corrections in green.

G. Leloup and D. Paillard

The manuscript by Leloup and Paillard tests the ability of a simple conceptual model to simulate Quaternary glacial cycles using different metrics of “orbital forcing” which differ by relative contributions of precessional and obliquity components. The authors used only one parameter (critical ice volume) to maximize model performance in term of a novel performance metric proposed by the authors. The main results of this study can be summarized as follows:

i) model performance does not strongly depend on whether “orbital forcing” is dominated by precession or precession is essentially absent.

ii) the transition from short to long glacial cycles in all cases can be achieved by an increase of the critical ice volume by factor 2-2.5.

While the latter is not surprising since 100/40=2.5, the first result requires more serious discussion (see below).

General comments

1. Since the review of Mikhail Verbitsky was already available at the time when I was requested to write my own, it is natural that I read his review before writing. And I must respectfully disagree with Mikhail in respect of the number of model parameters used in this study. Under “model parameters” (at least in our field) we understand parameters that can be used for model tuning. Gravity acceleration, Plank constant or Milankovitch‘ frequencies are not such parameters. This is why, formally, the Leloup and Paillard model (which is a simplified version of Parennin and Paillard mode) has only 5 parameters. In fact, the authors did not use four of them for model tuning since they used values for these parameters from a different model (Parennin and Paillard, 2003). Thus, the only parameter which the authors used for model tuning is the critical ice volume. Whether it is good on not is another issue.

We fully agree with the reviewer’s comment and thank him for taking the time to read previously published comments. We refer the reviewer to our answers to RC1 and RC3 for more discussion.

2. When authors discuss the current state of our understanding of Quaternary climate variability, they are too pessimistic. The authors repeat twice (in abstract and introduction) that “the nature and physics of the [link between insolation and the glacial - interglacial cycles] remain unclear”, and “the Mid Pleistocene transition ... remain mostly unexplained”. Such statements would be, probably, appropriate in 1998 but not in 2021. Of course, some questions remain and, likely, will remain for some time but the major issues are already clear.

Indeed, we do understand now much more than 20 years ago. We suggest to rephrase the introduction in the following way (l.21) : “[...] suggesting a strong link between insolation and the glacial - interglacial cycles. The nature and physics of this link has been a central question since the discovery of previous warm and cold periods [...]”

However, discussions remain open on modeling choices of the link between insolation and ice sheet evolution. This is discussed later on (answer to the specific comment relative to l.26).

3. Of course, it is up to the authors to decide which model to employ, which parameters use to tune the model and which criteria use to select the optimal parameters set. However, as the result of authors' choice, the model performance for all four “orbital forcings” for post-MPT glacial cycles are essentially the same. The authors claim that “we are able to represent the Mid Pleistocene Transition and the switch from a 41 kyr dominated record to a 100 kyr dominated record, by raising the deglaciation threshold (L. 291).” However, Fig. 4 clearly shows that this is not the case, since for three of four “orbital forcings” obliquity continues to dominate after MPT. Only for the solstice insolation, this is not the case, but then the model instead of sharp 100 kyr cyclicity simulates something which looks more like a red noise. Thus, as far as spectral properties of simulated glacial cycles are concerned, none of the model realisations is really successful. Whether this is a result of model formulation, fixing of four of five model parameters, or criteria for optimization - is not clear to me but has to be discussed in the paper.

We agree with the reviewer that the model data mismatch is not sufficiently discussed in the original version of the manuscript. We propose a rewriting of the corresponding discussion part (replacing l. 231 to 248) with first, a discussion of the timing based on our “accuracy criteria”(are the terminations at the right place?) and second, a discussion of its spectral properties. Indeed, the timing of terminations and the spectral properties are two rather different questions, though they are of course related. In the original manuscript, we focussed mostly on the first question by defining our “accuracy criteria”, but the second one is indeed unavoidable.

“Our conceptual model is able to reproduce qualitatively well the data (LR 04 normalized curve) over the whole Quaternary. The model's best fit over the Quaternary for each insolation forcing, as defined in Section 2.3*, is displayed in Fig. 4. It is able to reproduce the frequency shift from a dominant 41 kyr period before 1 Ma BP to longer cycles afterwards, as observed in the data, and thus by varying only one parameter during the whole simulation length : the deglaciation threshold V_0.

For every input forcing, longer cycles are obtained on the last part of the Quaternary (last Myr). Figure 4 displays the results over the whole Quaternary with the V_0 threshold being set to its optimal value on each 500 kyr period, while Figure 5 displays the results over the last million year with the V_0 threshold being set to its optimal value on the [0 - 1000] kyr BP period.

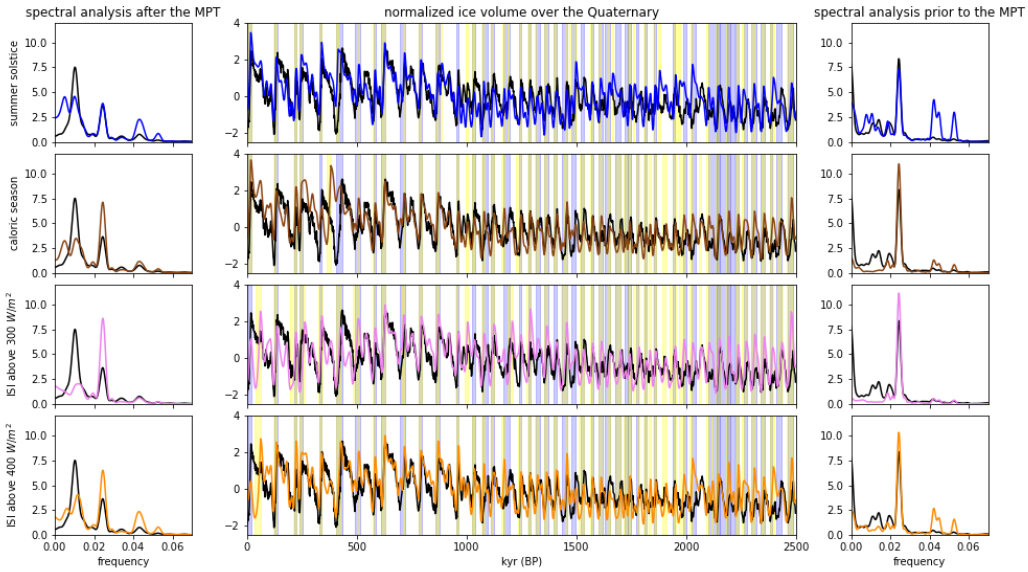
For the last million year, it is possible to reproduce with the right timing all terminations, apart from the last deglaciation, for all insolation forcings, with a single value for the V_0 threshold over this period. Some differences are however noticeable between the different forcings. Especially for the ISI above 300 W/m^2 forcing, the agreement is not as good as for the other forcings : Termination V (around 420 kyr BP) is triggered later compared to the data, while Termination III (around 240 kyr BP) is triggered too early. For the ISI above 300 W/m^2 forcing, the range of V_0 values allowing to reproduce correctly most of the terminations on the last million year is reduced (only values of V_0 = 3.9 - 4.0), whereas the results are more robust for the three other insolations forcings, with a broader range of working V_0 values. The ISI above 300 W/m^2 forcing has a low precession component, which explains why it is less successful in reproducing the data over the last million year. Experiments with our model setup have shown that a summer forcing with no precession component could not successfully reproduce the data over the post MPT period as accurately as the four forcings presented here, that contain both precession and obliquity**.

Despite the accurate timing of terminations, the spectral analysis of the model results over the last million year differs from the spectral analysis of the data. For all forcings except the summer solstice insolation, obliquity continues to dominate after the MPT. The spectral analysis shows secondary and third peaks of lower frequency, but does not show a sharp 100 kyr cyclicity as in the LR04 record. Compared to the data, all the model outputs over the post MPT period have a more pronounced obliquity and precession component and a less pronounced 100 kyr component. This feature is most probably due to the model formulation, and more specifically the direct dependence of ice volume evolution to insolation via the $dV/dt = - I / \tau_i$ term. This is one of the limits of our conceptual model. While the criteria on the switch to deglaciations allows us to reproduce the deglaciations at the right timing, the direct dependance of ice volume change to the insolation forcing is definitely too simplistic and probably produces an overestimated dependency of the ice evolution to the astronomical forcing on the latter part of the record.

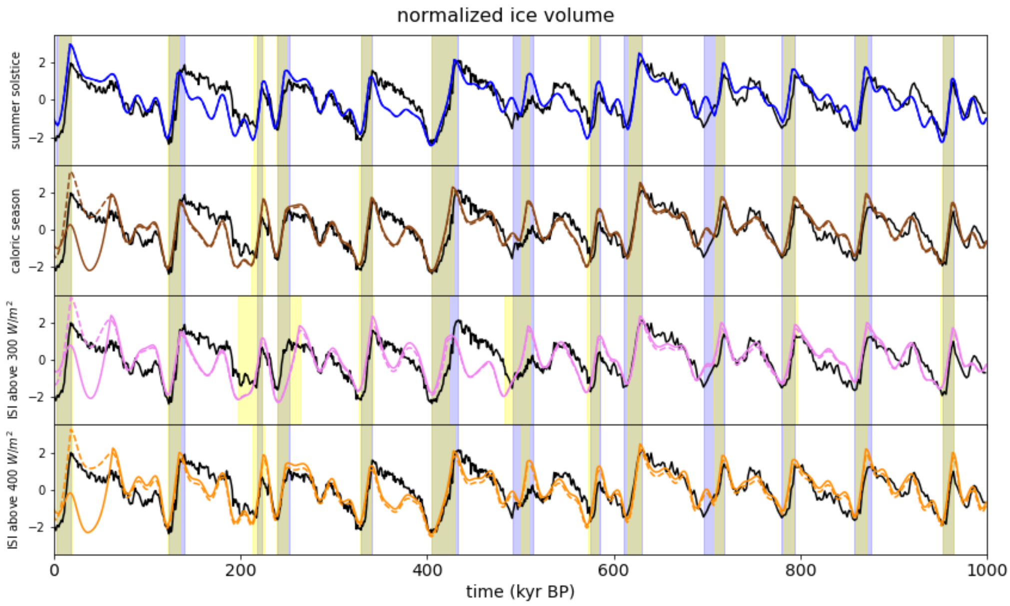
On the first part of the Quaternary (2.6 Ma BP to 1 Ma BP), the spectral analysis of the data is dominated by a 41 kyr (obliquity) peak. It is also the case for the model results, for each type of insolation. However, the model outputs also show a precession component (19 to 23 kyr), especially for the summer solstice and the Integrated Summer Insolation above 400 W/m^2 forcings, which does not exist on the data.

** The model best fit was not clearly defined in the first version of the manuscript, but will be in Section 2.3 in the next version. We refer to the answer to the reviewer's minor comment concerning this point, or to question 6 of RC4 for modification suggestions.*

*** Here we will refer the reader to the answer of question 3 of RC4*



[Figure 4 of the manuscript, with normalization changed for the spectral analysis (it was normalized by the maximum value in the manuscript, and is now normalized by the standard deviation)]



[Figure 5 of the manuscript]

4. As I already mentioned above, if the authors are convinced that model results are equally realistic irrespectively of whether "orbital forcing" contains a strong precessional component (solstice insolation) or almost none (ISI), then they should conclude that precession plays no role in Quaternary glacial cycles and, thus, 100 kyr cyclicity has nothing to do with eccentricity. Do the authors agree with such a statement? Please comment.

We thank the reviewer for this question, that will allow us to clarify the manuscript. We certainly do think that precession has a key role in the 100 kyr cycles.

Even if the model fit over the last million year for the ISI above 300 W/m^2 forcing has an accuracy comparable to the other forcings, its agreement with the data is poorer. We hope that the reformulation of the discussion section proposed above clarified this fact.

To answer question 3 of RC 4, we have furthermore performed additional simulations. In these simulations, we compared the model results in a case of a "pure obliquity forcing" : we have computed the summer solstice insolation at 65° N, but with a fixed value for the precession parameter, resulting in a signal that has no precession component.

These results show that it is not possible to reproduce accurately the terminations on the last million year with a fixed V_0 with this input forcing having no precession component.

Even if its precession component is low, the ISI above 300 W/m^2 forcing allows to better represent the post MPT part of the record compared to a forcing without precession, not only in terms of spectral content but especially in terms of terminations timing.

5. I do not understand what is shown in fig 1. Obviously, the figure heading (Normalized summer solstice insolation) is not applicable to the entire figure. More important is that the upper panel does NOT show summer solstice insolation. What it shows - I do not know.

Indeed, the figure heading will be changed in the next version. Also, the top panel mistakenly shows the summer solstice insolation between 2 and 3 Myr BP, instead of between 0 and 1 Myr BP. There is the same problem with the caloric season on the second panel. This will be corrected in the next version.

Here is the new version of Figure 1. The normalization of the spectral analysis has also been changed (normalization by the standard deviation, and not by the maximum value as previously).

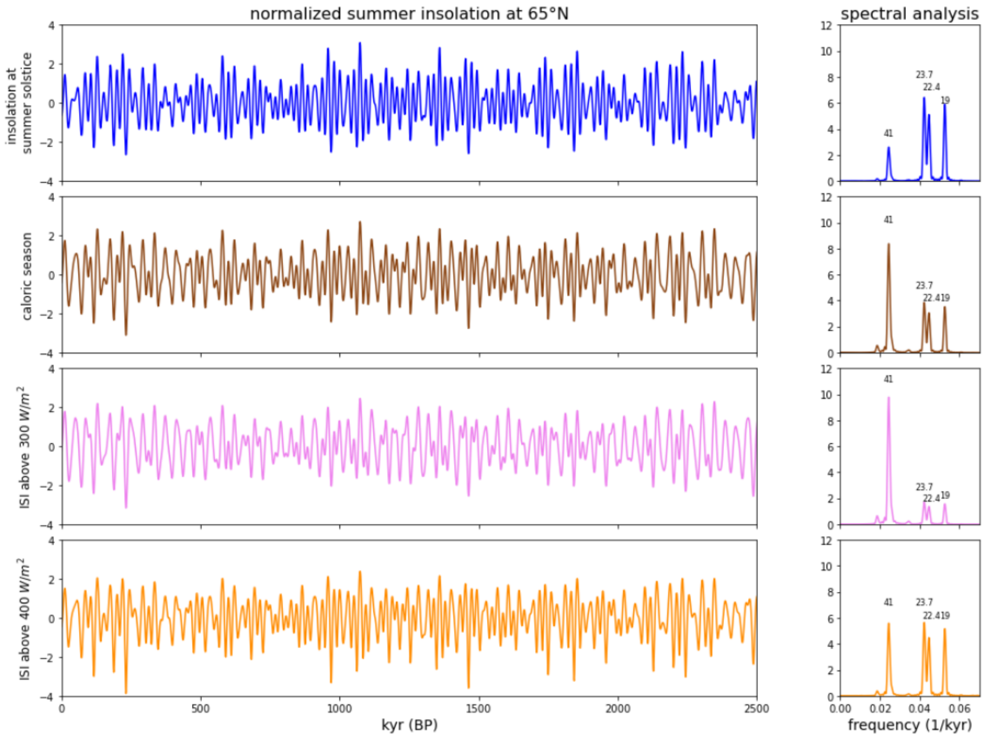


Figure : [New version of Figure 1 of the manuscript] (a) The four different summer insolation forcings at 65° N (summer solstice, caloric season, Integrated Summer Insolation above 300 W/m^2 and above 400 W/m^2) displayed over the last million year (respectively in blue, brown, pink and orange). (b) Corresponding spectral analysis.

Specific comments

L. 24. Milankovitch not just “popularized” this idea (which was not his own idea) but made it the key element of his ice age theory.

We propose the following reformulation based on the reviewer’s suggestion.

“In contrast, the idea that the decisive element for glaciation was the presence of cold summers, due to reduced summer insolation, at latitudes typical of Northern Hemisphere ice sheets, 65° N, was taken up by Milankovitch. He made it the key element of his ice age theory (Berger, 2021)”

L. 26. “This also raises the question of what period should be defined as summer”. It should be made clear that the question of how to define “summer insolation” is relevant only for conceptual models, like one used in this study. Climate models and ESMs compute insolation at each time step for each grid-cell and do not need such prescriptions.

Indeed, the question of the definition of summer insolation is only relevant for conceptual models. However, we would like to stress the fact that even for ESMs and climate models, the question of the link between insolation and ice sheet changes is still an open modeling question. We therefore suggest to reformulate and to add additional sentences, starting l.26.

“In contrast, the idea that the decisive element for glaciation was the presence of cold summers, due to reduced summer insolation, at latitudes typical of Northern Hemisphere ice sheets, 65° N, was taken up by Milankovitch. He made it the key element of his ice age theory (Berger, 2021). For conceptual models, this raises the question of which insolation to use as input. When summer insolation is used, this questions the definition of summer : should it be defined as a specific single day, like the summer solstice; the astronomical summer between the two equinoxes; or a fixed number of days around the solstice. This choice leads to very different forcings with different contributions from obliquity and precession. For ESMs and climate models, insolation is computed at each timestep for each grid area, and such choice of the input forcing is not needed. However, other modeling choices have to be made. For instance, several parameterizations are used to represent ice sheet surface melt (Robinson et al. 2010), like the Positive Degree Day (PDD) method (Reeh 1991), in which surface melt depends solely on air temperature, or the Insolation Temperature Melt (ITM) method (van den Berg et al., 2008), which takes into account the effect of both temperature and insolation. In both cases, the translation of insolation local and seasonal variations into ice sheet changes and ice age cycles remains an open modeling question.”

And to clarify l.48 that the choice of the input insolation is critical for not all, but conceptual models.

This gives (l.48) : “One of the critical questions for conceptual models is to decide which insolation to use as input.”

L. 28. This choice leads to very different forcings.

This will be corrected in the next version of the manuscript.

L. 41. The authors should make it very clear that they only consider here conceptual models of glacial cycles.

Indeed, this will be made clearer in the next version. We suggest to rephrase (l.41) by : "Several conceptual models have been developed to try to solve these questions."

L. 81, 93 and 94. I fully agree with the comments by Mikhail Verbitsky

This will be corrected in the next version of the manuscript. Please refer to the answer to RC1 for corresponding modifications.

Eq. 2. Please change V to v.

This will be corrected in the next version of the manuscript.

Last par, p. 7. When discussing pre-MPT model performance, it is important to realise that for this period of time, LR04 stack was tuned to obliquity. This is why it is not surprising that it contains nothing apart from obliquity

Indeed. Furthermore the LR04 record is not a direct representation of ice volume changes. We propose to enhance the different limits to the use of the LR 04 stack as a proxy for ice volume changes, starting l. 208 :

"Moreover, the $\delta^{18}O$ LR04 curve includes at the same time an ice volume and deep water temperature component. Ice volume and sea level reconstructions do exist (Bintanja, 2005; Spratt and Lisiecki, 2016), but are however limited to the more recent part of the Quaternary and do not allow the investigation of the pre MPT period. The use of $\delta^{18}O$ as an ice volume proxy has already been largely debated (Schackleton, 1967; Chappell and Schackleton, 1986; Schackleton and Opdyke, 1973; Clark et al., 2006) and recent studies (Elderfield et al., 2012) have shown that the temperature component may be as large as 50%. Furthemore, the stack was tuned to insolation (Lisiecki and Raymo, 2005). We refer the reader to (Raymo et al 2018) for a review of possible biases in the interpretation of the LR 04 benthic $\delta^{18}O$ stack as an ice volume and sea level reconstruction. All these reasons encourage us to remain at a qualitative level to fit the data."

L. 231. Which "data"? What "best guess" means?

In Section 2.3, l. 146 we introduce the data used, and propose the following reformulation :

"To compare our model results to data, we used the benthic $\delta^{18}O$ stack "LR04" (Lisiecki and Raymo, 2005) as a proxy for ice volume, considering that most of the $\delta^{18}O$ changes of benthic foraminifera represent changes in continental ice (Schackleton, 1967; Schackleton and Opdyke, 1973)".

We suggest to add an additional sentence, after l.145 : "Lower $\delta^{18}O$ values correspond to lower ice volume. The model results as well as the LR04 curve were normalized to facilitate their comparison. In the following "data" refers to the $\delta^{18}O$ stack curve LR04 normalized."

The use of the LR04 stack curve as data is only stated once in the text of manuscript, and we acknowledge that we should refer to it more often to improve clarity.

We suggest to add an explicit reference to it at the start of the Section 3.2 (l. 231) : "Our conceptual model is able to reproduce qualitatively well the data (normalized LR04 curve) over the whole Quaternary".

We agree with the reviewer that the model "best fit over the Quaternary"/ "best guess" was not clearly defined in the manuscript. This issue was also raised by RC4.

We suggest rephrase the end of section 2.3 (starting l.170) in coherence with the modifications suggested to RC4. In section 3.2, we will refer the reader to the definition given in section 2.3. The reformulation reads :

“The V_0 values that maximize the accuracy criteria for each time period and insolation forcing are called ‘optimal V_0’. To determine the optimal V_0 threshold corresponding to each period and insolation forcing, several simulations were carried out and the parameter values maximizing the accuracy criteria *c* were selected. More precisely, for each insolation and period, 3500 simulations corresponding to different V_0 thresholds (from V_0=1.0 to V_0=8.0 with a step of 0.1) and different initial conditions (initial volume V_{init} ranging from 0.0 to 5.0 with a step of 0.2, and initial state - glaciation or deglaciation) were performed. For each insolation forcing, the best fit over the Quaternary is defined as the simulation over the whole Quaternary (0- 2500 kyr BP) with a V_0 changing with time, and that is equal to the corresponding optimal V_0 at each time period.”

L. 300. Talento and Ganopolski is now published

This will be corrected in the next version of the manuscript.

References :

Reeh, N. (1991) : Parameterization of Melt Rate and Surface Temperature in the Greenland Ice Sheet, Polarforschung, Bremerhaven, Alfred Wegener Institute for Polar and Marine Research & German Society of Polar Research, 59(3), pp. 113-128.

Robinson, A., et al (2010) : An efficient regional energy-moisture balance model for simulation of the Greenland Ice Sheet response to climate change, The Cryosphere, 4, 129–144, https://doi.org/10.5194/tc-4-129-2010, 2010.

van den Berg, J., et al (2008) : A mass balance model for the Eurasian Ice Sheet for the last 120,000 years, Global Planet. Change, 61, 194–208, https://doi.org/10.1016/j.gloplacha.2007.08.015, 2008.